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# Part First.

## ORIGINAL COMMUNICATIONS.

### Article I.—*Case of Amputation at the Hip-Joint.* By James Spence, F.R.C.S.E., President of the Medico-Chirurgical Society, Senior Ordinary Surgeon to the Royal Infirmary of Edinburgh, and Lecturer on Clinical Surgery.

L. S., æt. 15 years, was admitted into the Royal Infirmary on the 13th of July 1862, on account of a large tumour occupying the middle and lower part of the right thigh. The previous history of the case is contained in the subjoined extract of a letter from Dr Meikle:—"I attended Miss L. S. and two of her sisters, in September 1861, during a mild attack of fever. She rapidly recovered, and did not again come under my observation till April last, when I was requested to examine her right thigh. The lower third of the femur was considerably enlarged, and the soft parts surrounding it were firm, and closely matted together. The circumference of the affected portion of bone seemed bulged out in all directions, and enlarged veins were seen ramifying on the surface of the tumour. The condyles of the femur were not involved in the disease, and the portion of the shaft above the tumour was of normal size. I was told that Miss S. had felt more or less uneasiness in the part since the date of the febrile attack, but she could trace it to no cause, having sustained no accident. The pain at this time was very moderate, being more, indeed, a sense of uneasiness than anything else, but it was increased during the night. Measurements were taken from time to time, which showed that the tumour at first was increasing in size, but not quickly, until July last, when, after exposure to cold in an open conveyance, she was seized with pleuritis, which reduced her strength very much. From this date the tumour made rapid progress, and it became evident that nothing but amputation would save the girl, for which purpose she was put under your charge. When I first saw her, her face had a somewhat dingy appearance, and it gradually became darker, until it assumed a leaden hue."

*Symptoms on admission.*—Patient is pale, emaciated, and presents a very cachectic appearance; pulse rapid and irritable; chest and urine found normal on examination; no glandular enlargement; tongue slightly furred. The whole circumference of the middle and lower part of the right thigh is occupied by a large tumour, over which the veins are greatly distended. The surface of the tumour is irregular, and there is at points an indistinct feeling of fluctuation. There is no œdema of the leg, nor glandular enlargement in the groin.

15*th July.*—To-day Mr Spence performed amputation at the hip-joint in the following manner:—The patient having been put under chloroform, Lister's tourniquet was applied to the abdominal aorta. The breech was brought well over the edge of the table, and the pelvis held steady by an assistant. The tourniquet was entrusted to Dr Gillespie, while Dr Watson compressed the femoral artery at the brim of the pelvis. Mr Spence having abducted, and slightly flexed the limb on the pelvis, gave it in charge to Dr Struthers. Then grasping the soft parts with his left hand, Mr Spence entered the point of the knife in front of the tuberosity of the ischium, and passing it obliquely upwards, in front of the articulation, brought out the blade almost midway between the anterior superior spine of the ilium and trochanter major, so as to form a large anterior flap, disarticulated the limb, and terminated by cutting a posterior flap nearly equal in size to the anterior. A large sponge was immediately applied to the posterior flap, until the large vessels in the anterior flap were secured. No artery jetted so long as the tourniquet was retained. After the arteries had been tied, and the tourniquet removed, the femoral vein continuing to bleed, a ligature was placed upon it. The flaps were then stitched together, and a pad placed on the anterior and posterior surfaces to keep them in apposition. The patient having been removed to bed, a beef-tea and brandy enema was immediately administered, and after she had rallied an opiate was given.

16*th July.*—Slept well; pulse, 130; no reactionary hæmorrhage since the operation.

17*th.*—Pulse, 128; patient progressing favourably.

18*th.*—Pulse, 120.

19*th.*—Pulse, 116; some of the stitches removed, and straps of plaster applied; has been taking beef-tea, wine, milk, and a little brandy for diet.

20*th.*—Pulse, 106; stump looking well; no dressing on the wound except a narrow strip of lint along the line of incision, and a pillow to support the hip.

21*st.*—Pulse, 116; wound dressed with chloride-of-soda lotion.

22*d.*—Pulse, 110; some more stitches removed to-day.

23*d.*—Pulse, 106; the remaining stitches were removed, and straps applied.

24*th.*—Pulse, 98; patient is taking tincture of the muriate of iron, nourishing diet, and wine.

25*th.*—Pulse, 102.

26*th.*—Pulse, 100; two ligatures came away to-day.

28*th.*—Stump looking well; appetite good; general appearance of patient very much improved.

*29th.*—Pulse, 100; tongue cleaning.

*30th.*—Pulse, 100.

*31st.*—Pulse, 96.

*1st August.*—Pulse, 90.

*2d.*—Pulse, 86.

*3d.*—Pulse, 90.

*12th.*—Pulse, 86; femoral ligature came away to-day; stump almost entirely healed up.

*13th September.*—Since last report, recovery has gone on in a most favourable manner. Not an occurrence worthy of notice has interrupted the convalescence, and to-day she returned to the country in a state of health bordering on that of former years. During the last fortnight she has been walking about the ward, and has been out several times for a drive.

*Remarks.*—The mortality which attends amputation at the hip-joint, and which has even led some to question the justifiableness of the operation, is undoubtedly very great; and hence considerable interest attaches to every successful case, the circumstances under which it was performed, and all the conditions which may have a bearing upon the general question of the operation in similar cases. According to the latest statistics of the operation, given in the recent edition of Cooper's Surgical Dictionary, we find that, in a total of 139 cases, there were 91 fatal results, or a mortality of 65·46. Moreover, when we know that in several of the successful cases the operation was performed on limbs which had previously undergone partial amputation, and where consequently the shock and disturbance of the system caused by the sudden removal of the whole lower extremity was wanting, or at least greatly diminished, it seems evident that the risks of this formidable operation are such as must limit its performance to cases of extreme necessity.

In cases of certain severe injuries the surgeon may be forced to perform it as the only chance of saving, or at least prolonging life, and alleviating suffering, by removing the shattered limb. In cases of disease, the question of operation comes before us under a somewhat different aspect, for the amputation may prove more speedily fatal than the disease would have done if left to its own course. Hence, to warrant its performance, the disease must be of a kind that admits no other remedy, and at a stage when the whole of the appreciable disease can be removed, and with a reasonable hope of prolonging life. In the case which forms the subject of the present remarks we have a good example of the conditions which warrant us in having recourse to amputation at the hip-joint. The girl was suffering from intense pain, her appetite was almost gone, and her strength exhausted by the pain and want of sleep; her pulse was quick and irritable, whilst the tumour was increasing rapidly. The urgency of these symptoms showed the necessity of operative interference, if any attempt was to be made to relieve her sufferings and to prolong life, even for a short time; whilst the absence of glandular enlargement, either in the groin or elsewhere, gave hopes that the lymphatic system was as yet unaffected, and that, with youth on her side, there was a reasonable hope of success attending the operation. On the other hand, if it were delayed, or not performed, a speedy and painful death was certain.

The disease was limited as far as malignant disease can be said to be limited in the living body, where the circulation of the fluids prevents positive limitation to any special part. But, practically, surgeons are agreed, from experience, that malignant disease, especially its medullary form, when situated in an anatomically limited organ,—as the testicle, for example, where it is invested by a dense resisting capsule,—admits of more thorough removal than when it occurs amongst muscles or other soft textures, whose structure it speedily involves in the same diseased action, without any definition or limitation. In the present case the disease had evidently commenced in the interior of, and was still apparently confined to, the shaft of the femur. Thus walled in, as it were, by the dense fibrous shell of the bone, or by the strong fibrous periosteum investing the shaft on every side, and closed in at either extremity of the bone by the articular cartilage of incrustation, the disease presented the utmost possible degree of anatomical limitation, and so presented the best possible chance of complete removal. But to profit fully by this limitation of the disease it was evidently necessary to amputate in the contiguity, and not through the continuity of the affected bone,—a rule which I consider of paramount importance in such cases. Hence the reason why I considered amputation through the trochanters inadmissible, although the bone seemed unaffected; for, in many cases where no perceptible alteration in the form of the bone can be detected, the disease will be found to be infiltrated into the cancellated texture, even of the neck and head of the bone; and there need be the less hesitation on this point, as I believe that, in cases of malignant tumours of the femur, amputation through the trochanters is attended with quite as much danger to life as amputation at the hip-joint.

In regard to the operation itself, in this particular case, I would only notice two points, and that very briefly. First, The mode adopted to restrain hæmorrhage during the operation. This was effected by compressing the abdominal aorta by the tourniquet, or compressor, proposed by Professor Lister, of Glasgow, and which he kindly lent me for the occasion. I had previously ascertained, that, from the thin condition of the patient, I could readily arrest the circulation by manual pressure on the lower part of the aorta, but an equal, unyielding mechanical compression is safer, and hence I preferred it; but, as it was a trial, I guarded against all risk from the anterior vessels by entrusting the care of the femoral artery to my colleague, Dr Watson. The result, however, showed the efficiency of the instrument, as it completely commanded the usually incontrollable flow of blood from the branches of the internal iliac distributed to the posterior flap; and avoidance of loss of blood in an exhausted patient must have a most important bearing on the success of the case. How far such compression could be trusted to entirely in a strong man, or in the case of a very fat person, with a large abdomen, or whether it might, if incautiously applied, lead to injury to the viscera in certain conditions, further experience can only reveal; but certainly in this, and two cases in which it was used by Professor Syme, it proved perfectly effectual.

The other point of the operation I would refer to, was the necessity which arose for applying a ligature to the femoral vein. The hæmorrhage from this vein continued after all pressure had been removed from the groin and abdomen, and as temporary pressure of its orifice and other means failed to arrest it, and as it returned whenever the pressure was removed, I found it absolutely necessary to tie it. I have frequently seen troublesome bleeding from the femoral vein in amputation of the thigh, but in general it stops when compression is made by applying the flap against its surface, when all pressure above is removed, and the stump elevated, but here these means proved ineffectual, or perhaps, I should rather say, the weak condition of the patient rendered it necessary at once to check all loss of blood. I confess that my prejudices against tying veins are very strong, and this step in the operation made me uneasy as to its effects; but as the history of the progress of the case shews, my fears were groundless, not a single bad symptom having followed ligature of the vein.

In conclusion, the uninterrupted progress of the patient's recovery was most satisfactory. It is not often that even a simple amputation of the thigh goes on so well; and whatever may be the estimate of the operation in general, or whatever may be the chances of the disease returning in some other organ hereafter, it has for the present at least, snatched the patient from the brink of the grave and restored her to her family, and to the enjoyment of perfect health.

## ARTICLE II.—*On some Results of Imperfect Deliverance in Abortion or Labour, especially Fetid Uterine Discharges: Experience in the Royal Infirmary of Edinburgh.* By J. MATTHEWS DUNCAN, M.D., Librarian of the Royal College of Physicians, Clinical Lecturer on the Diseases of Women.

EVERY obstetrician is well aware that the discharges from the genital passages of women are excessively fetid in a number of different circumstances. All discharges from these passages have a disagreeable, strong animal odour. To this I do not now refer, but to a putrid smell or fetor. There are varieties in the character and diffusibility of such fetid smells; that, for example, of cancer of the cervix differing from most of those resulting from mere putridity.

But such differences, however interesting to the practical physician, cannot be well described in words, and can only be learned by frequent experience.

Among the causes of fetor in the vaginal discharges of women may be mentioned the retention in utero or in vagina of menstrual fluid, an occurrence not unfrequent in some women; the retention of lochia; the retention of leucorrhœal or purulent discharges; the retention in utero or in vagina of the ovum at different degrees of development, or of parts of it; the retention of decidual membrane; cancer of the vagina or uterus; extra-uterine pregnancy; disintegrating fibrous tumour; abscess of the genital organs, or in the pelvis; and thrombus or hæmatocele in the same parts. These causes do not invariably, or under all circumstances, produce fetor, but they all frequently do so; and, it may be added that tents and pessaries often rapidly induce fetor of the discharges, as well as anything introduced from without that will decompose or lead to retention of matters that readily undergo this change.

Extraordinary cases of retention of the entire ovum, at or near the full time, and cases of retention of the placenta, or of portions of it, are well known. The latter class of cases has been recently made the subject of a monograph by M. Hegar. Those to which I now attract attention are more recondite, and less familiar to the profession. It is well-known to obstetricians that part of an ovum may be retained and cause putrid discharges, which are sometimes accompanied with danger to life, and which generally disappear in a shorter or a longer time; but the following cases have points of interest which distinguish them from ordinary accidents, and give them interest and importance.

Writing on the subject of abortion, in his work on Midwifery, Dr Burns (tenth edition, p. 334) says—" While part of the ovum is left, or the whole of the secundines are retained for a considerable time, we have another danger besides hæmorrhage; for, within a few days putrefaction comes on, and much irritation is given to the system, until the fetid substance is expelled. Sometimes, if gestation have not been far advanced, or the piece which is left be not very large, it continues to come away in small bits for many months, during the whole of which time the woman is languid, hysterical, and subject to irregular sanguineous discharges, or often to obstruction. But more frequently the symptoms are very acute; we have loss of appetite; prostration of strength; tumid or tender belly; frequent, small, and sharp pulse; hot and parched state of the skin of the hands and feet; nocturnal sweats, and various hysterical symptoms. The discharge from the vagina is abominably fetid, and hæmorrhage sometimes occurs to a violent degree."

The cases to be recorded illustrate consequences of imperfect abortion to which Dr Burns does not make any reference. They show that part of an early ovum may be retained without any urgent symptoms for several months. Like Dr Burns, Dr Ramsbotham (Obstetric Medicine and Surgery, third edition, p. 666) believes that retained parts of placenta begin to decompose and be putrid very early, and at farthest within a period of some days; Dr Ramsbotham, indeed, considers it worthy of remark that he has "known a number of instances in which the placenta was not thrown off for many days after the young embryon had passed, and yet it did not possess in any degree a putrid odour." The following cases show that months may elapse before the sign of putridity is observed. This lapse of time between the incomplete abortion and the supervention of serious symptoms especially of putrid discharge, together with the general teaching on this subject, illustrated from the justly popular works of Burns and Ramsbotham, sufficiently account for the circumstance that the most interesting of the cases now recorded were inaccurately diagnosed by the physicians under whose care they at first happened to be.

In the title to this short paper I have written "Fetid Uterine Discharges." But the absence of fetor in some examples of imperfectly completed abortion is very remarkable, and almost unaccountable; and this absence of fetor renders such cases less likely to be correctly diagnosed and treated, and makes them, in this respect, of much importance. The presence of fetor, indeed, ought soon to attract attention, and lead to careful investigation of the case in which it occurs; but, in its absence, there may be no special symptom to lead the practitioner to suspect that the case under his care is one having any character to distinguish it from more ordinary uterine ailments. The following case of retention of an ovum for seven months is a remarkable instance of the absence of fetor, or, at least, of its presence to so slight an extent, in a scrupulously cleanly lady, as never to be noticed by herself or by her husband, himself a physician. I would, indeed, have it understood that by absence of fetor I mean its absolute absence, or its being such as not to attract the attention of careful, cleanly individuals.

Case I.—Mrs —— was delivered of her second child fourteen months before consulting me. When the child was about seven months old she believed herself to be again pregnant, and therefore gave up suckling. But she soon resigned the notion of her being pregnant, all symptoms of that condition gradually disappearing. After this time her uterine health became impaired. She suffered from weight in the uterine region, slight bearing down, occasional irritability of bladder, irregular action of the bowels, occasional disorder of the stomach, and even sickness. She soon began to have a constant leucorrhœa of a brownish colour, and bright bloody discharges were never absent above a week, the loss in this way being occasionally somewhat profuse. Having these symptoms, she made the long journey from India to Edinburgh. Soon after her arrival here a severe metrorrhagia occurred, and I was called to see her. The uterus was found enlarged, its cavity being also larger than natural. The cervix was healthy, but preternaturally open, the end of the finger being easily passed just within the circle of the os externum. The case was diagnosed as one of retention of some of the remains of an ovum which had produced for a time the symptoms of pregnancy, had ceased to live, but had never been entirely expelled. A sponge tent was inserted into the cervix. On the day following its use both the tent and the blanched ovum were found in the vagina. They were very fetid, as was to be expected from the use of the tent. The ovum was a placental mass, an inch and a-half broad and above half an inch thick. On opening the bag of membranes a few drops of dirty brownish fluid escaped; no remains of an embryo or cord were discoverable, and the ovum perhaps never contained any, being addle from the beginning. The fœtal surface of the placental mass presented prominences distended with bloody fluid, such as are often seen in similar circumstances. The patient soon recovered, and after aborting twice, bore a healthy child at the full time.

CASE II.—A case somewhat smiliar to the preceding occurred to me several years ago. The patient was a soldier's wife who had miscarried at about the end of the third month of pregnancy. She was attended by a woman who told her that all had come away right. But from that time till I saw her, ten weeks after the abortion, she never ceased to have bloody discharge. At this time the placenta came away. It was not fetid, but had undergone peculiar changes which I do not here describe: only it was œdematous, and its fœtal surface was covered by numerous distended bladders of chorion of the size of half of a boy's marble, and filled with bloody fluid.

The case which I have next to record differs from the two former in the circumstance of the presence of a very profuse and intensely fetid brownish discharge evidently containing pus, and in the discovery of decomposing structures. Besides, it was erroneously diagnosed and treated as a case of ulceration of the uterus. The speculum and caustic had been diligently used, this treatment being the resort, in almost all uterine cases, of many ignorant, and not a few careless practitioners. On this I say no more at present, because it would be out of place to enter now on the many gross abuses of a plan of treatment sometimes found very useful.

It should be remarked that in Cases III. and V. fetor did not commence till after the disease had lasted for a long time. I am unable to give a quite satisfactory reason for the delay of the appearance of the putrid odour; but the long delay in two of the following cases brings the two preceding cases into the same category with them, it being almost certain that in them also, had the retained structures not been completely discharged, intense fetor would soon have made its appearance. The following appears the most probable method of accounting for the delayed putrescence (as in Case V.): The ovum dying, or by some morbid process becoming disconnected from the healthy decidua, may pass away entire and without decidual structures, or break down and be discharged; or it may be accompanied by only a portion of the decidua. In such cases the remaining decidua may be supposed to retain its uterine connexions for an indefinite time, say weeks or months, and then being thrown off, may be retained in utero, and at last become putrid. When the cause of abortion is disease of the decidua, especially if there be hæmorrhage beneath it, or when uterine contraction or spasm is strong, then persistent adhesion and retention will be evidently less likely to occur than when the decidua is healthy and uterine contractions are inconsiderable. This mode of explaining the delay of putrescence is scarcely applicable to instances like Case I.; for in such it is not decidual but ovuline, and especially placental structures, that are not putrid. And in such cases, while a prolonged vegetative life can scarcely be imagined to continue without growth or hydatigenous or other degeneration, yet it is easy to suggest that a slow decomposition without putridity may explain the phenomenon.

Case III.—Mrs B., æt. 25, was mother of four children, the youngest of whom was born nine months before her admission into the Infirmary. After its birth she was for three days much troubled with after-pains, but made, upon the whole, a good recovery. A month after her delivery she began to have severe pain in the lower part of the belly, and in the back and thighs, with bearing down, and at the same time a leucorrhœal discharge made its appearance. She was so ill as to require to be constantly in bed or on a sofa. Four months after delivery she menstruated, and this change thereafter returned nearly regularly. At these times the loss of blood was so severe that her physician plugged the vagina each time, and the discharge lasted long—on one occasion for nearly three weeks. Two months before admission the discharge became more brown in colour and extremely fetid.

When admitted into the Royal Infirmary she had the symptoms above mentioned. The discharge was profuse, brownish, purulent, and very fetid. The uterus itself was not remarkably tender, mobile, and without surrounding induration. It was much enlarged and capacious, its cavity being large and having a length of about three and a half inches. The cervix uteri had a healthy surface, but the os was unnaturally patent.

Sponge-tents were used to effect dilatation of the cervix, and the interior of the uterus was explored with a long polypus-forceps. Shreds of membrane and little masses of decomposing tissue were brought away, but it was found impossible to identify the special structure. The uterus was now repeatedly washed out by injecting freely a stream of warm water through a double catheter, such as is used in injecting the bladder. Numerous shreds of tissue came away. Ten days after this treatment was commenced the fetid discharge had disappeared; the uterus was reduced to its natural dimensions, and the woman soon left the hospital quite well.

Even in women carefully delivered at the full time, accidents may happen analogous to those illustrated in the cases already mentioned. In most such cases, when a portion of placenta, or of membrane, or of both, is left adherent to the uterine wall, separation and extrusion or decomposition and discharge of the abnormally persistent parts is effected soon after delivery,—that is, within four weeks. I do not discuss the question of resorption of such parts, believing that such an event is, so far as fœtal structures are concerned, physiologically impossible. But there is no doubt that parts of the placenta may be retained undecomposed for a considerable period and then break down into fetid discharge, or be expelled entire. The following case will illustrate this to some extent; it is interesting because an able and careful practitioner and a good monthly nurse both satisfied themselves at the time of the confinement that the after-birth was delivered entire and not disrupted. There is a very important class of cases of menorrhagia or periodical monthly floodings occurring after delivery, from which this description of case is easily distinguished and separated by the two circumstances, that the cause of the flooding was a retained lump of placenta, and that the flooding took place only once, the cause of the affection being removed.

Case IV.—Mrs P., multipara, was easily delivered of a child at the full time. After its birth some hæmorrhage flowed, but it was arrested on the removal of the after-birth. She made a slow but good recovery; and the discharges were reduced to a very insignificant amount. Four weeks after delivery, on attempting to get up, hæmorrhage took place, and on making a similar attempt next day blood flowed still more abundantly. Arriving at the bedside, I made an examination, and discovered a firm mass dilating the upper part of the cervix uteri. This I proceeded to seize with a forceps, intending to remove it by any means that might be necessary. It was easily brought away, and found to be a placental mass of the size of a large walnut. It was not in the least degree fetid. After this the hæmorrhage ceased and convalescence was soon perfect.

It is generally believed that putridity always occurs very soon in cases of retention of a portion of the fully developed placenta. Dr Ramsbotham remarks "that the placenta of an early ovum does not always putrefy when left in the uterus, although that of a child at full time, under the same circumstances, invariably takes on the putrefactive process, and that within thirty or forty hours." Case IV., just related, sufficiently proves the possibility of entire absence of putridity for a whole month.

But the most common and easily accounted for cause of fetid discharges from imperfect abortion, is retention of masses of decidual membrane. This can only occur in the early months of pregnancy; for, as this state advances beyond the third month, the decidua is rapidly thinned and atrophied as the uterus is distended, and does not anywhere present any mass.

The advancement of the pathology of abortion, a subject little understood, will surely throw much light on the subject-matter of this paper. At present I merely remark, that in abortion it sometimes happens that the entire double layer of decidua is discharged with the ovum: in this case the abortion may be truly called complete. It also sometimes happens that the ovum alone is discharged unaccompanied by any decidual structures; and in such cases this incomplete abortion is followed, after a few hours, or even a day or two, by the expulsion of the remaining decidual masses. Occasionally no such decidual masses are discharged as masses, and yet recovery is undisturbed; and in cases of this kind the persistent decidual membrane must either disintegrate rapidly and come away imperceptibly in the discharges, or, maintaining its uterine connexions, the membrane may slowly exfoliate and atrophy in like manner as it does after ordinary menstruation. But it also, though rarely, happens, that the decidual masses are retained for many weeks undecomposed [perhaps adherent to the uterus], and then become separated, putrefy, and cause fetid discharges, until they are expelled. In the following case, this series of events occurred; and it is to be noted that fetor was not observed till five months after the end of the first part of the miscarriage.

Case V.—Mrs M., æt. 36, had borne several children. Her youngest child was six years old. Six months before admission to the hospital, she miscarried at about the end of the third month of pregnancy. In about a fortnight thereafter she felt quite well, having only a slight yellowish discharge. But, beginning to walk, she found the discharge become bloody, and it continued so, more or less, till she came to the Infirmary. In the fifth month after the miscarriage, the discharge became much more profuse, occasionally coming away in clots. Before the expulsion of clots she had always much abdominal pain. Shortly after this the discharge became fetid, and the stench about her person was sometimes so great that her children could not abide in the same room with her.

On examination there was observed a very fetid, grumous, bloody, and purulent discharge, containing some shreds of animal tissue. In the vagina was found a rounded decomposing mass of decidua of the size of an almond. Its rounded shape differed from that of the irregular lacerated piece it had originally been, just as a water-worn boulder differs from an unaltered angular fragment. Another piece of decidua like the former was soon afterwards discharged. The os uteri was morbidly patent, and the uterine cavity was three-fourths of an inch longer than is normal. The woman was in a very bad and weak state of health generally.

As fetid discharge still continued, the os uteri was dilated and the uterine cavity washed out with warm water, as was done in Case III. In three days thereafter the fetor was no longer observed. The discharge rapidly diminished; the general health improved; and the woman soon left the hospital cured.

It is to be remarked that all the cases here recorded were characterized by more or less of the local pains of uterine ailment, by the presence of discharges which were either purulent or bloody, by occasional metrorrhagia, by patency of the os uteri, and a hypertrophied condition or a state of imperfect involution of the uterus. Besides, after the lapse of a considerable time, in Cases III. and V., the discharges assumed an intense fetor.

The importance of this class of cases need not be insisted on. By means, the use of which is illustrated in Cases I.. III., and V., the diagnosis will be easily made if the practitioner only suspects the true nature of the case, and is thus induced to investigate it.

For the cure, nothing is required but the complete evacuation of the offending structures from the interior of the uterus. This is effected by the use of the sponge-tent, the polypus-forceps, and the double catheter injection apparatus.

In conclusion, I wish to make a single remark on the pathology of abortion, with the view of suggesting inquiries which will certainly, when completed, throw much light on the subject of this article and on abortion generally.

In early abortions there is frequently observed great hypertrophy of the decidua. This hypertrophy of the decidua may be of two very different kinds. In the first, it is observed to be out of all proportion to the ovum and not otherwise notably morbid. In such cases the history often makes it evident that the ovum being addle or otherwise diseased has ceased to grow but has retained its vitality, while the decidua has continued to grow, and of course the uterus with it. Specimens of abortions of this kind are not very rare. The woman, often a person of experience, reports herself as six weeks or two months, or even farther, advanced in pregnancy; she aborts and discharges an immense mass of decidua having an ovum in one corner of it not bigger than a hazel nut. In the second kind, the ovum has grown to a size proportionate to the presumed stage of pregnancy, but the decidua is larger than is natural at that stage. In cases of both kinds, but especially of the latter, I have observed morbid conditions of the decidua, the result of sanguineous congestion and of inflammation: in some instances I have seen even extensive croupous exudation on the surface of the decidua.

I have already remarked that the ovum is sometimes discharged without any investment of decidua, and have pointed out what may be the future history of the persistent membrane. In connexion with this matter it would be interesting to find the true history of cases of discharge by abortion of one ovum in an early plural pregnancy which was not farther disturbed; whether it came away without accompanying parietal decidua or with more or less of it.

## ARTICLE III.—*On the Theory and Classification of Inflammations of the Skin.* By A. B. BUCHANAN, M.D., Physician to the Dispensary for Skin Diseases, etc., Glasgow.

I HAVE long been of opinion that unnecessary difficulties are introduced into the study of many diseases of the skin, by a tendency to contemplate them as individual unities, while losing sight of their mutual relations. It does not do to fix too exactly the limits of particular diseases; to force nature into accord with our ideas, instead of endeavouring truly to interpret her phenomena. It is, no doubt, very desirable that a clinical physician, or a student, should be able to say decidedly, "this is such or such a disease;" and most of our books seem to have been written by men rather keenly alive to the advantages of rigorous definition. But all deviations on our part from the interpretation of actual phenomena, though they may assist the beginner or the practical physician in some few cases, are just as sure to lead to worse confusion in others. It is far from rare for a study of seeming complications to result in simplifying, to an unexpected extent, subjects which they seemed at first only to embarrass; and nothing can better illustrate this truth than the simplification which results from the study of cutaneous affections, both in their typical forms and in their forms of transition from one well-marked variety to another. Few things, for example, can be more confusing than to open the elaborate work of Devergie, and to read of the endless new species and compound affections enumerated by the learned author, as the result of his unwearied assiduity in one of the widest fields for observation in the world. One would imagine, at first sight, that the complexity of the study was increased in proportion to the number of novelties and compounds introduced by him into its nomenclature. On the contrary, more careful research into the nature of such compounds proves them to be often mere stages of transition from what was previously considered one distinct disease to another, and shows that these new names, instead of encumbering the science by increasing the number of its manifestations, ought to be regarded as conducing to its simplicity by doing away with distinctions which had hitherto been accepted as essential. Thus, *eczema* is placed in one group of Devergie's; in a second, *impetigo;* and in a third, *lichen* and *prurigo;* while these four separate diseases are subdivided altogether into somewhere between thirty and forty distinct varieties. Many are what are called "composite forms," and it only requires some study of these, and some attention to the true nature of the affections, to show that the whole of the diseases thus carefully distinguished, with all their subdivisions, are not only members of one natural family, but mere varieties of the same disease; stages, with a few slight modifications, of a single pathological process.

The inflammations of the skin, towards which I wish ultimately to direct attention, form a perfectly natural class of skin diseases. I mean to examine this class in detail,—to define its limits,—and also to point out some of the highly interesting relations which its members bear to one another. It will be absolutely necessary, therefore, to make a few preliminary remarks on the subject of classification, in order to determine the position of the class inflammations.

Of the different systems of classification which dermatologists have contrived for the purpose of grouping together and distinguishing skin diseases, I shall briefly allude to five, pointing out, in passing, some of their respective excellencies and defects. I shall then suggest and point out the principles of a somewhat different arrangement, which I look upon, at the present moment as not altogether uncalled for, but which I mean here to sketch in its most general features only, that I may return the sooner to the subject of the inflammations. The five classifications which I shall first advert to are,—the classification of Willan; that of Alibert; that of M. Hardy, of the St Louis Hospital in Paris; the classification of Hebra, and that of Mr Erasmus Wilson.

The classification of Willan, and the same as modified by Bateman, is one so much respected by old physicians in this country, as being the system studied by themselves in their youth, that it is almost necessary, for this reason alone, to have some acquaintance with it. It is, except in the case of the order *Exanthemata*, a purely anatomical classification,—an imperfect enumeration of the lesions affecting the skin, which throws no light whatever on the pathological processes concerned in the production of the various diseases; or, in other words, on the essential nature of these diseases. It is a classification of symptoms, or symptomatic lesions; and, accordingly, under it the most dissimilar diseases are sometimes grouped together; and sometimes one disease is dissected into three or four different affections, each of which belongs to a distinct principal order of the system. In the order *pustulæ*, for example, we encounter impetigo side by side with porrigo, with variola, and with scabies. Lichen occurs in one order, impetigo in a second, eczema in a third. Erythema ranks next purpura in one order, pityriasis next psoriasis in another, while strophulus is transferred to a third, so that the natural relations of numerous diseases are altogether disregarded. Willan's arrangement is based on a classification on similar principles, given out twenty-two years before (in 1776), by Plenk, who enumerates *fourteen* classes of skin diseases; or, more properly, of cutaneous lesions. The orders enumerated by Willan are only *eight* in number, he having omitted from Plenk's system various lesions which he rightly regarded as of secondary formation. They are as follows: *papulæ*, *squamæ*, *exanthemata*, *bullæ*, *pustulæ*, *vesiculæ*, *tubercula*, and *maculæ*. Now, of course, an observer like Willan could not deny that, in many diseases, a great number of these lesions are to be found side by side; but he made an attempt to group skin diseases according to the lesion which he considered to be the *primary lesion* in each affection, or to be, on the whole, *most typical* of it. However excellent for the time this attempt may have been, its results cannot now be regarded as either natural or fortunate. Of this, the heterogeneous affections that are still brought together in almost every English work on dermatology bear too ample witness.

The great French dermatologist, Alibert, conceived the idea of arranging skin diseases on a completely different principle, namely, into groups the several members of which should present obvious points of analogy among themselves. He conceived the idea of arranging skin diseases, or *dermatoses*, as he calls them, into *natural families.* Of these families he enumerates *twelve*, of which I shall mention one or two only, to show how really natural some of them are. His first family, for example, is that of *eczematous dermatoses;* and, although I cannot acquiesce in his division of the family, there is nothing more natural than a group of diseases of the skin related to eczema. Then he groups together *exanthematous dermatoses*, or febrile eruptions, cancerous diseases, strumous diseases, hæmorrhages of the skin, diseases of the pigment of the skin, and several others, all of them being collections more or less founded in nature, and showing a great advance, in principle at least, beyond the mere classification of anatomical lesions as enunciated by Plenk and Willan.

Following in the footsteps of Alibert rather than of Willan, M. Hardy, of the Hôpital St Louis, has given us a classification which is, perhaps, taking it as a whole, the best which has yet been published. He calls it a classification of skin diseases *according to their nature;* and although we may be obliged to regard some of his divisions as of more than questionable propriety, still we cannot but accord the highest credit to the accomplished author for this valuable contribution to dermatological science. He divides skin diseases into *ten* natural families, or classes, in the following manner:—1. *Macules* and *Deformities;* 2. *Local Inflammations;* 3. *Parasitic Diseases;* 4. *Eruptive Fevers;* 5. *Symptomatic Eruptions;* 6. *Dartres*, or Tetters; 7. *Scrofulides*, strumous eruptions; 8. *Syphilides*, syphilitic eruptions; 9. *Cancers;* 10. *Exotic Diseases.*

A number of these groups are excellent. The diseases composing them have a natural relation to one another, and both on theoretical and practical grounds ought to be considered together. The *first*, *third*, *fourth*, and *eighth* groups may thus be especially commended. The propriety of the remaining six is, however, more doubtful; and the two worst of all seem to me to be the *sixth* and *tenth.* The class of "Exotic Diseases" comprises simply those which do not occur in France, and with which, therefore, M. Hardy has had no opportunity of making himself personally acquainted. The sixth class, that of *dartres* or *tetters*, comprehends four diseases: eczema, psoriasis, lichen, and pityriasis. These, Hardy, Bazin, and other French dermatologists conceive, although, so far as I can see without any reasonable ground, to be expressions of a hypothetical constitutional tendency, invented for the special purpose of grouping a number of obstinate affections together, under the title of the "dartrous diathesis" (*Herpetismus*).

A classification has recently been enunciated, or rather adopted from Rokitansky, by perhaps the greatest dermatologist of the present day, Professor Hebra of Vienna. Willan classified skin diseases according to their symptomatic lesions; Hebra attempts to classify them according to the preceding pathological processes. Thus, he enumerates *hyperœmias*, *anœmias*, *hypertrophies*, *atrophies*, *exudations*, *hœmorrhages*, and so on; in all, *twelve* classes. These need not be mentioned in detail; because, though an admirable catalogue of the pathological processes occurring in the skin, they cannot be regarded as a successful classification of the *diseases* of the skin.

It would lead me too far to state at length the last classification of Mr Erasmus Wilson, at which the student will do well to look, as it is a step in the right direction; but it does not seem to me to be a successful step. Mr Wilson is, in the first place, too much of a Willanist to make a good classification; he seems afraid to interfere too much with these "holy orders" of exanthemata, vesiculæ, etc., to which we have so long been accustomed. He sacrifices, also, a great deal to the fatal error of attempting to introduce uniformity of principle in classification. Ætiology is surely misapplied as a principle when nothing more can be made of it than such mere verbiage as "general causes," "special external causes," and "special internal causes." I cannot tell what these phrases mean, any more, probably, than Mr Wilson himself; but I see that they result in putting scabies side by side with burns and chilblains, while psoriasis is encountered in the same group as lupus and elephantiasis. I can scarcely believe that Mr Wilson would *now* refuse to recognise a group of vegetable parasites; but the want of it is certainly a grave objection to his system as it stands, more especially as it is the very group to which the ætiological principle of division applies with the greatest pertinency.

Any classification, *resting on one principle of division only*, runs the risk of being, to a greater or less extent, artificial and untrue, in nosology as well as in botany. To secure a *natural system*, several principles must be taken into account, though greater importance may be attached to some of these than to others. However imperfect the arrangement may be of which I shall now give an outline, it has yet been constructed on principles to the soundness of which I have no hesitation in committing myself.

Pathology is the science of disease. Skin diseases, like all others, ought to be classified according to their *nature;* in other words, according to their *pathology*, in the widest sense of the term. Every disease has, whether we know it or not, a *cause*, which cause determines certain *pathological processes* in the system, and these give rise to symptomatic *structural lesions*, and occasionally even to obscurer symptoms, the connexion of which with a definite lesion may not be apparent. Now, a classification according to the nature of the structural lesions accompanying different diseases would correspond in principle with that of Dr Willan; and a classification according to the pathological processes determining such lesions would correspond with Professor Hebra's. In both these systems, however, the *cause*, as an element of distinction, is overlooked: a defect which is remedied in the system of Hardy. A knowledge of the cause of the disease gives generally a far more accurate indication of its true nature, and, as a consequence, of the best means of getting rid of it, than can be bestowed by any knowledge of the processes, lesions, and symptoms which are the mere *effects* of that cause. Therefore we ought to classify skin diseases according to their causes, *if we can;* and if we cannot classify them all in this way, owing to their causes being multiple or unknown, we ought, at least, to group as many as possible together on this principle, which I propose to do under the head of "Diseases defined by uniform causes." Such causes must be *definite;* they must really have an existence in nature; and must not be merely hypothetical, as is the case with the *dartrous diathesis* of Hardy, which cannot be proved to exist at all. The *strumous diathesis*, the *cancerous diathesis*, the *tubercular* and *rheumatic diatheses*, and so on, express conditions which *may* result from special causes, but the existence of these has never been fully demonstrated. So far as we know at present, the groups of skin diseases defined by uniform causes are *three* in number: the *parasitic diseases*, and the *syphilitic* and *febrile eruptions.* There is nothing hypothetical about the existence of a parasite, since the animal or vegetable can be seen with the eye, or with the aid of a microscope. There is nothing hypothetical about the poison of syphilis, since it can be transferred from person to person on the point of a lancet. And, lastly, there is nothing hypothetical about the existence of some subtle agent, capable of transmitting from one person to another such diseases as measles and typhus fever. Sometimes we know of the existence of a cause, can isolate it and recognise it; as in the case of a parasite. Or we know of the existence of a cause, can isolate it, but cannot recognise it; as in the case of syphilis or small-pox. Or, again, we know of the existence of a cause, but can neither isolate it nor recognise it; as in the case of the majority of contagious fevers. Still, in none of these cases is the actual existence of the cause in the slightest degree doubtful or hypothetical.

With regard to the diseases which are not defined by uniformity of cause, we do not know the cause of them, and so we must arrange them into groups according to another principle, and, if possible, according to that which is of next greatest importance in determining their real nature and affinities. Now, a disease is best determined, if we do not know its cause, by the process immediately induced by the action of that cause on the body. The pathological processes determining lesions in the skin enable us, when we consider them, to separate three groups: the *inflammations*, the *new formations*, and the *hæmorrhages*. I do not mean to say that there are not other pathological processes besides these. We have, for example, hyperæmia or congestion; anæmia or inanition; but these do not lead to results which are fairly to be considered as *diseases* of themselves. Thus, unless hyperæmia be sufficiently intense to pass into inflammation, or unless it be *secondary*, from the influence of another distinct lesion, as in acne rosacea, it is merely a transient symptom, which does not rise to the rank of a skin disease.

What is *inflammation* of the skin? A disease of the local circulation, say some; and, no doubt, when a part is inflamed there is generally a rush of blood towards it, which, however, does not make it necessary that the rush of blood should be the cause of the part inflaming. It affects the "pars papillaris" of the corium as well as the "pars reticularis" and the subjacent cellular tissue; and it occurs, according to Virchow, in the papillæ in which there are no bloodvessels in exactly the same way as in the papillæ in which these are present. It occurs also in the papillæ in which there are no nerves just as much, and in the same fashion, as in those in which there are. But, more than this, it occurs in the epidermis, where there are neither vessels nor nerves; and there we see that it is an affection of the cells, which, in consequence of some irritation, become morbidly active; imbibe a greater quantity of plasma from the blood for their nutrition; enlarge, by which they cause swelling of the parts; and even, when the process is sufficiently intense, pour out an exudation from their walls, giving rise to what is called *infiltration*. How much of this infiltration is, in a given case, due to exudation from the cells, and how much to direct transudation from the vessels, is, in the present state of our knowledge, not easy to determine. Suffice it to bear in mind, that an exudation probably proceeds in part from the walls of the capillaries, and in part from the inflamed cells which enter into the composition of the infiltrated tissues.

When the elementary cells of a part, in place of merely demanding more nutriment, swelling up, and pouring out exudations from their walls—in place, in other words, of simply inflaming—undergo alterations of a more permanent character, and which do not, as a rule, disappear when the inflammatory process has come to an end, we give to these results the name of *new* or *morbid formations*. The expression in great measure explains itself. The results are produced in two different ways: (1) by excitement of the *nutritive energy* of the cells, so that they are stimulated to accumulate in their interior substances, such as pigment, which were not there before; and then (2), by excitement of the *reproductive energy* of the cells, so that they are stimulated to divide and multiply, till new parts are formed which were not there before. These new parts are either made up of cells similar to those from the irritation of which they originally sprung, or the cells composing them have become more or less modified in the process of repeated division. If the parts be still similar in elementary structure to the surrounding tissues, or at all events not much modified, so as to be easily traced back to their origin in these tissues, then we call them *homologous* or *benignant* new formations. If quite dissimilar to these tissues, we call them *heterologous* or *malignant* new formations Finally, the heterologous new formations may be conveniently subdivided into *pseudoplasms* and *neoplasms;* the former, as *lupus*, being incapable of independent growth, but extending by the implication of surrounding tissues; the latter, as *cancer*, containing fertile elements within themselves.

The *hæmorrhages* of the skin arise from the escape of blood in small quantities, owing to the rupture of capillary vessels, or to the escape of colouring matter from the blood along with exudations. They are affections peculiar to the cutis; for, although, blood may be effused *among* the epidermic cells, hæmorrhage cannot of course originally constitute an epidermic affection.

If the skin were a simple organ, having established our group of diseases according to cause, and our three groups according to the nature of the process directly called into action, and finding that this division, to all practical intents, exhausts the category of skin diseases, we might consider our classification as accomplished. But the skin is complex; it contains a number of accessory organs—glands, hairs, nails, vessels, etc.—whose affections it is much more natural to consider together than among the inflammations and new formations of the skin. We have, therefore, added to our enumeration a fifth class, founded this time on anatomical considerations, the *Diseases of Accessory Organs.*

In the foregoing classification, uniformity of principle having been deliberately rejected, it will sometimes happen that a disease *apparently* belongs to more classes than one. In such a case, however, it belongs *in reality* to the class established on the most important principle of distinction. And it will be found expedient, not as an absolute rule, but specially with reference to diseases of the skin, to consider the "diseases of accessory organs" as intermediate in importance between diseases defined by uniformity of cause, and diseases defined by the nature of the pathological process. Thus, favus of the nail would belong, not to the diseases of accessory organs, but to the group of parasitic affections. Acne, on the other hand, would belong, not to inflammations, but to the diseases of accessory organs. In classifying the diseases of every great system of the human body, the introduction of some special arrangement of this kind, not always the same, but determined, as it is necessitated, by anatomical peculiarities, will be found expedient as conducing to the formation of *natural families*. The *system* (cutaneous, or nervous, or whatever it may be) must be taken as a whole; but the anatomical peculiarities of that system must be allowed to have a chief influence, second only to the great principle of uniform causation, in determining the distribution of its parts. With these observations the following tabular view of the foregoing classification may be given; the classes arranged in the order that appears *for the diseases of the skin*, to be the most natural and expedient. Diseases belonging to Class 5 are excluded from 4, 3, 2, and 1; those belonging to 4, from 3, 2, and 1, and so on. Thus a syphilitic eruption may be an *inflammation*, but it belongs not to Class I. but to Class V.

*Diseases of the Skin.*

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|---|---|
| Class I. Inflammations, | 1. Erythematous.<br>2. Eczematous.<br>3. Phlegmonous. |
| „ II. New Formations,— | |
| A. Homologous, | 1. Epidermic.<br>2. Pigmentary.<br>3. Dermic. |
| B. Heterologous, | 1. Pseudoplasms.<br>2. Neoplasms. |
| „ III. Hæmorrhages. | |
| „ IV. Diseases of Accessory Organs. | |
| „ V. Diseases Defined by Uniform Causes,— | |
| A. Parasitic Diseases. | |
| B. Syphilitic Eruptions. | |
| C. Febrile Eruptions. | |

With regard to *inflammations* of the skin, while the greater number affect principally, and in the first instance, the superficial layers of the corium, others, the *phlegmonous inflammations*, are more especially subcutaneous, or they implicate the skin in its entire thickness. Some of these affections are diffuse, as cutaneous cellulitis (dermatitis diffusa phlegmonodes); while others, as furunculus and anthrax, have been appropriately grouped together by Hebra under the title of "dermatitis circumscripta." The remaining inflammations, with which we have now to deal, are more superficial. The division of these according to the nature of the anatomical lesion, according to the presence of macules, papules, vesicles, pustules, and so on, is obviously artificial and untrue. On the other hand it seems to me that the pathology of the inflammatory process—a much more important principle to regard than the mere resulting anatomical lesion—supplies us with a distinction founded in nature; in consideration of which the superficial inflammations of the skin may be divided into the *erythematous* or *diffusive*, when the individual lesions are diffuse, or in patches, and the *eczematous* or *selective*, when the lesions are concentrated at points. Such points I believe to be determined either by the presence of cutaneous glands, or by the peculiar arrangement of the capillaries; and these different points of selection—the glands representing the most vascular situations on the surface of the skin—are in reality nearly coincident. A vesicle does not form at the mouth of a gland, sebaceous or sudoriparous, because a gland is there, but simply because such a point is more vascular and becomes, therefore, more readily a centre of exudation.

An important distinction of this kind unquestionably exists in nature, though dermatologists have hitherto made no use of it for purposes of classification. Some modes of irritation of the skin have a tendency to produce one of these inflammatory types, and other modes, with equal certainty, produce the other. If we irritate the skin with cantharides, mustard, ammonia, or corrosive sublimate; or if we produce irritation by rubbing a surface again and again severely; or if the skin be scalded, or burned, or irritated by urine; in all these cases we generally look in vain—I do not say *always*, but *generally*—for the production of papules, vesicles, or pustules upon the affected surfaces. The skin does not get thickened from the exudation of a plastic material at particular points, but both corium and cellular tissue get generally infiltrated with serous fluid. I think it highly probable that this fluid may filter directly through the walls of the bloodvessels, and that the more plastic secretion of the selective inflammations may be, on the other hand, elaborated in the cells of the affected parts. The epidermis, owing to this tendency to diffuse watery exudation, is not raised from the skin in minute vesicles, but the skin is blistered, *vesicated;* the accumulation of large quantities of serous fluid elevates the epidermis in the form of *bullæ.* On the other hand, if we irritate the skin with croton oil, tartar emetic, and several similar agents, the inflammation does not lead to the formation of bullæ, but to that of papules, vesicles, and pustules; in a word, to a series of lesions corresponding to those witnessed in the course of an eczema. Of course, *any* irritant may produce effects such as these last *exceptionally;* as on the skin of a person constitutionally predisposed to eczema, although not on that of a healthy person. We are able to call forth both types at will upon healthy skins by the use of different agents; and therefore I am disposed to recognise in these two types, expressions of actual phenomena, the principles determining which may be naturally referred to in classifying inflammations of the skin.

The threefold division of inflammations into erythematous, eczematous, and phlegmonous, has been already partially applied to an inflammatory group by v. Baerensprung.[1] To Dr Albert Reder, however, so far as I know, belongs the credit of having first suggested its more general use, in a manner nearly in accordance with my own views of its significance.[2] He distinguishes diseases of the erythematous group chiefly by their acute course, and by the absence or fluid nature of the exudation. Diseases of the eczematous group on the other hand, run a course mostly chronic; and the exudation is accompanied with copious new formation of cells, giving rise to crusts or to adherent squamæ. Still, neither Reder nor Bærensprung seem to have perceived the confirmation of an arrangement obviously founded in nature, by the applicability to it of a tendency to diffusion or selection on the part of the elementary lesions; the lesions of the one group for the most part occurring in patches, those of the other usually at special anatomical centres. Baerensprung, who divides inflammations in the first instance into diffuse, exanthematous, and furuncular, is much encumbered by the necessity of including in his enumeration the anomalous forms of eruption brought out under the influence of syphilis and fever. He puts eczema among his diffuse eruptions, impetigo among his exanthemata. Side by side with this last occur variola and psoriasis; showing, once more, that all attempts at classifying diseases on the application of a single principle must necessarily fail in producing natural results.

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[1] Die Hautkrankheiten, von Prof. Dr F. v. Baerensprung. Erlangen, 1859.

[2] Allg. Wien. Med. Zeitung, 1860. Nos. 32, etc.

I shall first give a general view of the erythematous inflammations of the skin, with explanatory remarks; and then treat in the same manner the group of eczematous inflammations. I merely premise that, in subdividing *genera* (if one may so speak) into *species* and *varieties*, more than one principle of division is, here also, admissible in each case; otherwise, the *natural character* of the classification would often be sacrificed.

### *Erythematous Inflammations* (*Diffusive*).

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|---|---|---|
| 1. Erythema | simplex. | |
| | papulatum. | |
| | squamosum | Pityriasis furfuracea; membranacea.<br>〃 〃 rubra. |
| | nodosum. | |
| | strophulus. | |
| 2. Herpes | simplex. | |
| | zoster. | |
| 3. Urticaria | *idiopathica ; ab ingestis ; uterina ; diutina.* | |
| 4. Dermatitis | idiopathica. | |
| | symptomatica = Erysipelas. | |
| 5. Pemphigus | vulgaris | *benignus.*<br>*diutinus.* |
| | foliaceus. | |

All the foregoing eruptions are *diffuse;* by which I mean, that their elements have no tendency to occupy particular centres, such as the orifices of hair follicles and glands, determined by the anatomical peculiarities of the skin. This is very obvious in most erythemata; in dermatitis or erysipelas; in pemphigus, where bullæ rise at once over considerable surfaces; and in urticaria, the lesion of which, or *pomphus*, has been well described by Hebra as a circumscribed œdema, limited to the domain of clusters of capillary loops which spring from a common stem, and are under the influence of a common nervous twig. The first three varieties of erythema enumerated represent so many stages of a *typical erythema,* which may, however, exist in any of the forms indicated, from the very commencement of the affection. When the erythematous inflammation is severe enough, the principle of *selection* is sometimes exceptionally brought into play; and we may then have a few exudations, or papules, concentrated at particular anatomical points of the cutaneous surface, just as in the outbreak of an eczema, only as a superadded, or secondary, and not always constant phenomenon. The form of erythema in which this sometimes occurs is more chronic than *erythema simplex*, but still it runs its course in from two to six weeks, and is designated *erythema papulatum*. It is further distinguished from erythema simplex by its site, and other peculiarities which it would here be out of place to enumerate. Finally, the last stage of an erythema is a desquamation: *erythema squamosum*. Such a form may exist as a desquamation from the first, apparently as a mere inflammation of the epidermis, without the corium being more than congested, if, indeed, it be affected at all. To this form of erythema only is the title of *pityriasis* applicable; and it is also the only form which has any tendency to become *chronic*, the first stage of the disease invariably running a course more or less acute. It includes furfuraceous and membranaceous desquamations of the surface of the skin; as well as the peculiar and rare disease described by Devergie as *pityriasis rubra*.[1]

*Erythema strophulus* is a transitional form connecting erythema with the eczematous group. The papules which characterize it may be formed secondarily on congested patches; but they are still more frequently formed independently at particular points of selection. Instead, however, of assigning it on this ground a place side by side with *lichen*, the affinities of strophulus to erythema, as established by the course they both run, and by their frequent coincidence as symptomatic of the same derangement of system, are so well marked as to leave no doubt as to the natural position of this affection.

The *phlyctænæ*, characteristic of herpes zoster, have been described as large vesicles. They should rather be called, however, *small bullæ;* inasmuch as they frequently cover, even without confluence, considerable areas, and their ordinary size obviously precludes the idea of their originating in connexion with special anatomical centres. *Herpes zoster* naturally takes a position intermediate between erythema, which it resembles in its course, and urticaria, which it resembles in its manifest connexion with the nervous apparatus of the skin, and, as a consequence of this, in the general plan according to which its vesicles are disposed on a given patch. Let a patch of urticaria be compared, *in respect only of the form of the eruption*, with a patch of herpes zoster, and the difference will appear to depend on the same pathological process being carried a little further in one case than in the other; the circumscribed œdema of urticaria being carried, in herpes, to a circumscribed serous exudation. One is not, therefore, surprised to find Hebra describing a case of urticaria in which many of the pomphi passed into the form of bullæ.[1] Of course, however, in spite of such accidental resemblances, no one could possibly confound two diseases in other respects so radically different as herpes zoster and urticaria. *Herpes simplex* is sometimes merely a herpes zoster in which the peculiar distribution of that eruption, in correspondence with the course of particular nerves, fails to be perceived. In other cases it is nosologically identical with erythema, as proved by their occasional coincidence in the same individual from the same cause, both diseases running the same course. Erythema and herpes facialis are thus encountered indifferently as symptomatic of coryza, and after debilitating fevers, etc.; while the non-parasitic circinate form of the one eruption is well-known to be the same thing as, and often to coexist with, the circinate form of the other.

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[1] Traité Pratique des Maladies de la Peau: Paris 1857: p. 442. See also Glasgow Medical Journal, for January 1858.

*Dermatitis* differs from erythema in being an inflammation extending to the deeper parts of the corium, and even, it may be, to the cellular tissue beneath it. The inflammation is not primarily subcutaneous (as in dermatitis phlegmonodes), but it is not necessarily limited to the surface of the corium, as in true erythema. To take Hebra's division,—it is either *idiopathic*, comprehending burns (Derm. ambustionis), the effects of cold (Derm. congelationis), of irritants applied to the skin (Intertrigo; Derm. ab acribus, etc.); or it is *symptomatic*, when it corresponds with erysipelas.

The erythematous, or diffusive inflammations, thus appear to possess sufficient affinities among themselves to entitle them to recognition as a natural family. As a rule, with the single and not always constant exceptions of erythema strophulus, and herpes, each anatomical element of the eruption breaks out from the first over a surface more or less extensive. Though some forms of affection may be chronic, all of them have a tendency to be *acute*, to exhaust themselves after running a course, varying in the different diseases, but the length of which can still be approximatively predicted. To this tendency even pemphigus offers no exception, at least in its occasional form of pemphigus benignus. Again, in erythematous inflammations, the exudation, when there is any, is purely serous, in place of being plastic or purulent. Squamæ separate from the surface of the skin, often agglutinated by a viscid secretion, but these are never well-formed *crusts*, nor is there ever any chronic thickening of the corium due to epidermic infiltration. A further test of the *naturalness* of the group is furnished by the tendency of many of its genera to pass, in particular forms, into particular forms of collateral genera. Thus we see the connexion between erythema and herpes simplex, between herpes zoster and urticaria, between erythema and dermatitis, between erysipelas and pemphigus vulgaris, and between pityriasis rubra and pemphigus foliaceus. At the same time, there are also transitional forms tending to connect erythema with eczema, as seen in erythema strophulus and in herpes simplex. In like manner, dermatitis is closely related to the phlegmonous inflammations, so that Hebra puts them into one class, distinguishing the one as *dermatitis erythematosa*, and the other as *dermatitis phlegmonosa.* Reder also enumerates, and perhaps with justice, *erythema nodosum* (dermatitis contusiformis) among the phlegmonous inflammations.

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[1] Allg. Wien. Med. Zeitung, 1858, No. 2.

*Eczematous Inflammations* (*Selective*).

1. Eczema.
    - 1*st Grade*, Dry..
        - erythematodes.
        - papulatum .................. *Lichen simplex* / *Prurigo.*
    - 2*d Grade* (*Ec. humidum*).
        - vesiculare.
        - rubrum.
        - pustulosum = *Impetigo* ... *sparsa.* / *figurata.* / *pilaris.*
        - rimosum
    - 3*d Grade*, Dry..
        - Lichen...................... *exudativus.* / *ruber.*
        - Ec. squamosum.
2. Acne.
3. Ecthyma.
4. Psoriasis (*punctata*, *guttata*, *nummularis*, *circinata* [*Lepra*], *gyrata*, *confluens*).

I would first observe that I have marked the position which *acne* ought to occupy, if it should seem to any one more natural to take it up in connexion with the inflammations of the skin, rather than as one of the diseases of its glandular apparatus. It seems to me that eczema, acne, and ecthyma form so many genera of an extremely natural group. Psoriasis, on the other hand, is a disease *per se*, spreading according to laws of its own, affecting, almost always, robust and otherwise healthy persons, and quite unlike any other cutaneous disease.

The common characters of the group of eczematous inflammations are these. The eruptive lesions are concentrated at special anatomical centres: the orifices of hair follicles, the mouths of glands, or at any rate, at particular points probably determined by extra vascularity. The deeper epidermic layers are liable to become thickened by a process of infiltration with plastic liquids. When fluid effusion takes place, it is not merely serous, but plastic or purulent; and contributes to a true *crust formation* on the surface of the skin. I have already said that I regard it as *cellular* in its origin, rather than as filtering, like the fluid of a blister, directly through the walls of the capillaries. Lastly, the course of all these affections is chronic and indeterminate. Even if exceptionally acute, this happy result cannot be predicted with any certainty beforehand.

The varieties of eczema—in enumerating which I have taken no more liberties than have seemed to me absolutely indispensable—are, as in the previous example of erythema, expressive of the stages that would occur, one after another, in an ideal case. Thus, the lesion of eczema at the commencement is a *localized macule* (Ec. erythematodes); the macule passes into a *papule* (Ec. papulatum); the papule into a *vesicle* (eczema of the Willanists); the vesicle, on giving way, into an *excoriation* (Ec. rubrum); or into a *pustule* (impetigo). In the next place the skin becomes infiltrated, while the secretion, if there has been any, dries up (Lichen *proper*); and the whole process terminates with a *desquamation* (Ec. squamosum). Now, most of these forms may be assumed from the beginning; or they may follow one another with a regularity more or less complete. They represent *ideal stages;* but at any of these stages the disease may be arrested and may persist; so that they come to express not stages only, but *varieties.* The first two stages (grade 1) are dry eruptions: *eczema siccum.* The next four (grade 2) are moist: *eczema humidum.* The last two (grade 3) are again dry, with the exception of lichen agrius, which is a hybrid or transitional form.

*Eczema erythematodes* indicates, in the first place, a disease to which some authors would give the name of *erythema chronicum.* It is usually circumscribed; beginning at a point, slowly extending peripherically into a patch, which lasts, with symptoms of redness, itching, and slight desquamation on the surface, for months or even years. It differs, therefore, from an erythema, both in its punctuated commencement and in its long persistence; while it generally occurs in debilitated subjects, and in those who have previously been the victims of eczema in other forms. The expression is also applicable to irregular erythemata supervening during the course of an eczema, and obviously manifestations of the same morbid cause as the eczema itself.

*Eczema papulatum*, usually the lichen simplex of dermatologists, is a chronic, dry, papular eruption more or less diffused over the surface of the body. It has been called "strophulus in the adult;" and chiefly differs, indeed, from *erythema strophulus* in not running the general acute course of the erythemata. Lichen simplex is mostly attended with itching; and when the papules happen to be of nearly the same colour with the adjoining cuticle (Bateman) it forms one variety of the indefinite disease which has been described by dermatologists under the name of *prurigo;* an affection, however, often merely symptomatic of phthiriasis, or which is traceable ultimately to an *urticaria nocturna* which has eluded observation. But in other cases, prurigo, when evoked by heat, and often when constitutionally recurrent, is clearly neither more nor less than a peculiar form of eczema papulatum.

When eczema passes into its moist stages, it occurs as *eczema vesiculare;* and, on the rupture of the vesicles belonging to this, as *eczema rubrum* or *madidans.* The lesion in the one case is a vesicle, formed in connexion with some local peculiarity in the anatomy of the cutis; in the other it is an excoriation. Finally, in eczema humidum, the epidermis is scarcely less liable to become infiltrated than in eczema siccum. In this case the epidermis may become *fissured*, and from the fissure exudes the peculiar secretion of eczema. It is then the *eczéma fendillé* of Devergie and Hardy,[1] which is described in no work in the English language. Nevertheless, this is one of the commonest forms of eczema; and as my colleague Dr M'Call Anderson and myself have long been alive to the necessity of having a name for it, we propose to translate the one bestowed by Devergie, and to call it *eczema rimosum.*

I hate new names, and I like to preserve established nomenclature when that is possible; but, first of all, I must interpret nature as I find it. *Lichen exudativus* is really a necessary expression, and one the use of which is sanctioned by the example of Hebra. Lichen simplex has already been discussed, and I let the name remain; it is an eczema papulatum, a lichen as defined by Willan. *Lichen exudativus*, on the other hand, is something very different from a mere diffuse outbreak of papules on healthy skin; it constitutes the more advanced stage either of a lichen simplex or of an eczema humidum, in which the skin, altered by infiltration, is rough, thickened, and fissured. This corresponds closely with the old definition of lichen by both Hippocrates and Galen: "summæ cutis asperitas, cum multâ prurigine, squamis et furfuribus."[2] *Lichen exudativus* may originate in two ways: (1) from a lichen simplex, the papules forming by their confluence a more or less uniform infiltration of the skin; and (2) from any form of eczema humidum, attended by infiltration, in which the secretion becomes secondarily dried up. (*Vid.* Baerensprung, l. c., p. 73). Eczema rimosum thus passes very readily into lichen exudativus; and *lichen agrius*, a variety which it is useless to distinguish, represents the transition form between eczema vesiculare and rubrum on the one hand, and lichen exudativus on the other. *Lichen ruber* is a rare and peculiar affection; for a description of which, as I have never seen it, I must refer the reader to Hebra (l. c., p. 315). Finally, *eczema squamosum* is the last stage of an eczema, whatever its form may have been, dry or moist; and at this stage the disease may persist for long periods. It differs from pityriasis in its antecedents; and also very generally, in being a desquamation *on the surface of an epidermis more or less thickened by infiltration.*

The pustules characteristic of *ecthyma* form, like those of impetigo, at special anatomical centres; but they thereafter spread peripherically till they gain a far greater size. The same mode of advance is shown by *psoriasis*, the lesion of which is a papule extending peripherically, with an accumulation of squamæ on its surface. A *patch* of psoriasis is thus formed, which extends further by confluence with adjacent patches. Lichen and psoriasis, however unlike in nature, are anatomically very similar; and, the true distinction between them is, unfortunately, not much attended to by our dermatologists. It consists in this; that, in lichen, the papules do not extend peripherically so as to form infiltrated patches; on the contrary, successive outbreaks of papules take place, filling up the interspaces between those already present, till a uniformly infiltrated surface is at length produced.

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[1] Devergie, l. c., p. 238; Hardy, Leçons sur les Maladies de la Peau: Paris, 1858: p. 46.

[2] Hebra, Hautkrankheiten, p. 310; in Virchow's Handbuch der spec. Pathologie u. Therapie.

However my own experience may have led me to different opinions on certain points, I cannot conclude this paper without expressing my sense of obligation to Hebra and Hardy, whose instructions first made the subject of Skin Diseases intelligible to my own mind. If my remarks succeed in stimulating some reflective readers to use their own judgments, and to cease to regard papules and vesicles and pustules as the sole, or even the principal means by which skin diseases are to be recognised, they will have answered, in part at least, the end I have had in view.

## ARTICLE IV.—*Notes and Remarks on the Medical Topography of Sanauer and the Mountains around the Simla Sanitaria.* By W. W. IRELAND, M.D., late Assistant-Surgeon H.M.'s Bengal Army.

IN my nearly two years' residence on the top of a mountain above six thousand feet high the question often occurred to me, Whether the human organism was in any degree affected by the absolute elevation? Calling to mind that the pressure of the atmosphere is so much diminished by the ascent, that water boils 13° lower, one can conceive there may also be some change in the condition of the fluids of the body, capable of affecting vital action, as is certainly the case above four miles' elevation in the atmosphere. In the healthy state, up to the height of 10,000 feet, I failed to observe any effects which could not be assigned to the great difference of temperature. The animals and plants of a temperate climate thrive here; and neither Europeans nor natives, as a general rule, suffered in any way by an abrupt elevation from the plains. Majer[1] has observed, that elevation of a place above the sea has an effect in raising the rate of mortality during the first year of life. He found the greatest mortality, during this period, in a district of Middle Franconia, at an elevation of from 1200 to 1400 feet. The mortality of infants under one year was here from 38 to 50 per cent. At an elevation of from 600 to 900 feet, the mortality was no higher than 19 to 22. He found the same relation of infantile mortality to elevation all over Germany. In Prussia, the average of deaths in the first year of life, from the year 1849 to 1855, was 18·1. In Bavaria, from 1851 to 1857, the

[1] Canstatt's Jahresbericht, 1859.

average was 31. This, however, is compensated by the diminished mortality of the children in high elevations from one to five years of age. With them it was only 7·6 in Bavaria, while in Prussia it was as high as 13.

But Majer himself is uncertain whether this difference ought to be set down to the more changeable climate, to the absolute elevation, or to the comparatively deficient care of the children in the more elevated districts; and it could easily be shown that his highest rates of mortality have been reached in some situations in the plains.

About a mile from Kussouli is the hill of Sanauer, on which stands the Lawrence Asylum, founded for the benefit of the children of European soldiers. The hill is about 6000 feet high, composed, as mentioned in my paper of July last, of mica schist, the mica in small fine grains. There are some beds of tuff, and here and there veins of felspar, but no traces of lime. The top of the hill is covered with pine-trees. The temperature is a little warmer than at Kussouli, from which it is separated by a deep ravine. It is on the second range of hills, about twelve miles from the plains by the winding hill roads. The arrangements and food are good; the children appear healthy and contented. The whole institution is under the guidance of a principal, the Rev. Mr Parker, whose name deserves to be associated with that of its great founder in the early history of this most useful foundation. The statistics of the Lawrence Asylum may help us to come to an opinion on the often debated question, whether a European colony could retain its standard of health on the Himalayas.

I have the statistics of eleven years, from its foundation in 1847 to 1859.[1] This institution now includes the children both of its original foundation and of the lower orphan asylum, removed in 1856 from the plains. The amount of sickness has been naturally larger in the children of the latter asylum, which the medical officer attributes "to the want of constitutional tone to resist infantile diseases, which are more virulent in the hills than in the plains." This may be thought some confirmation of the observations of Majer. The asylum has been visited by epidemics, a severe one of hooping-cough, and one of measles; and in 1857 by cholera. There were thirty-five cases of cholera and twelve deaths. It may be here noticed, that cholera has been proved to ascend above 6000 feet. It was at Kussouli in 1845. It is not common in the hills. Mountain chains, in a sanitary point of view, may be considered as barriers to the farther spread of epidemics and contagious diseases, as has been noticed in the mountains of Thuringia, which have checked the advance of hooping-cough from Northern Germany. The disease could only spread by creeping round the flanks of the chain of hills. In like manner, the Himalayas have often saved Thibet from the visitations of cholera, as the great chain of the Alleghanies saved the Atlantic States of North America in 1848 from its advancing march up the Mississippi.

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[1] "The Lawrence Military Asylum, being a brief account of the past ten years of that institution, Sanauer, 1858;" and, "Report for 1859 for the ten months ending 30th April." The medical portion of the Report is short, and the details are, in some cases, rendered valueless by the desire to condense, *e.g.*, fevers are all classed under one head. The author of this article has tried by personal inquiry to fill up the scantiness of the details. The Report was a general one, not intended for the medical world in particular.

My friend Dr Fehrszen adds a very striking illustration derived from the lower animals. He witnessed an epidemic among the horses in the Cape colony, where upwards of 91,000 horses perished in one year; but the progress of the epidemic was checked by the chain of the Camdeboo Mountains, in the district of Graaf-Reinet (Dr Fehrszen's own district),[1]—the farmers on the one side of the mountain losing nearly 90 per cent. of their horses, whilst the loss on the other side did not amount to 3 per cent.

The average of sick in the Lawrence Asylum, during the two disastrous years, amounted in 1856 to 8 per cent., and in 1857 to 6 per cent. The average for the eight years from 1847 to 1855, both included, was about 4·5 per cent. Influenza is said to prevail annually, but it does not appear to present very well marked symptoms. The most singular fact, however, in the medical history of the little community, is the great prevalence of ophthalmia; in 1854 there were as many as twenty-one cases throughout the year, the greatest number being in April (three cases) and in May (four); there were none in March and November. This disease has never left the institution. "The cases of ophthalmia," writes the medical officer to me, "have, during certain years, been numerous, owing in some measure to contagion, but chiefly to the cause assigned in the accompanying Report," which is as follows:—"This disease has been more than usually prevalent (in 1859), and more especially among the boys. This may, perhaps, in some degree, be accounted for from the fact, that the girls are accustomed to play more quietly, and on grassy slopes, whilst the boys' playground is small for the number of children, and has a surface of clay, which, when triturated by the many busy feet constantly upon it, flies up in clouds of dust. It would be difficult on any other supposition to account for the much larger number of cases amongst the boys—as compared with the girls—nearly four to one, whilst in all the other diseases the boys have the advantage. For instance, they have been less subject to fevers by two thirds, to diarrhœa by one third, to dysentery by eleven twelfths, to chilblains and sores by five sixths, and to colds by two thirds." The danger of contagion has, of course, been foreseen and guarded against, and we must look to some other causes to explain the frequency of this disease.

The inhabitants of sandy districts are notoriously liable to ophthalmia, but in some districts much more so than others. In a French work[1] on eye diseases, the question is pointedly put, Why is purulent ophthalmia so common in Egypt? One may reply, owing to the dust, glare, and heat of the sun; but then there is as much dust, glare, and heat in Algiers as in Egypt. Why, then, is there so much less ophthalmia? Here at Sanauer, we have less heat and dust than in the plains below, but more ophthalmia. If we admit the cause assigned in the Report, the question comes—Is there any species of dust peculiarly apt to produce ophthalmia? The dust at Sanauer is micaceous, and so is that in Sinde and in Egypt, countries notorious for the liability their inhabitants have to ophthalmia. Does mica also occur in the soil of Algiers? Perhaps the inflammation may be produced by particles of micaceous dust adhering to the conjunctiva, a view which, if correct, would materially guide the treatment.

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[1] Dr Fehrszen's practice lay in high districts, up to 3000 feet, and also in the plains. He did not remark any greater mortality in the high districts, but rather the contrary.

E. Weber,[2] from statistical observations in Baden, has found that in the primary mountain formation (Urgebirge), which is characterized by forming high mountains and deep valleys, goitre is thrice as common as in the tertiary formation. But the Himalayas in Sirmoor and Bissahir belong to the primary mountain (schistose) formation, and the mountains are as high and the valleys as deep as any in Baden, and there is little or no goitre, save in one corner, near to Dugshai. I have no statistics, it is true; but I have travelled slowly through the hills for hundreds of miles, and the hill people often came to me for medical advice. Yet I never saw one case of goitre, which in some places, *e.g.* in the Nepaul terai, is so obtrusively common. Leopold v. Buch remarked:—"Where there are cretins, it never hails; where there is much goitre, it hails rarely. (Wo es cretins gibt da hagelt es nie; wo es viel kröpfe gibt da hagelt es selten.)" In these hills, however, hail is common, and occasionally the hailstones fall larger than grape bullets.

After diligent inquiry, I was unable to find in the healthy state, either amongst the children of the Lawrence Asylum, or in the hill population in general, any especial tendency to the diseases which experience has proved to be aggravated by an ascent from the plains. These are, according to the Landour Committee, "Malconformations of the chest, affections of the head, organic affections of the heart, rheumatism, and incurable strumous disorders;"[3] to which we may add, for the Simla sanitaria, syphilis and diarrhœa. The first of these diseases appears in the hills in a very severe form, often accompanied by phagedænic sores, which do not appear to exempt the patient from secondary and tertiary symptoms of unusual virulence. It is much dreaded by the hill men, who affirm that it was introduced by the European soldiers, though it is now to be met with in valleys hundreds of miles away from any military station.

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[1] Gosselin et Denonvilliers sur les Maladies des Yeux.

[2] Froriep's Notizen, 1861, iii., No. 15.

[3] Medical Regulations, chap. xlii. § 14. Some would add gout. It is curious to note, that physicians have arrived at similar opinions in high elevations at another latitude. In the Engadin, the highest inhabited valley in Europe, where there are dwellings up to 6600 feet, it has been remarked that cases of deep-seated organic diseases, especially those of the heart and lungs, apoplexy and epilepsy, do not derive any benefit. The cases best suited for this now fashionable sanitarium are—chlorosis, anæmia, scorbutus, indigestion, functional relaxation in general, hepatic congestion, hæmorrhoids, and some kinds of diarrhœa. There are two "brunnens" for Germans. See Das Engadin, etc. Von Professor Lebert. Breslau: 1861.

I have already adverted to the great frequency of diarrhœa in the Simla group of sanitaria; it is the most common disease among the children of Sanauer. Fixing their eyes upon such facts, some medical writers have hurried to the conclusion that this tendency to diarrhœa is the natural result of the great elevation attained by our sanitaria, an opinion endorsed by an eminent Indian pathologist. Hence it has been proposed that we ought to seek less lofty sites for new sanitaria, a proposal which would lead us into very grave practical errors. By availing himself of the medical reports, and the not very accessible literature of the subject, Dr Norman Chevers has given us a most valuable outline of the medical history of the Indian hill stations, from which it is apparent that such conclusions are based upon a very narrow foundation of facts. Indeed, the higher hill stations are almost entirely free from endemic diarrhœa, *e.g.* Darjeling (8008 feet), Murree (7300), Landour (6786), Nynee Tal (6800); 1300 feet below Nynee Tal, at Almora, diarrhœa is prevalent. To find a sanitarium free from diarrhœa, we need not, then, avoid a great elevation; and to obtain one beyond the reach of malaria, in the Himalayas at least, we must take an elevation of from 7000 to 8000 feet, unless we can obtain a hill site not surrounded by a belt of jungle, nor by close narrow valleys, exhaling miasmata for the invalids above.[1] If the reader will reflect upon the known relation between a high temperature and the production of malaria, and a low temperature and its disappearance, he will be disposed to admit that it is only in the higher and colder flights of air one can look for an atmosphere free from the malarious poison.

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[1] Four words of Cullen, "febres miasmate paludum ortae," sum up our knowledge of the etiology of intermittent and remittent fevers. Our hypotheses ought to be directed to explain this fact, the result of immense experience. Of what trifling comparative weight are the alleged exceptions, the theoretical objections, and flimsy theories which have been stated against it? At Kussouli, I have noticed facts which seem to intimate that, in hazy weather, the malaria will readily ascend from the terai. For instance, in the month of December 1858, I and two of my servants, and several natives, had an attack of intermittent fever. I think the temperature of the valley between us and Sanauer was then too low to generate malaria powerful enough to diffuse itself so high, and attack so many people, who had not been troubled with it during a six months' residence in the hills, which, moreover, included the rainy season. It must, then, have ascended from the terai. The hill men who descend into the valleys for wood often get intermittent fever. Though severe, it yields readily enough to treatment. The most formidable-looking case I ever witnessed on the hills, accompanied with great, rapid, and painful enlargement of the spleen, yielded readily to common salt, in drachm doses, and sulphate of copper ($\frac{1}{4}$ gr.), which were used, as quinine and atees were not to be had.

The paramount indication in treating miasmatic fever is, if possible, to place the patient out of all reach of further malaria, for with him a trace of it, which would never affect a healthy individual, will cause a return of the paroxysms. Such a sanitarium ought to be carefully chosen, and guarded by all possible precautions known to be available. At the same time, it would most likely be too cold for invalids suffering from other disorders of tropical climates, who could recover quite as well at a lower elevation. A sanitarium ought, then, to be double; one station at an elevation of from 4000 to 5000 feet, under an experienced physician, who might select the cases requiring a colder climate, and send them up to a higher station, between 7000 and 8000 feet, whence they could descend in the winter to the lower and more temperate stations. These considerations are confirmed by personal observations in the Himalayan range. As the valleys become higher and the climate colder, ague becomes less and less frequent among the hill people.

The total number of deaths in the first ten years of the Lawrence Military Asylum was 40; and, calculating the average number of children at 203, the 40 deaths give a per-centage of 1·97 per annum. In 1850, during the ten months reported on, there were 391 children in the institution, and only 1 death. The ages of the children ran from three years to sixteen.[1]

If one calls to mind that these are children who have passed through the dangerous nurture which brings to an untimely end such a large proportion of soldiers' children in India, and that they enjoy at Sanauer advantages which the offspring of settlers would not share, he will be disposed to think the figures not very encouraging. It is difficult to find a similar institution at home, where children are admitted at all ages. Dr Balfour[2] gives 9·7 per 1000 on an average of ten years, and, after some important dietary improvements, 4·9 per 1000 on an average of 8½ years, as the mean mortality of the boys of the Royal Military Asylum, Chelsea.

But, granting that European colonists could exist and perpetuate their race on the breezy hill tops, they must descend into the hot, close, malaria-haunted valleys, to obtain the means of living. The great objection, however, is, that the Himalayas are already over-

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[1] TABLE of the Approximate Age of the Children of the Institution on the 30th April 1859.

| Sex. | 1 and under 2. | 2 and under 5. | 5—7. | 7—10. | 10—14. | 14—16. | 16—17. | Total. |
|---|---|---|---|---|---|---|---|---|
| Girls............... | 0 | 12 | 23 | 39 | 63 | 27 | 5 | 169 |
| Boys............... | 2 | 9 | 23 | 56 | 98 | 26 | 8 | 222 |
| Total............ | 2 | 21 | 46 | 95 | 161 | 53 | 13 | 391 |

[2] Quoted in Dr Brown's Horæ Subsecivæ, vol. i., p. 256–61.

peopled; from Peshawur to Sikhim there is a large, frugal, laborious, active, striving population, pressed between the snowy range, which they cannot cross, and the torrid plains, into which they cannot descend to labour: for such has been the effect of long residence in a more temperate climate. All the ledges of ground that occur here and there amongst these steep hills and narrow ravines are already occupied. The government have indeed sent home details of hundreds of acres of waste land; many of them have since turned out to be most fallacious; and it has been found necessary to reprimand an employé for "discouraging European settlers." There may be enough here and there for a tea plantation; but we do not believe that in the whole range there is a valley unoccupied which could sustain a dozen English families. The hill people would as soon dream of selling their limbs as their tiny sloping fields. These people are mostly of pure Hindu race, hardy, frugal, and industrious, content to live on coarse unleavened bread, and to go half-naked, in order to save a few rupees for the marriage of their children.

One thing that strikes the observer with surprise, is the hardihood which even natives of the plains, resident in our hill stations, show in enduring the cold. In Kussouli, they will stand out the winter, without any apparent detriment to their health, on half the amount of clothing, fuel, and carboniferous food, necessary to an Englishman. I have seen their little children running about in the snow almost naked, or, at most, with a thin cotton dress over their bodies. The black pigment of their epidermis, which saves them from sudden cooling in their own tropical climate, is no doubt also useful in retaining the bodily heat at low temperatures. These people, be it remembered, are natives of Upper India and the Punjaub; the inhabitants of the coast are much less vigorous, and accustomed to a more uniformly warm and more enfeebling climate.

BONN, 14*th November* 1862.

## ARTICLE V. — *Case of Comminuted Depressed Fracture of the Sternum and Abscess in the Anterior Mediastinum, with Compound Comminuted Fracture of the Lower Jaw, terminating favourably after an attack of Pyæmia.* By JAMES D. GILLESPIE, M.D., F.R.C.S., Surgeon to the Royal Infirmary.

(*Read to the Medico-Chirurgical Society, on the* 19*th November* 1862.)

DAVID HUME, æt. 18, lithographer; admitted 24th June 1857. This patient was walking up the South Back of the Canongate, beside a waggon drawn by two horses, and was immediately behind the front horse, when the driver struck it with a whip. It lashed out and kicked him on the mouth.

*On admission*, there was smart hæmorrhage from the mouth, and from a lacerated wound of about an inch in length between the lower lip and chin. There was an irregular transverse comminuted fracture of the lower jaw to the left of the symphisis. Two of the lower incisor teeth were driven inwards along with part of the alveolar margin. There was great displacement of the left side of the jaw, the teeth sloping inwards and lying under the tongue.

The two isolated teeth were removed along with their alveolar connexions, and considerable force was employed for the purpose of bringing the fractured bones into position, but without effect.

*25th June.*—On examining the fracture to-day with the patient under chloroform, the exact nature of the injury was ascertained. Besides what has been already stated, a longitudinal fracture of the left ramus was also found to exist, causing the almost horizontal direction of the teeth on that side inwards, so as to lie completely under the tongue. This fracture did not appear to extend through the entire thickness of the bone, as no amount of force had any influence in producing a more normal state. The left irregular transverse margin was drawn inwards and backwards.

A pasteboard splint, moulded so as to fit accurately, was applied to the jaw.

*26th June.*—He complains to-day of pain over the right side of the sternum, and swelling is observed there, though there is no appearance of bruising.

The patient now, for the first time, has informed us, and confidently asserts, that the horse's hoof struck him on the chest before it reached the mouth.

*27th.*—The pulse was very frequent and full last night, and he now complains much more of pain over the sternum than at the jaw. He has a short dry cough, but takes a full breath without difficulty. In the evening the pulse was 130, full and incompressible, and he complained much of pain in the anterior part of the chest; no abnormal pulmonary symptoms could however be detected. The patient could not bear examination over the centre of the sternum, where a puffy tumour was forming. Owing to the urgency of the symptoms, he was bled by the house-surgeon to the extent of 16 oz. After the venesection he experienced much relief.

He was ordered the eighth of a grain of tartar emetic, combined with morphia, every three hours.

*28th.*—He had a rigor this morning, which lasted nearly half an hour. Pulse at visit 90, easily compressible.

*29th.*—Pulse fuller and more frequent, 108. During the whole of last night he was very restless and delirious. This evening, pulse 125, incompressible; again active delirium.

*30th.*—Early this morning, having been still more restless, he got an opiate, which procured him some sleep. Since then the bowels have been fully moved by medicine previously taken.

The puffy tumour over the sternum was observed to receive an impulse from the heart's action, and examination caused the detection of a fracture of the sternum nearly opposite the attachment of the third rib. The ends of the bone were found to be separated to the extent of nearly an inch. Friction sounds were heard over the right front; to a less extent over the left. The heart sounds were tolerably distinct and quite normal. The patient had muco-purulent expectoration, but it was doubtful whether it did not come from the mouth, where exfoliation to a certain extent was going on. Pulse 130, full and hard.

Tartar emetic to be stopt and the following substituted:—℞. Sol. mur. morph. ʒiss; Infusi digital. ℥viii.—M. Sign. Half an ounce every three hours.

At evening visit, pulse softer and breathing easier. He is occasionally delirious, but speaks rationally when roused. Six leeches to be applied over the heart, and an opiate to be taken at midnight.

1*st July*.—Pulse less rapid and softer. He is constantly bathed in profuse perspirations. The swelling over the sternum has increased in size, but does not impede the breathing; the respiration is indeed easier since the swelling appeared externally.

2*d July*.—He has passed a very restless night, raving incessantly, and he appears to be weaker. Pulse 108, irregular. Tumour increasing in size. Every pulsation of the heart causes uniform powerful expansion of the tumour, but there is no aneurismal bruit.

A careful examination of the chest was made. The apex of the heart was found beating about an inch below the left nipple. The two sounds were normal, but there was increased dulness over the pericardium. There seemed to be little wrong with the lungs save slight bronchitis.[1]

3*d*.—Another very restless night. Constant raving. Tumour increasing, still more strongly pulsating, and separating widely the fractured sternum. Profuse sweating.

4*th*.—Diaphoresis very profuse. Pulse 104, weak. Tumour fully half an inch higher than the level of the sternum, which is also more prominent than natural. The ends of the bone are fully two inches apart, and the breadth of the tumour is nearly three inches. The digitalis has been stopt, but the opiates have been continued. He has been getting some sherry daily, and it is now to be increased to 8 oz.

5*th*.—Delirium gone. He is now quite composed. The swelling is still increasing, and becoming tympanitic on surface.

6*th*—Pulse 96, regular. Tumour very tympanitic. Skin discoloured, and thin at one point. In the evening he felt very acute pain in the chest and began to scream out that he could not breathe. The house-surgeon inserted a small hydrocele trochar into the place which showed symptoms of pointing, and evacuated a quantity of offensive gas and about two ounces of very fetid pus. When this quantity, by no means the whole, had been permitted to escape, the wound was closed.

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[1] Dr Struthers to-day entered on the charge of the case, owing to my unavoidable absence.

*7th.*—Patient has passed a quiet night. Pulse 96. The abscess is not smaller, but softer and less painful. Three or four oz. drawn off to-day, so that the tumour is now on a level with the sternum.

*8th.*—Between five and six oz. taken away to-day. Patient easy; not sweating quite so much.

*9th.*—A large quantity of pus is coming away by the trochar wound, pumped out by every pulsation of the heart.[1]

*10th.*—Patient not so well. Pulse 120. Countenance anxious; more pain in chest. Perspirations profuse.

*11th.*—A free incision made into the abscess, a large quantity of pus being thus evacuated. Patient easier.

*12th.*—Decoction of cinchona, with sulphuric acid, given to check the constant profuse diaphoresis.

*13th.*—On an examination of the fracture to-day, it was found to be comminuted, a middle portion, not entirely detached, being much depressed. The pus was pumped out from beneath this by every pulsation of the heart. The cartilage of one rib, if not more, is detached from the sternum.

*15th.*—The discharge is not so free, and the cavity of the abscess can only now be evacuated by turning the patient on his face. Eight oz. of wine, and strong soup, still the only nourishment, as he cannot yet use his jaw. An angular portion of the left ramus exfoliated and came away some days ago, also a long narrow thin scale from the longitudinal fracture on the left side. Since this has come away the teeth have regained almost entirely their normal position, and the jaw is becoming rapidly consolidated.

*22d.*—Still sweating extremely, and the abscess discharging freely.

*24th.*—Complains of a lump over the sacrum, which turns out to be a large abscess extending to the hip. Another abscess is forming on the left arm. About 12 oz. of pus were evacuated from the abscess near the hip. The fractured sternum is covered with exuberant granulations; the depressed portion, which was quite bare at one time, is also covered, and has become attached at its upper part. The lower end of the fracture has been drawn up so as to overlap and form a sort of valve, through which the pus comes out with rhythmical precision in accordance with the pulsations of the heart.

*28th.*—Suffering considerably from bronchitis. Abscess near hip healing, that on forearm larger.

*30th.*—Abscess on forearm evacuated. It contained about 5 oz. of pus. Abscess again filling near sacrum.

*2d August.*—A small exfoliation from sternum removed. Small doses of opium ordered to check the cough.

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[1] I again resumed charge of the case.

4*th.*—Opium stopt, as it seemed to encourage the diaphoresis, which was incessant, and a cough mixture substituted.

15*th.*—Abscess on arm healed, that on sacrum nearly so. The depression of the sternum is now becoming spontaneously raised.

20*th.*—Another exfoliation from sternum removed. He is now able to be up for a short time daily, and the jaw is so firm he can partake of animal food.

27*th.*—Out for a short time to-day.

8*th September.*—The fractured ends of the sternum are now slowly approximating, and are on a level. The cavity of the abscess is filled with granulations, which are assuming a plastic form.

6*th October.*—At his own desire he has become an out-patient. The sternum is now firmly consolidated, but a small superficial opening still remains, filled with exuberant granulations, which are to be brought down by sulphate of copper.

16*th January.*—Re-admitted some days ago, owing to sinuses having formed, leading to dead bone. An exfoliation from the sternum has been removed, and he is now quite well.

27*th March.*—Since last report the patient has passed through a very severe attack of typhus fever, for which he was treated in the medical wards, and while much reduced in strength and nervous energy, sinuses re-opened at the sternum, and discharged freely, but they are now completely healed, and he is again in the enjoyment of good health.

Fracture of the sternum is a very rare form of injury, and, when it does occur, is so frequently attended by more serious lesions, that we seldom find recorded a successful case. The complications in this case render it peculiarly interesting. We may, *first*, note the fact, that the patient suffered so much pain and inconvenience from the compound comminuted fracture of the lower jaw, that he completely overlooked the injury to the sternum, and did not allude to it at all for two days, when the appearance of a puffy tumour first attracted our attention. *Secondly*, We have the formation of a large abscess in the anterior mediastinum, to which the throbbing of the heart communicated such uniform pulsation, that for some days considerable anxiety existed as to whether it might not be an aneurism. When this abscess was opened, the churning motion caused by the heart pumped out of the wound, simultaneously with the systolic contraction, the secreted pus. *Thirdly*, There can be no doubt that this patient, in addition to the dangerous lesions mentioned, passed through what I consider a still more serious trial, an attack of pyæmia. The symptoms of this very dangerous malady were strongly marked, and lasted for some weeks, and I cannot recollect any case where a successful issue resulted after such long-continued pyæmic prostration.[1]

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[1] The notes of this case were very accurately kept by my house-surgeon, Dr Alex. Monro Inglis, now Physician to the Worcester Hospital.

## ARTICLE VI.—*Case of Intestinal Invagination: Sloughing: Recovery.*

### By J. BRISBANE, M.D., Galashiels.

ON Tuesday the 8th of April last, a boy aged 10 years, son of a merchant in a neighbouring village, had been leaping, running, and using other active exercise, when at night on returning home he complained to his mother of pain in the belly. Thinking this was dependant on slight gastric disorder, she administered some simple medicine. This having no effect, any more than two other and stronger doses subsequently administered, advice was applied for, under which, more powerful medicines, including croton oil and enemata, were resorted to. No beneficial results following, the parents became seriously alarmed, and I was asked to see the case on Saturday the 12th. At this stage the symptoms were as follows:—Pulse 120, and weak. There was considerable tenderness over the abdomen more particularly in the right iliac region, to which part the patient always referred as the seat of pain. For several days there had been no alvine evacuation, with the exception of a small quantity of fæces probably lodged in the rectum, and washed thence by the reflux of the enemata. Occasional retching occurred, the contents of the stomach being ejected along with bilious matter, but there was no stercoraceous vomiting. An examination was made, but no appearance of hernia was detected.

I had the patient put into a warm bath, where he remained for several minutes. He was then raised, and the cold douche applied, in the shape of two pitchers of cold water dashed suddenly from a considerable height over the abdomen. Immediately after a tobacco enema was administered, but without effect, the bowels still remaining obstinately constipated. All purgatives were now stopped, and small opiates were administered every two hours. The rectum bougie was passed, but no stricture within its reach was found. Inflation was next had recourse to, and failed as signally in its effects as the previous remedies. Towards night another and more powerful tobacco enema was thrown up, and though it produced the most powerfully depressing effect and somewhat alarming narcotic symptoms, still the consummation so devoutly to be wished was as far from being attained as ever.

On Sunday I found no abatement of the symptoms. The patient had passed a restless night. The cold douche was again had recourse to, and, during the day, *strychnia* in doses of gr. $\frac{1}{20}$ every three hours, and calomel in doses of a grain every two hours were ordered, together with gentle friction over the abdomen with olive oil at occasional intervals. The anus, from the frequent administration of enemata, had become so exquisitely tender that the patient could no longer suffer the passage of the tube.

On Monday the intensity of the symptoms had alarmingly increased. Pulse 140; vomiting more urgent, though not stercoraceous, and abdominal tenderness much greater; great anxiety. I had now little doubt as to its being a true case of intus-susception, and from the general history of the case, I had little doubt as to its locality. The little patient had all along pointed to the right iliac region as the seat of his greatest suffering. On administering the enemata it was found that they passed up freely, and from the patient being of a *very spare* habit, I could distinctly trace the distended colon through the abdominal parietes almost to the cœcum. Moreover, the urine was secreted copiously and without difficulty, showing that absorption was going on freely in the upper intestine, and consequently that no obstruction existed there. From all these circumstances my inference was that the obstruction existed somewhere in the ileo-cœcal region.

Hitherto all remedial measures had proved abortive. Almost in despair I thought of the application of galvanism. It was night, however, before a battery could be obtained and put into working order. One pole was then applied to the right iliac region, and the other carried over the abdomen generally. Although the galvanic power was low, yet the pain consequent on its application was so great, from the tender state of the abdomen, that it had frequently to be stopped. After it had been applied at intervals for nearly half an hour, the patient complained of a rumbling in his bowels, and shortly afterwards there was a considerable evacuation of dark green fœtid fæces, which gave him great relief. During the night he slept well, and had another most copious evacuation.

Next day (Tuesday) the symptoms were materially abated, and the patient expressed himself as greatly relieved. The pulse had fallen to 100, and the abdominal tenderness was much diminished. He had taken a little tea and toast to breakfast.

For a fortnight after this there was little change; he had an evacuation daily, though scanty and tinged with blood; he continued to complain of griping pains in his bowels.

On Monday the 28th, nearly three weeks after his first seizure, these pains again increased to an intolerable degree, and at the time when they had reached the acme of intensity, the patient felt a most imperative desire to go to stool. The most dreadful pain and violent straining attended this, when something rather bulky having been discharged, he turned suddenly to his mother and said, "Now mother, I am all better." His mother was rather alarmed, however, on observing pendant from the anus a black fœtid mass, which she removed and unfortunately *threw away*. The exclamation of the boy was fully realized. From this time his recovery was rapid, and with the exception of slight griping pains in the bowels, and the stools being occasionally tinged with blood, he is as strong and healthy as before. I regret exceedingly that I had no opportunity of examining the black mass above mentioned, as the mother had in her excitement disposed of it beyond hope of recovery.

However, from the circumstances attending its evacuation, and the description which she gave of it, there can be no doubt as to its having been an intestinal slough. She described it as being a black soft circular mass, with one portion slipped within the other and horribly fœtid.

This case contrasts strikingly with one which I saw when attending the clinical class of Professor Easton in the Royal Infirmary of Glasgow, and which was afterwards recorded in the Glasgow Medical Journal for April 1859, and epitomized in the New Sydenham Society's Year-Book (1859), page 247.

In it, after the failure of other remedies, favourable results followed the administration of tobacco enemata. In the present case two very powerful ones were employed with no other effect than the production of somewhat alarming collapse.

In commenting upon his case, Dr Easton stated that intestinal obstruction sometimes depended on spasmodic contraction of the muscular coat of the intestines, while at other times it owned an opposite cause, viz., muscular atony, and of course very opposite modes of treatment were required in these different cases.

The difficulty is always to state with precision the true cause, and consequently the treatment frequently becomes empirical, remedies the most opposite being used in close succession. In the present case purgatives of the most powerful character, together with enemata, inflation, and sedatives, were all employed successively and all proved abortive. It was with great misgivings that at last I had recourse to galvanism, being apprehensive lest the galvanic stimulus might cause invagination of a fresh portion of bowel, and thus increase the mischief already existing. Having, however, mentioned it to the parents, they were importunate for its application, which was most fortunately attended with the happy results above recorded.

## ARTICLE VI.—*Note on the Internal Exhibition of Atropia and of Strychnia.* By ALEXANDER FLEMING, M.D., F.R.C.P. Lond., and Physician to the Queen's Hospital, Birmingham.

(*Read before the Midland Medical Society, November* 4, 1862.)

I HAVE for many years employed solutions of atropia and of strychnia for internal use, and prefer them to the galenical preparations of belladonna and nux vomica, because of their greater safety and efficiency. The solutions of both alkaloids which I employ are so proportioned in strength, that ten minims (by measure) is the ordinary commencing dose,—which admits easily of increase for the adult, and of lowering for the child.

The solution of atropia which I use is made thus:—Atropia, 1 grain; distilled water, 5 drachms. Dissolve *thoroughly* with the aid of a few drops of diluted muriatic acid, and add of rectified spirit sufficient to make 10 drachms. This solution keeps well, and is of uniform strength. The tincture and extract of belladonna, however carefully prepared, vary much in power. I have found the tincture of one chemist seven times the strength of the same preparation from another and equally respectable chemist; and the extract is even more uncertain. The internal, and at same time efficient, use of these preparations is for this reason very unsafe.

The solution is so proportioned that 10 minims, containing 1-60th of a grain of atropia, is the commencing dose for the adult. It should be given in a little water, once daily, at bedtime, and on an empty stomach. The dose is increased daily by 2 or 4 minims until a slight degree of the early physiological effects—dry throat, wide pupil, and dim sight—is produced. This is attained with much precision and safety; but it may be necessary to increase the dose to 30, 40, and 50 minims, according to the strength of the patient. For children of one year and all ages under one year, the commencing dose is 1 minim, of two years, 2 minims, of three years, 3 minims, and so on up to ten years, when 10 minims may be given. In verifying the commencing doses for children I have been assisted by my late pupil Mr Burnie, House-Surgeon of the Children's Hospital of this place.

The commencing doses here indicated for children and adults, are all fixed below what may be given with propriety in the majority of cases. While weak persons are readily influenced by atropia, much larger doses than those indicated are necessary in strong subjects, in whom, therefore, a few days are usually lost in the commencement of treatment, before the requisite dose is attained. In the diseases in which I use atropia, as epilepsy, asthma, constipation, and hooping-cough, this delay has no practical inconvenience. It is perhaps not superfluous to note here, that by minims I mean minims by measure. Ten minims of this solution of atropia are equal to eighteen drops, in which form it ought not to be prescribed. I generally order the solution alone, and direct the patient to be supplied with an Alsop's minimmeter to measure the dose.

I give it *once* daily. The action of one dose does not subside completely for sixteen or eighteen hours, and if a second be given before the effects of the first have passed away, we risk the production of cumulative action. When so exhibited the degree of action is less under control, and we may induce unexpectedly an alarming amount of atropism. The cumulative exhibition of atropia is not required to secure any of its therapeutical indications. It must be given *on an empty stomach.* The dose of atropia requires for its due action to be promptly absorbed; mixed with the contents of a full stomach it enters the system very gradually, and manifests its usual effects very imperfectly or not at all. This is one reason why the drug, when taken into the stomach of the rabbit, has no action. It meets there always a large quantity of food, and, mixing with it, enters the system very gradually. The kidneys, meanwhile, are busy eliminating it with the urine, and the atropia is never present in the body in sufficient force at any one time to cause its physiological action. Several experiments have satisfied me that this explanation applies to some at least of the other examples of the immunity of grassfeeding brutes to certain poisons. Their stomachs are always full. Lastly, I give the solution *simply diluted with a little water*, that it may pass quickly and easily into the blood. Atropia should never be given in pill, which may undergo solution very slowly or not at all, when two or three pills accumulating in the stomach or bowels, may, from some change in the gastro-intestinal fluids, be suddenly dissolved and excite severe atropism.

Atropia is sometimes employed internally in the form of valerianate, but this is not a convenient preparation. The valerianate is a very deliquescent salt, and forms a gummy mass, which it is difficult to weigh with accuracy. Moreover, it is a delusion to suppose that the valerianic acid represents in any sense the valuable medicinal properties of the valerian.

The solution of strychnia which I use is made thus:—Strychnia, 2 grains; distilled water, 5 drachms. Dissolve the strychnia *thoroughly* with help of a little diluted muriatic acid, and add of rectified spirit sufficient to make 10 drachms.

This solution has the same advantages over the powder, extract and tincture of nux vomica, that the solution of atropia has over the tincture and extract of belladonna. It is uniform in strength, passes readily into the circulation, and the dose can be apportioned with accuracy. The commencing dose is 10 minims, and contains 1-30th of a grain of strychnia. When employed for its *tetanic* action the solution should be taken in the morning, half an hour before breakfast, and in half an ounce of water, and the dose increased 2 or 4 minims daily until a slight degree of the physiological action—stiffness about the jaws or neck or spasmodic movements in the paralyzed muscles—of the drug is manifested, when no further increase should be made. As a tetanic it should be given only once daily, to avoid the risk of cumulative action by giving a second dose before the operation of the first has entirely subsided. It is taken in the morning, so that its action may be over before bedtime, and the sleep is not disturbed. Lastly, it should be given on an empty stomach, and diluted with water to ensure its prompt and easy absorption. Strychnia ought never to be given in pill. It is hard of solution in the weak acids of the stomach, and several pills may remain unchanged and accumulated there or in the bowels. A change in the secretions may then dissolve and transport them all simultaneously into the blood, and give rise unexpectedly to alarming tetanic symptoms. This is commonly the correct explanation of the so-called cumulative action of strychnia. It is the sudden solution and absorption of hard pills which have accumulated in the stomach or bowels.

When the strychnia is employed as a *tonic*, the dose of the solution is 5 minims, and it may then be exhibited twice daily with safety and advantage.

It would be a great convenience if all our solutions—and under that name might be included the stronger tinctures, as those of digitalis, aconite, opium, and Indian hemp—were made so as to have a commencing dose of 10 minims, which admits, as I have said, of easy lowering for the child, and when increased, does not involve the giving of too much spirit. By helping the memory, such an arrangement would promote materially efficient treatment. There can be few practitioners who are not conscious that in their daily practice, good opportunities of using active drugs are allowed to slip, because, at the moment of prescribing, the memory is not faithful as to the exact dose. A pharmacopœia which made the doses equal of all preparations bearing the same name would help to remove this difficulty.

# Part Second.

## REVIEWS.

*Studies for a History of Nervous Diseases consequent upon Syphilis.* By Gjör. Norsk Mag., vol. xi.

*Des Affections Nerveuses Syphilitiques.* Par les Drs Gros et E. Lancereaux. Ouvrage couronné par l'Académie de Médécine. Prix Civrieux, 1859.

*De la Syphilis dans ses rapports avec l'Aliénation Mental.* (*Dissertation Inaugurale.*) Par le Dr Hildenbrandt. Strasbourg: 1859.

*Des Paralysies Syphilitiques.* (*Dissertation Inaugurale.*) Par le Dr Ladreit de Lacharriere. Paris.

*Maladies Syphilitiques du Système Nerveux.* Par le Dr Lagneau, fils. Paris: 1860.

*Des Affections Nerveuses Syphilitiques.* Par D. A. Zambaco, Docteur en Médécine de la Faculté de Paris, etc., etc. Ouvrage couronné par l'Academie Imperiale de Médécine. Prix Civrieux, Concours de 1859. Paris: 1862.

[*Syphilitic Affections of the Nervous System.* By Dr D. A. Zambaco. To which the Civrieux prize for 1859 was awarded by the Imperial Academy of Medicine. Paris: 1862.]

The last work in our list, which emanates from the school of Ricord, is not a mere compilation, but the result of careful research and the study of cases occurring in hospital and private practice. Dr Zambaco its author, who, we presume, is a Greek by birth, distinguished himself as a student, a graduate, and house-surgeon in the French capital, and, having enjoyed the inestimable privilege of being under Ricord in the Southern Hospital, brings to this task, which has gained him such well-deserved laurels, all the knowledge and critical acumen which are so well known to characterize that much beloved master, who is universally acknowledged as *facile princeps* in this department of surgical pathology.

The subject given out for the 1859 competition for the Civrieux prize was, "Nervous Affections due to the Syphilitic Diathesis;" and in considering this subject, in order to form a common platform by which the logical integrity of his work should be estimated, Dr Zambaco first explains the signification he attaches to the words "*nervous affections*," and then sketches in a general way the history and characters of the syphilitic diathesis. Having thus defined the two essential terms of the problem assigned for solution, Dr Zambaco enters very fully into the pathological anatomy of the syphilitic disorders of the nervous system, but not more so than is commensurate to the importance of the subject.

To facilitate the description of the diseases, they are considered as belonging to three classes,—1*st*, Those which are characterized by an impairment of the motor functions of the nervous system; 2*d*, Those which are manifested by perversion or abolition of general or special sensibility; 3*d*, Those affections which disturb the intellectual faculties. But as this classification does not include such affections as hysteria, epilepsy, chorea, etc., these affections of the nervous system are considered along with the disturbances of the intellectual faculties, under the head of syphilitic neuroses. Furthermore, a chapter on syphilitic paralysis *sine materia*, and syphilitic intermittent fever, occupies a position in the work midway between the affections of sensibility and the neuroses we have already alluded to. The work concludes with chapters on diagnosis, prognosis, and treatment.

To an analysis of this last portion of the work we propose to confine ourselves in this review.

The diagnosis of diseases of the nervous system as depending on syphilis is surrounded with difficulties; but in general terms it may with truth be said, that while no one symptom is pathognomonic of a disease being due to such a cause, the past history and a careful examination of the present condition of the patient are alone calculated to enable the practitioner to discover the essential cause of the malady. The question of diagnosis will, however, be more easily elucidated by taking a rapid glance at the more usual forms which the affection may assume, and observing the different aspects it may present as it occurs in practice.

In the most simple and easily recognised form we find that the nervous derangements are accompanied by other manifestations of the syphilitic diathesis, by which the attention of the practitioner is likely to be arrested.

Thus, at the very outset of the syphilitic cutaneous manifestations we meet with convulsions, hysterical and neuralgic affections. The eruptive fevers are the only diseases with which one is likely to confound the symptoms of nervous derangement which may occur during this period of the syphilitic infection; and this mistake may be all the more easily committed when we recollect that the eruption of syphilis may very closely simulate variola, varicella, or roseola, while the nervous prodromata of which we are speaking usually diminish or disappear in all of these conditions when the eruption becomes developed. But while this is the case, the syphilitic eruption usually lasts much longer than the exanthem of an eruptive fever, and the general nervous disorders, when they have once existed, recur again shortly after the manifestation of the specific eruption, if they calmed down on its appearance. Besides, new symptoms indicative of the diathesis occur, which prevent or, at all events, enable us to avoid or correct any error which might otherwise be made in diagnosis. That such mistakes do occur is abundantly shown in several cases detailed in the body of the work. In one, for example, the patient was supposed by his medical attendant to have three times suffered from measles, thrice from small-pox, and once from typhoid fever.

At a more advanced period in the progress of the diathesis the different paralytic affections, neuralgia, diseases of the eye, etc., may also occur co-ordinately with other manifestations of syphilis, the presence of which render the diagnosis much more easy than it would otherwise be. In such cases an attentive examination of the antecedents of the patient, the progress or connexion of the different syphilitic phenomena, the relation of date or of intensity of the different nervous disorders, with the other concomitant symptoms of syphilis, are so many guides which lead to the discovery of the true nature of the disease of the nervous system. We should, however, make allowance for coincidences, and not attribute every condition which may present itself to syphilis, merely because the patient is undoubtedly syphilitic, for various diseases quite unconnected with syphilis may become developed coëtaneously with it.

At the same time, if a patient has never previously suffered from an analagous affection, of a neuralgic kind for example, if this condition became developed without the existence of any appreciable cause, if it was one of those affections which recur in the course of syphilis at that period at which the diathesis had arrived, if by the date of its appearance it could be referred to syphilis, if, in fine, it was benefited by the employment of specific remedies which produced a decidedly curative effect upon the other concomitant symptoms of the diathesis, we should then have the requisite proofs to enable us to satisfy the most exigeant, and to establish as certain a diagnosis as can possibly be attained to in medicine. Let us take the example of a shepherd suffering from syphilitic epilepsy, detailed by M. Zambaco at page 473. In the first place, none of his family had ever been affected with any disease of the nervous system. He had always enjoyed good health and never displayed any sign of cerebral disease. In these circumstances he contracted syphilis, for which he was subjected to very irregular and insufficient treatment. Two years afterwards, having already suffered from a condylomatous affection of the fauces and violent pains in the head, he became epileptic. After several attacks, the epileptic symptoms yielded to the employment of specific treatment, while at the same time the affection of the throat disappeared, after which several years elapsed during which he enjoyed good health. But at a more distant date, other constitutional symptoms made their appearance, such as ulceration of the pharynx, affections of the nasal bones, and intense pains in the head, and were speedily followed by a return of the epileptic seizures. Subjected anew, and after two years' persistence of these conditions, to specific treatment, the superficial and observable diseased conditions rapidly improved, while at the same time the epileptic attacks diminished in frequency. Some time after both affections disappeared completely. In such circumstances, though the disease for which the patient came under treatment was called epilepsy, it was incontestible, that its symptoms as well as the others we have described were really due to the influence of the syphilitic infection of his system.

But circumstances are not always so simple and satisfactory. It sometimes so happens that, during the active existence of constitutional syphilis, cerebral symptoms make their appearance, which are ill-defined in their characters, or, assuming the form of apoplectic attacks or cerebral softening, persist after the other external syphilitic symptoms have improved under the employment of specific treatment. For example, a patient suffering from gummata of the skin becomes hemiplegic, the cutaneous tubercles disappear under constitutional treatment, the paralysis continues unaffected; still it was due to syphilis, for at the autopsy a tertiary syphilitic tubercle was found in the corpus striatum of the opposite side. But the plastic deposit was surrounded by cerebral matter inflamed and softened. This acute cerebral softening was the cause of the continued fever, the convulsions, and the muscular contractions, which made their appearance nearly a month and a half after the occurrence of the hemiplegia. Thus a new series of difficulties occur to increase the chance of falling into error in our diagnosis. In such cases, we must therefore bear in mind that the syphilitic tubercles by their situation and by the compression of surrounding parts may set up cerebral softening, which may then become the important and even fatal malady; and it need be no wonder in such cases that the specific treatment fails to cure the head affection, occasioned though it be by the syphilitic diathesis.

The diagnostic problem becomes still more difficult of solution when the patient, suffering from some nervous or cerebral disease, has no manifest symptom of the syphilitic diathesis. In such circumstances a careful study of the history of the case, especially if it has been all along under our own care, will afford very great assistance. It was in this way that M. Vidal arrived at a satisfactory diagnosis, and cured, by the employment of antisyphilitic remedies, a young patient from an apoplectic attack, and that M. Ricord similarly cured a patient suffering from cerebral disease.

It is obvious that the difficulties in diagnosis must greatly increase, when the syphilitic symptoms are separated from the disease of the nervous system by a long period of time. Such cases require the most careful and attentive research, not only into their history, but for any trace of the pre-existence of syphilitic affections on those parts of the body most accessible to investigation.

It has been noticed by every writer upon syphilis, that in this disease the painful symptoms occur only or become most intense during the night. But some have applied this tendency to nocturnal exacerbation or recurrence not only to the pains in the bones, of which it is very characteristic, but to other conditions as well. Thus, Frank holds that it is a characteristic of syphilitic epilepsy that the seizures come on at night, and some have attempted to maintain that all neuralgic affections which possess a nocturnal periodicity or increase in severity are due to a similar cause. Were this the case, diagnosis would be immensely simplified; unfortunately, however, all such conclusions are found to be quite unsupported by facts.

Among the different nervous diseases some are so frequently found to be due to syphilis, that it is only proper in the practitioner when such a case occurs attentively to expiscate the antecedents of the patient so as to determine whether or not the syphilitic diathesis forms an episode in his biography. Among such nervous affections paraplegia, palsy of a single limb, of the muscles of the eyeball, and amaurosis, may be mentioned as common examples.

There are, however, another and rarer class of cases in which the presence of syphilis as a determining cause is too frequently overlooked, and for which the only really efficient remedy is thus unemployed. An example will make our meaning more intelligible. Suppose that we are called to see a young man, not above 28 years of age, suffering from hemiplegia. In such a case, although there is no symptom indicative of syphilis present, the practitioner recognises as a fact the result of experience, that cerebral apoplexy in one so young is an exceedingly rare condition. He therefore inquires into the previous history, and finds that at some antecedent date there is good reason to believe that he had undoubted symptoms of syphilis. This first hint in a right direction, corroborated by probability, supported by additional facts, and elucidated by reflection, leads to certainty in diagnosis. Among the probabilities we may instance, first, the frequent relapses met with in syphilitic cases, its resisting treatment, especially when that treatment has been irregular, whether as regards kind or continuance; the suddenness of its attacks, occurring as they often do when the patient is apparently enjoying perfect health, etc. In such circumstances the practitioner may very reasonably suspect the existence of syphilis as an exciting cause of the diseased conditions which are present, and reasonably diagnose a syphilitic affection of the brain, more especially if the paralytic symptoms have come on by degrees and progressively, or, as is more common still, if when the paralysis has occurred suddenly there has been no loss of consciousness.

Treatment will still further assist us in determining the question, by rapidly removing the symptoms from which the patient suffers. But as we have already said, in exceptional cases no satisfactory results occur even when the treatment has been both carefully and rationally conducted, allowance must be made for the effects which continuance of the lesion of the nervous system must have produced on the surrounding parts.

From what has been said it may be inferred that a study of the history of any such case, with a thorough acquaintance of the various aspects under which syphilis may show itself, should enable the practitioner in the majority of cases to arrive at a decided and satisfactory diagnosis. We must not, however, forget the difficulties which beset us in tracing such histories. For, in fact, such is sometimes the determined obstinacy of patients to deny the possible occurrence in them of syphilis, that the practitioner requires to be constantly upon his guard lest he should be duped by a too ready dependence upon their statements. This dissimulation is particularly common in the case of females, who ostentatiously deny any syphilitic antecedent, and even persist in their denial when we have undoubted signs of syphilis in various parts of the body, such as condylomata, gummata, nodes, etc. It is only just, however, to add, that patients and more particularly female patients, are frequently quite ignorant of their having become infected, so that when they attempt to deceive the practitioner whom they consult it is usually from ignorance and not from any sinister motive.

Lastly, The comparison of doubtful cases with others which are analogous in all respects, with this exception, that in them the existence of syphilis as a cause has been demonstrated, may frequently afford us valuable assistance in making out our diagnosis. And this fact is all the more necessary to be remembered, for a patient may suffer from all the sad and fatal consequences of syphilis who has never contracted the disease in its primary form; for hereditary syphilis is quite as competent as the acquired form of the disease to produce disordered conditions of the nervous system. In such cases, the true nature of the disease will be all the more easily misconceived when the disease of the nervous system develops itself at a distant date from birth.

From all we have said it is sufficiently obvious that to demonstrate the presence of the syphilitic diathesis as an active cause of disease is sometimes very difficult. It is no doubt true that, in such cases, practical men sometimes make a happy guess when rigid proof is wanting. But, for the satisfactory diagnosis of syphilitic diseases of the nervous system, or in fact of any syphilitic disease of a recondite kind, it is essential that the practitioner should be thoroughly acquainted with the pathology of this protean disease. In the investigation of every disease it is in fact quite essential for the practitioner to keep in view the possibility of syphilitic infection having some part in its production. In studying the histories of most cases, it is very common to inquire whether the patient has suffered from measles, scarlet fever, small-pox, and typhus or typhoid fevers, etc., etc. A similar careful research, with or without making the patient conscious of it, as to whether syphilis has formed an antecedent in his history would seem to be a reasonable deduction from the considerations and facts elicited in this volume.

The prognosis of the nervous affections which occur as a result of syphilis constitutes a complex problem, which may, however, be separated into the following considerations:—

1. Do syphilitic nervous affections always undergo a cure?
2. By what signs can one recognise that they are making progress towards a cure?
3. Do they leave any infirmities as a result?
4. Have they a tendency to recur?

We have already admitted that the affections of the nervous system produced by syphilis are not always curable. Some of them pursue their course to a fatal termination, in spite of the most regular and rational treatment, even when employed from the commencement of the diathesis. Nevertheless, it must be granted, that in most cases syphilis is a disease which is more easily cured than almost any other in the whole nosological catalogue.

Putting aside, in the meantime, these unfortunate and for the most part exceptional results, when we attempt to foresee as far as we can the progress of the affection, it is necessary to take into account the following circumstances. Other things being equal, we may expect the disease will be more intractable in proportion to the length of time it has existed, the frequency with which it has relapsed, and where it is of hereditary origin. It is still further of great importance to take the site of the disease into account in considering the ulterior progress of such syphilitic nervous cases. The affections which involve the nervous system generally, such as convulsive or hysterical attacks, or disturbances of the sensory or motor functions, etc., usually disappear at once under the influence of mercurials, and sometimes even spontaneously, without the employment of any treatment. Those affections which are apparently without any organic lesion seem to be more amenable to treatment than those in which we have organic disease interfering with the functions of an organ. Furthermore, the structure of the part affected should be taken into consideration; thus, the retina may undergo, in consequence of a syphilitic affection of old standing, such changes as must in all time coming prevent the possibility of its regaining its healthy function. The form which the affection assumes may sometimes be of service in assisting our prognosis. Thus, for instance, it will usually be found that those cases of general paralysis which resemble the general paralysis of the insane, die in spite of treatment. In some cases, again, the stage of progress of the symptoms appearing upon the surface may to a certain extent serve to reveal to us the condition of internal organs, and so to indicate the probable tendency of the nervous symptoms. Thus, in a patient suffering from syphilitic chorea, with coincident sore throat, the increasing severity of the latter symptom presaged the serious character of the disease of the nervous centres. Under treatment, both classes of symptoms disappeared; but, shortly after, a fresh affection of the throat, coupled with hysterical attacks, presented itself; while finally gummata of the velum palati and epileptic seizures occurred together.

When a relapse occurs after a first attack which has yielded readily to treatment we may expect a like satisfactory issue to result from its re-employment.

The affections of the encephalon, but specially those of the spinal cord, are rarely completely cured by the employment of treatment.

In the case of the neuroses, properly so called, and more particularly is this true in epilepsy, the symptoms may persist, and the attacks recur, in spite of the syphilitic symptoms having entirely disappeared. At the same time, it is well to recollect as a rule that syphilitic epilepsy is in most cases quite curable.

Speaking in general terms, when an improvement takes place in the symptoms accessory to those of the nervous system, we may anticipate a favourable change in the latter, and the complete disappearance of the external affections is usually indicative of a more or less complete cure of the disease having occurred. Still, the symptoms of syphilis are so liable to relapse, so insidious, too, in their progress, that we should never trust too implicitly to any improvement, however decided and advanced it may be. Case No. 21, in the body of the work, serves to illustrate this point. A syphilitic patient suffered from paralysis of the third pair of cerebral nerves; after five months' continued treatment, every symptom of this affection completely disappeared; but within fifteen days after this apparent cure, when the employment of medicine had been discontinued, cerebral symptoms supervened, and, becoming more serious than before, terminated fatally. At the autopsy, a syphilitic tubercle of the size of a hen's egg was discovered, occupying the situation of the *sella turcica*. In all such cases, a prudent reserve should be exercised by the practitioner in speaking too confidently of the cure which has been effected, recollecting that this has proved too often only temporary and illusory. In the instance we have just quoted it is obvious that death was not due to a freshly developed affection, but to the persistence of the same morbid condition which, in the few days preceding the fatal result, had probably undergone a fresh development, due without doubt to the discontinuance of the treatment. As a corollary to this it would seem only prudent to continue the employment of the specific treatment of the constitutional disease for a considerable period after the symptoms have disappeared, if we hope to obtain a permanent and complete cure.

We have already indicated that, in some cases the degree of structural change which has taken place in consequence of the progress of the disease, it is impossible to expect the function of the part to become restored, however carefully and persistently treatment may be employed. This fact is well illustrated by the progress of cases of syphilitic affections of the retina and choroid which have been left to themselves for a considerable period. In them, although vision does, under the action of specifics, usually undergo a marked improvement, we more frequently find that the plastic deposits, seen in the fundus by means of the ophthalmoscope, remain permanent; just as in syphilitic iritis, the lymph upon the free margin of the iris continues unaffected by treatment, and produces more or less deformity of the pupillary aperture. The same, too, holds true in regard to syphilitic paraplegia, all experience proving that, when once established, though every other indication of the syphilitic diathesis has been completely removed by the use of specific treatment, the lower extremities never completely regain their functional integrity. It is certainly difficult to account for this persistence of the enfeeblement of the motor power when once it has occurred; the only legitimate theory is to suppose that the compression of the essential elements of the spinal cord by the syphilitic deposit has so modified its intimate structure that the nerve tubes never become thoroughly restored, even when the tumour becomes completely absorbed.

It is very necessary for the practitioner to exercise a prudent reserve in replying to the inquiries which will be certainly made of him as to the degree of confidence which should be put in any apparent cure of syphilitic affections of the nervous system; for Dr Zambaco states that, in every case of syphilitic paraplegia which has come under his notice, a return of the symptoms has occurred at some after date.—

> "So much so, that if we examine our observations generally, it is rare to find a patient who has suffered but once from manifestations of syphilis; frequently, on the contrary, in spite of the best devised treatment continued for three, four, or five months, after every diathetic symptom has disappeared, it has formed no insuperable barrier to the development of new affections. These relapses may occur within a very indefinite period, varying from months to several years. By examining our observations, we find remissions of five, ten, and even twenty years, without any diminution in either the intensity or violence of the diathesis. Frequently, after such long intervals, during which the diathesis has slumbered, it wakens afresh, revealing itself by manifestations all the more dangerous that their insidious commencement makes them easily overlooked."

We do not propose to consider, under the head of treatment, more of the extensive subject of the therapeutics of syphilis than has a direct bearing upon the nervous diseases which are due to syphilitic contamination. But these affections, occupying as they do the two principal periods in the history of the syphilitic diathesis, now accompanying the secondary, now the tertiary period, may be said to afford examples of the treatment of this protean disease under circumstances which enable us to appreciate the benefits of treatment far better than in those other manifestations of the disease which, if more apparent, are less liable to be followed by untoward results.

Mercurial remedies, then, first call for inquiry, possessing all the reputation which long use, popular confidence, and violent detraction, can impart to any therapeutic agent. Mercury, it is well known, has been, and is at the present day, accused of being the occasion of all the affections of the tertiary period, nay, by some of producing these very diseases of the nervous system which we have considered. Such an impression, after being abandoned for a very considerable period, has been again broached, not only in the German schools, but even nearer home. How far such views are justified by facts can be best estimated by considering, *first*, what is the natural history of syphilis when left to itself, without the employment of any constitutional remedies? Such cases have fallen under the observation of most surgeons; Fergusson, for example, has seen gummata, nodes, and other tertiary affections occur in individuals who have been treated, from first to last, without the administration of a single grain of mercury. The results of Porter's observations confirm these remarks. Having treated cases of sores upon the genitals without the administration of any mercurial, he found that, in a fourth of his patients, constitutional symptoms of syphilis occurred, which is a proportion somewhat larger than is usually accredited to the general mass of cases of chancre, taken indiscriminately. The conclusion, therefore, to which we must inevitably come appears to be, that syphilis without the employment of mercury is quite competent to produce all the symptoms of "confirmed pox."

The *second* question which we must eliminate in the discussion of this subject regards the effects of mercurials upon the system, when syphilis creates no confusion in estimating results. In cases of chronic poisoning by mercurial vapour or emanations, as occurs in various trades, we meet certainly with trembling limbs, salivation, eczematous eruptions, etc., which, however, have no analogy whatever with syphilis. But it is in its effects upon the osseous system that it has been supposed that mercury is most nearly allied to syphilis. On this subject Virchow has collected in his work upon syphilis all the facts which are supposed to favour this view. He points to the observations of Lorinser, who speaks of having seen extensive necrosis occur in the workmen in quicksilver mines. M.

Vénot speaks of fracture of the bones, especially the ribs, and more particularly when occasioned by slight causes, as characteristic of syphilis; and, according to Jüngken, this, too, is common among the workmen in the mines of Almaden. He also reports the case of a shepherd employed in rubbing a solution of corrosive sublimate into some sheep affected with scab, who was attacked with profuse salivation and necrosis of the tibia. He also speaks of having seen a case of extensive necrosis of the lower jaw supervene upon the use of the acid nitrate of mercury as a caustic in uterine disease. He further attributes the production of rheumatism in exhausted subjects to the employment of mercurials. Daily experience, we think, serves sufficiently to enable us to appreciate the true value of such assertions. We frequently enough meet with patients who work with quicksilver attacked with trembling limbs, paralysis, and all the phenomena of a cachectic state of body; but they have none of the symptoms of syphilis; they do not suffer from nodes, or gummata, or any of the specific alterations of parts or organs so characteristic of syphilis; in fact, none of their symptoms could possibly be confounded with syphilis by any one sufficiently acquainted with the characters of the disease to make his opinion of any value.

In the *third* place we have to consider, What are the effects in syphilitic patients when mercurials have been too actively employed? If, in former times, when the use of mercury was carried the length of abuse, necrosis of the lower jaw sometimes occurred, this complication was due to the mercury, not to the syphilis; to the effect of the mercurial, in loosening the teeth from the gum, producing irritation in their sockets, and communication of the mischief to the periosteum.

We have already said that the deleterious effects of syphilis upon the nervous system has been attributed to the administration of mercury. This is an old bugbear, as old as Van Sweeten at all events, who narrates a case where he says he found mercury in the metallic state in the ventricles of the brain; but M. Reynaud more recently brought up the question in the Academy of Medicine, by the narration of the case of a syphilitic patient, who died with symptoms of cerebral *ramollissement*, after undergoing a course of anti-syphilitic treatment, in whom he discovered mercury in the substance of the brain, and concluded that his death was due to the use of the mercury. Cruveilhier made some interesting observations upon this subject. After employing daily mercurial frictions upon the surface of the abdomen and inner aspect of the thighs in several dogs, he secured them in a leather breeching so as to prevent them from licking themselves, and found that they all died within a week of mercurial poisoning; in fact, they were so saturated with the poison that they passed it in their stools; still, in spite of the most careful and minute analysis, M. Guérard was unable to find the slightest trace of this metal in their tissues.

It is scarcely possible to imagine anything more conclusive than these experimental researches. Still it is worthy of remark, that if it be the case that Otto (Pathologische Anat., vol. i. p. 150) has found mercury in the bones, and if Coulson has discovered it in the blood of patients when taking mercury, and M. Grassi in the left anterior lobe of the brain, these results have not been confirmed by later investigations. M. Personne, who, by analysis, has detected mercury in the milk of nurses when taking this remedy, has always failed to detect it in the brain substance of patients who have died after mercurial treatment which has even been continued for several months. Besides, even if mercury were always detected under such circumstances in the brain of individuals to whom this remedy has been administered, before giving it the blame of causing the nervous symptoms, it would be necessary to prove, by comparative analysis, that more mercury was contained in the brain than in an equal weight of liver or any other organ.[1]

In regard to nervous diseases the question we are considering is not immaterial, for mercurials may, in certain circumstances, produce tremblings, paralysis, and other affections of a like kind, which may be confounded with the effects of the syphilitic infection. Dr Lagnau, for instance, while he admits that convulsive attacks may result from the abuse of mercurial remedies, believes that epileptic attacks cannot be occasioned by such an agency. Vidal (de Cassis), on the contrary, has recorded an instance of this in his work.

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[1] The following interesting results are extracted from a paper of *Waller* of Prague in the *Prager Vierteljahrsch.*, vol. lxiii., p. 134: 1859. The inquiry was to determine the accuracy of the statements of the Viennese anti-mercurialists, with reference to the accumulation of mercury within the system, and the power of iodide of potassium to effect its elimination. "For this purpose he analyzed with care the urine of eight syphilitic patients who had taken mercury, and found, 1*st*, That the mercury demonstrable in the urine of patients taking this remedy is excreted spontaneously by the urinary organs without the assistance of iodide of potassium; 2*d*, That this spontaneous excretion is observed in whatever way the medicine is given; 3*d*, That without accurately determining the quantity necessary to produce this elimination, it is evident that small doses are quite sufficient; 4*th*, That this spontaneous elimination of mercurials commences some time after the commencement of the treatment, and goes on gradually diminishing in quantity for an undetermined period till no more can be discovered." Mercury thus absorbed into the system behaves itself like other substances of a like kind, being by degrees eliminated by the urine, and in every instance independently of the influence of iodide of potassium. M. Schneider in the *Journal de Pharmacie et de Chemie* (November 1860), shows that so long as mercury is administered it appears in the urine, and that this continues for rarely so long a time as six weeks after its administration has ceased: that this eliminative process is not due to the administration of the iodide of potassium; nay, that the employment of this salt rather tends to check the elimination of the mercurial. He also mentions the case of a patient who had suffered from syphilitic affections for five years, and during this period was three times energetically treated with mercury, and died only two months after the last course of treatment, in whose body not a trace of mercury could be found except in the kidney, when the quantity was infinitesimal, and in the liver, where its presence was doubtful.—See *Archives Général de Médécine,* vol. ii., p. 466: 1861.

The patient, who was suffering from psoriasis, had employed mercurial baths for twenty-two days consecutively, when a convulsive seizure occurred, characterized by foaming at the mouth and loss of consciousness. On coming to himself the patient was affected with a tremulous state of the limbs, which continued for a long time, and presented exacerbations and remissions. One certainly might naturally enough be inclined to blame the mercury, but as this patient was under the influence of the syphilitic diathesis, for the cutaneous manifestations of which he was employing the mercurial baths, the nervous affections may more reasonably be attributed to the syphilis than to the mercury.

It seems void of practical benefit to prolong this discussion. Clinical facts obviously are all against the anti-mercurialists, and no one who has carefully studied the syphilitic diathesis will not assent to Virchow's assertion that it is almost impossible in such cases to distinguish the effects of the mercurial remedies from those of the syphilitic infection. Since the success obtained by Torella and Berenger de Carpi, mercury has reigned supreme in the therapeutics of syphilis, since that period, too, it has constantly had its detractors. No less than 300 years have been spent in this vain controversy; it is about time to end it! Every therapeutic agent of any activity has been blamed for producing diseased conditions precisely similar to those for the treatment of which it is employed. Has not sulphate of quinine been supposed by some to produce cerebral mischief in rheumatic patients; but that is no reason for renouncing its employment. The same may be said for mercury; it merits none of the blame which has been heaped upon it. It is the abuse and not the appropriate use of mercurials which deserves censure, and for long its employment has by most of those conversant with the treatment of syphilis been restricted within proper limits.

But it is not in the secondary stage of syphilis only that nervous symptoms occur, they also appear during the tertiary period. Here it is that the use of iodide of potassium is indicated. This useful agent has also shared the fate of mercury: reprobated by some, approved by others. When it was brought into repute from comparative obscurity by Wallace, who first pointed out its therapeutic efficacy, it was by some supposed to excite the very maladies it was employed to cure. No doubt in some idiosyncrasies it may produce cerebral symptoms, influencing the sensibility, motion, and even the intellectual faculties, or sometimes inducing stupor, as has been pointed out by M. Baumes; but in the vast majority of cases the symptoms indicating a lesion of the nervous system and occurring in the course of the syphilitic infection make their appearance before a single grain of this remedy has been administered.

Having thus justified the employment of these two principal remedial agencies in the treatment of syphilis, we would briefly describe the mode of their application.

In the first place, then, while, as a rule, mercury is specially suited to the treatment of those nervous lesions which occur in the secondary period of the syphilitic infection, and iodide of potassium to those which appear in the tertiary period of its evolution, it should not be forgotten that these two stages insensibly blend one into the other, and that to this intermediate stage a mixed treatment consisting of both mercury and iodine is found to be best suited.

Before the anti-syphilitic properties of iodide of potassium were properly understood, mercury was employed to treat the diathesis in every period of its evolution. Even at the present day some practitioners employ it indiscriminately at every period and for every symptom, while others only resort to the use of iodide of potassium when mercury fails to afford relief. Vidal, for instance, says that mercurials may be employed with success in the treatment of every form of venereal disease, still he recommends iodide of potassium to be given in the tertiary stage, as the whole of his work testifies. As a rule it may be stated that mercury is to syphilis what cinchona is to the paludal poison. Thus it is, that even in the tertiary stage of syphilis, where the iodide of potassium seems either inert or inefficacious, the addition of mercury to that remedy, or even the employment of mercury alone, is attended by the most satisfactory results.

In the early stage of the diathesis, when neuralgic affections are the form of nervous derangement most commonly met with, they yield rapidly to the administration of mercurials. In such circumstances, the proto-iodide of mercury in small doses seems particularly successful. When, after some degree of improvement has taken place, we find that, push the remedy as we may, the condition remains obstinately unaffected, further good effects follow upon a change in the preparation of the mercurial which is administered; thus, the bichloride may be successfully substituted for the proto-iodide; and M. Rayer finds the pills of Sédillot (mercurial ointment and soap) afford very satisfactory results in cerebral syphilitic affections. In administering mercury it is generally advisable to produce a slight degree of the physiological effects of the remedy, but not more than merely to touch the gums, so as to enable us to know what is the dose requisite to produce the full effect of the drug—a dose which, it must be remembered, varies in each individual instance. Having attained to this degree of effect, further buccal irritation is not desirable, and, should it occur, must be checked by the use of chlorate of potash, purgatives, and astringent applications to the mouth. In such circumstances, of course, the use of the mercury must be stopped until the mouth is better.

Sometimes we meet with cases of extreme obstinacy, which refuse to yield to the internal administration of the mercury. In such cases, inunction with mercurial ointment may be satisfactorily resorted to. In using this method, care should be taken to prevent local cutaneous irritation, resulting from the continued application of the ointment; this may be satisfactorily secured by shifting the locality to which the ointment is applied, and by washing the surface from time to time with soap and water.

When the mercurial administered by the mouth upsets the stomach and bowels, the addition of a small quantity of opium or morphia with each dose removes this disagreeable effect. Where, however, it cannot be borne, then inunction or the mercurial vapour-bath are the only means by which the remedy can be satisfactorily or safely introduced into the economy.

When mercury is employed with these precautions, the symptoms of syphilis may be said constantly to yield to its employment. At the same time, care must be taken and attention paid to details and individual peculiarities, in order to obtain satisfactory results, for no wholesale system of anti-syphilitic treatment can be expected to succeed.

In the secondary stage of syphilis, Wallace was the first to recommend the use of iodide of potassium. Unfortunately, however, with him this remedy was a universal venereal panacea. It remained, however, for Ricord, after an extended trial of this remedy, to show that its effects were only satisfactory in cases where the syphilitic diathesis had attained the tertiary stage. In such circumstances, under its employment, rapidly beneficial results follow; but allowance must always be made for the nature and structure of the part affected, else we may be disappointed in our expectations; as, for instance, in tertiary affections of the choroid and retina, of the brain or spinal cord, when such a degree of structural change has taken place that, however completely the syphilitic tubercular deposit may be removed, the function of the part cannot be restored. In the administration of the iodide of potassium a great diversity of opinion exists as to the dose; some would never give more than six grains in the course of the day, while others would give as much as half an ounce. Some patients bear these larger doses well, without derangement of stomach or bowels; others cannot take the smallest dose for any protracted period without suffering from symptoms of iodism. The remedy should therefore be employed carefully, beginning with a small dose, gradually increasing it, so as to test the individual susceptibility as evinced by the occurrence of physiological effects, or by the dose required to produce the greatest and most rapid improvement.

There are undoubtedly cases in which the iodide does not act satisfactorily but where its association with mercury renders its effects well marked. Thus, in cases of syphilis of old standing and great obstinacy, some, among whom we may mention Ricord, employ both these remedies at the same time. For this purpose, the mercury may be administered by the skin, by inunction or vapour-bath, and the iodine by the mouth; or both may be given in the form of a deuto-ioduret of the iodide of mercury, formed by combining in solution corrosive sublimate and a large preponderance of the iodide of potassium. M. Trousseau, again, recommends a different plan; he brings the patient gently under the influence of mercury, by giving calomel, after which he commences the employment of the iodide of potassium.

In most cases, two, three, or four months of such treatment removes all the symptoms of syphilis, both as affecting the nervous system and the frame generally; but it by no means follows that this has effected the complete extinction of the diathesis, so that the patient is free from all risk of any relapse. Experience proves that after the elapse of months, or even of years, a fresh explosion of the constitutional taint may occur. Nay, this may be observed in spite of the continued use of prophylactic remedies for months after every appreciable symptom had disappeared.

Without at all wishing to detract from the value of the remedies we have been discussing, it is well to recollect that syphilis left to itself often follows an intermittent course,—now appearing, and again disappearing, without the employment of any treatment; while, again, it is undoubted that, sometimes in spite of the most judicious treatment, a temporary alleviation is only produced, the disease running on from bad to worse till it results in a fatal issue.

It would certainly be difficult to account for this persistence of the disease were we to assume the popular idea of its being a poison which exists in the blood, but the view which has been developed in the German school, of its being a disease which has a local residence in the lymphatic system and blood-forming glands generally, could alone satisfactorily account for the intermittent manifestations of the disease, and the occurrence of anæmia at its commencement and at its close, while such a pathology would serve to establish an analogy between it and other constitutional affections. Besides the anti-syphilitic treatment, there is usually an urgent necessity for the employment of tonics, so as to do away as far as possible with the effects upon the system of the prolonged employment of mercury and iodide of potassium. For this purpose, iron and quinine will be found very essential accompaniments of these anti-syphilitic remedies.

Independent altogether of these measures calculated to affect the disease through the medium of the system, various therapeutic agencies may be requisite as adjuvants in treatment. Thus, in syphilitic neuralgia, the endermic application of morphia, or some local opiate application, may be required; in hysteria, occurring in the early stage of syphilis, although from anti-spasmodics, when used alone, little advantage will be gained, along with mercurials they prove of signal service; in paralytic affections, blisters, the use of the actual cautery over the spine, and galvanism applied to the palsied muscles, assist very materially in restoring movement to the paralyzed limbs; in cerebral affections, again, blisters to the nucha, dressed with mercurial ointment, purgatives, and, in exceptional cases, the application of leeches, may be needed; besides, warm bathing, vapour-baths, etc., by acting on the skin, tend materially to relieve internal symptoms. Cold bathing, the douche-bath, and various of the devices of the hydropathic practitioner may help to give tone to the whole frame, but more particularly to the stomach and bowels when they have become debilitated. In most cases, there is no need, on account of the cold bathing, to stop the use of the mercurials.

When the extreme effects of syphilis have occurred, when, in fact, the syphilitic cachexia is developed, we can hardly expect to have much success crowning our best efforts to obtain a cure. In such circumstances, both mercury and iodide of potassium would be badly borne. Tonic treatment, good food, cod-liver oil, and suitable hygiene, are the only means which can be expected to prolong the lives of such miserable victims marked out for a death which cannot be long delayed. P. H. W.

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*Researches on the Nature and Treatment of Diabetes.* By F. W. PAVY, M.D., Assistant Physician to, and Lecturer on Physiology at, Guy's Hospital. London: Churchill: 1862.

UNTIL the discovery by Bernard as to the glycogenic function of the liver, great obscurity involved the pathology of diabetes. Dr Mead believed that it depended upon a morbid state of the liver and bile; Cullen ascribed it to a fault in the assimilatory powers; Dr Darwin had a fanciful theory that it was due to a retrograde motion of the absorbent vessels from the lacteals into the lymphatics of the kidneys or bladder.[1] Latterly, however, Dr Prout's views were very generally entertained. That excellent chemist and physician believed that the cause of diabetes was a suspension or destruction of the function by which the saccharine principle formed in the stomach from alimentary matters is, in the normal condition, converted into oleaginous, albuminous, and possibly other matters.[2] The real cause of the disease was, however, still left in obscurity. The publication of Claude Bernard's researches threw an immediate light upon much that had previously been obscure. The discovery that the formation of sugar was a normal function of the liver, at once pointed to that organ as, in all probability, chiefly involved in the diseased process; while the fact of this function being under the direction of the nervous system showed that the disease had not necessarily its starting point in the hepatic tissue. Though this was a great step, it became evident that the pathology of the disease was exceedingly complicated. It might depend either upon an increased formation or a diminished destruction of sugar. In the latter case, owing to a morbid condition of the lungs, or some other cause, a portion of the saccharine materials, which should have been destroyed almost as soon as formed, escaped decomposition, accumulated in the blood, and was eliminated by various abnormal channels, in particular by the urine. In the former, the cause of the disease might be looked for in an abnormal condition of the central organs of the nervous system, in an increased irritation of the pulmonary branches of the pneumogastric nerve, in a functional or organic change in the hepatic tissue, or in an unnaturally stimulating property of the blood of the portal system. It therefore was, and still is, in any given case, a very difficult problem to determine on what particular morbid condition diabetes depends.

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[1] See Craigie's Practice of Physic, vol. ii. p. 1157.

[2] On Stomach and Renal Diseases, fifth edition, p. 15.

These remarks take for granted the correctness of Bernard's views as to the formation of sugar, founded on his well-known experimental researches. These researches professed to prove that sugar is not, under ordinary circumstances, to be found in the systemic circulation; that the blood of the portal vein does not naturally contain it; that it is to be detected in large quantity in the blood which leaves the liver by the hepatic vein; that it is present in the blood in the right side of the heart; that it disappears in the lungs, and is no longer to be discovered in the blood contained in the pulmonary veins. Dr Pavy, as we shall see presently, denies (upon experimental grounds) the accuracy of some of these statements.

Dr Pavy, as he informs us in his preface, has devoted special attention to the subject of diabetes for a number of years past. Having worked at the subject under Bernard, in Paris, he entered upon a series of investigations, with a view to determining the nature of the process by which sugar is destroyed in the lungs. In the course of these he was led to believe that the data from which he had started were erroneous, and that sugar was not, in the normal condition, formed in the liver in the way supposed by Bernard. Some of Dr Pavy's observations have already been published in the Philosophical Transactions, and in Guy's Hospital Reports; in the present volume the conclusions he has come to are arranged in a systematic form, with a special bearing upon the pathology and treatment of diabetes.

The first chapter treats of the mode of detecting sugar in the urine and elsewhere. For this purpose, both in qualitative and quantitative researches, Dr Pavy prefers the copper test, using a solution prepared according to the following formula:—Sulphate of copper, 320 grains; neutral tartrate of potash, 640 grains; potassa fusa, 1280 grains; distilled water, 20 fluid ounces. The tartrate of potash and caustic potash are to be dissolved in one portion of the water, and the sulphate of copper in another; the two solutions are then to be mixed. The liquid to be tested is mixed with a little of this solution, and boiled for a short time, when, if any grape-sugar be present, the protoxide of copper loses half of its oxygen.

and is thrown down as a yellow, orange-yellow, or orange-red precipitate. The test is also conveniently employed quantitatively, for the strength is such, that half a grain of grape-sugar exactly reduces the oxide contained in 100 minims of the solution. Dr Pavy much prefers this process to the fermentation test, which he has found neither so delicate nor so accurate as the cupro-potassic solution.

The next chapter is a very interesting one, and is entitled "The Physiological Relations of Sugar." In it Dr Pavy details his experiments, and states his conclusions regarding the formation of sugar in the animal body. We cannot enter fully into the subject, which occupies upwards of seventy pages in the original, but can merely give a brief abstract of the views propounded. With a single exception, Dr Pavy admits the accuracy of Bernard's experiments. The experiment, the accuracy of which he contests, is, however, an all-important one. Bernard, as already stated, maintains that the blood in the right side of the heart contains sugar, and established this both by examination of the blood of an animal recently killed, and by testing the blood removed from the right side of the living heart. Dr Pavy, again, denies that in the *living animal* sugar exists in the blood of the right side of the heart in a larger proportion than is to be found in the blood of the arterial and general venous system, and maintains that the presence of sugar in the liver, and in the blood of the hepatic vein, is a *post-mortem* phenomenon. He believes that, in health, little or no sugar is to be found in the liver, but that there exists in the hepatic cells a substance to which the term amyloid matter has been applied; that this substance, after death, and in certain unnatural circumstances, is readily converted into sugar, and that this speedy conversion after death has led to the erroneous conclusion that sugar was present during life. The following is, in Dr Pavy's words, a short outline of his views regarding the so-called glycogenic theory:—

"Evidence has been given that the so-called glycogenic matter is formed in the liver from sugar as one of its sources. Hence, if sugar were its natural destination, the process would be sugar into glycogene, and glycogene back again into sugar—a repetition that certainly appears, to say the least of it, extraordinary and improbable, as a designed occurrence of nature.

"The blood, under normal circumstances, only contains more or less evident traces of sugar; and that belonging to different parts of the system does not present, as far as I can discover, after adopting the necessary precautions in the examination, any appreciable variation in constitution. In some most carefully conducted examinations, I was unable to distinguish, by any difference in behaviour, the blood of the portal vein from that of the right side of the heart.

"The liver itself is found free from sugar, or, at the most, is only impregnated with it, to the minutest extent, when treated so as to come down upon it in a condition the nearest possible to that of life."

Again—"Whatever sugar may be present in the blood of the right side of the heart will, as far as can be shown by analysis, be found to the same extent after this blood has traversed the lungs and reached the arterial system. Hence, if the liver possessed a sugar-forming function in accordance with the glycogenic theory, the urine of us all would always present a strongly saccharine character. There not being the destruction of sugar going on in the lungs that was formerly supposed, it is apparent, that whatever sugar, no matter from what source, reaches the general circulation, must be distributed through the system with the arterial blood. Sugar would thus be circulating through the kidney; and, on account of its diffusibility, could not fail to escape and occasion a more or less saccharine state of the urine according to the existing saccharine quality of the blood."

It will now become apparent why such fundamental importance attaches to an accurate knowledge of the condition of the blood on the right side of the heart. If the result of Bernard's experiment be incorrect, his conclusions must, to a great extent, be set aside; if Dr Pavy's facts be erroneous, his theory at once falls to the ground. Between the two we shall not attempt to decide; it is purely a matter for experimental inquiry. While Bernard's precision and accuracy are well known, and while his results have been confirmed by others, Dr Pavy's careful and frequently repeated experiments are not to be lightly set aside, and we must wait till they have been either decidedly confirmed or refuted before expressing an opinion on the subject.

In the third chapter the pathology and treatment of diabetes are considered. Having discarded the glycogenic theory as to the physiological formation of sugar, Dr Pavy naturally declines to accept its pathological deductions, and expresses views not very different from those of the late Dr Prout. Like Dr Prout, Dr Pavy demurs to Liebig's view that sugar is capable of direct oxidation in the economy, or that, as sugar, it is capable of contributing directly to the maintenance of the animal heat. Dr Pavy believes that, in health, the sequence of events connected with the occurrence of sugar in the animal economy is the following:—Sugar, ingested as such, or derived from the metamorphosis of starch, enters the blood through the portal vessels by the physical principle of diffusion; hence it is carried to the liver, by the cells of which it is extracted from the blood, and converted into amyloid matter. If, however, too much sugar has found its way into the portal blood, the conversion is not complete, some sugar passes through the liver, enters the systemic circulation, and is discharged by the urine. All the amyloid substance, however, is not derived from saccharine matter, and it is probable that it is also produced by the metamorphosis of other kinds of food, as well as from some of the products resulting from the wear and tear of the tissues. The amyloid substance so obtained is not reconverted into sugar, but, in Dr Pavy's opinion, is changed into fat. In diabetes Dr Pavy believes that the liver is the organ at fault, yet the mode in which the morbid condition is established is still undetermined.

"Although there appear to be good grounds for believing that it is upon a defective action of the liver that the phenomena of diabetes depend, yet, when we come to speak of the circumstances determining this defective action we arrive at a point that, it must be admitted, still remains without a clue to its solution. If, however, experimental research has not yet disclosed the nature of the pathology of diabetes, I believe it has paved the way to a position from which the disease is more likely to be investigated with success. It is fair to infer that, as long as our notions upon the physiology of this subject were based on erroneous conclusions, no real advance was likely to be made in its pathology unless by some improbable fortuitous discovery.

"As to the cause of diabetes, I take it that in different cases different causes may be concerned in the production of the disease; just as we learn, from experiment, that there are various ways by which saccharine urine may be artificially occasioned."

There is one other point regarding the pathology of diabetes to which we must refer, and to which Dr Pavy alludes in the following words:—

"The most frequent mode of unfavourable termination of diabetes is by disease of the pulmonary organs. This disease of the lungs is commonly spoken of as phthisis; but, although it runs the same course, and presents the same symptoms as phthisis, yet, it seems in reality to consist of a simple chronic inflammation; leading to the breaking down of the lung tissue, and the formation of cavities; and produced by the state of the blood, and not by any true or strumous tubercular deposit. I have witnessed the examination of several cases of diabetes where death has resulted from lung disease, and there has been no sign of the existence of tubercle. Dr Wilks's experience is also to the effect that it is not the true tubercular deposit that is met with in the lungs of diabetics who have died from pulmonary disease. There is a deposit around the cavities which is often spoken of as tubercle, but which consists in reality of a simple inflammatory production."

We do not altogether agree with these statements. The deposit in the lungs in the cases alluded to presents all the physical and microscopic characters of tubercle; it runs the same course; and a very important point is, that the occurrence of tubercular ulceration in the intestines in these cases is just as frequent as in unequivocal cases of tubercular phthisis. In fact, the great frequency of disease of the lungs in cases of diabetes indicates more than a casual connexion between the two conditions, and seems to us to be rather in favour of the views of Bernard.

In the treatment of diabetes attention is, above all, to be directed to the diet. The great object is, to withdraw from the food the saccharine and farinaceous elements. The great difficulty consists in persuading patients to give up the use of bread, and of articles, such as vegetables, which, by filling the stomach, satisfy the craving appetite more completely than animal food. Gluten bread has been largely employed for this purpose of late years, but is not without disadvantages. It is very distasteful to some patients, and, from Dr Pavy's examinations, does not appear to be free from a considerable quantity of starch. Dr Pavy has been led to recommend the use of the sweet almond as a substitute for bread. It has no disagreeable taste, contains no starch, is nutritient and emollient, and, when thoroughly ground, is by no means indigestible. Its oily character is likewise a desirable quality as supplying respiratory food to the diabetic. The mode of preparation, and the result arrived at, are thus described:—

"Finding that, with eggs, a solid form of biscuit could be procured, I sought the assistance of Mr W. Hill, of 60 and 61 Bishopgate Street, E.C., London, an expert confectioner. To Mr Hill, jun., my thanks are due for having entered so fully as he has done into the spirit of the undertaking. Several difficulties had to be overcome. And, many trials of different plans were effected, the object held in view being to produce a material as closely resembling our ordinary wheaten food as possible. The only substances taken for use are, blanched almond powder, which has been washed, and eggs; so that the product is free from any objectionable principle.

"It is not for me to speak in praise of this almond food myself; but, thus much I may say, that Mr Hill has succeeded in producing a rusk and different forms of biscuit, which give me good cause to hope that the diabetic may find in this application of the almond a not unimportant nor unpalatable accession to the limited list of articles he is permitted to consume, without producing an aggravation of his complaint. And, there is this in the almond food, that the patient is supplied in his substitute for bread, with that which he can appropriate, and which his system is exactly in need of. It must not, of course, be expected that an article equally as relishable as bread can be produced. Growing up, as we do, from almost our earliest period of life, upon wheaten food, it is not likely that its place, certainly at first or until a second habit has been engendered, can be supplied with anything that will prove so agreeable for a continuance."

We have tried the almond-biscuit in one case, but not, we confess, with a very satisfactory result. The case, however, was an exceedingly obstinate one, and proved quite unamenable to any form of treatment. The biscuit itself is by no means unpalatable, and, in this instance, was relished by the patient.

Dr Pavy has little confidence in any of the remedial agents which have been recommended for the cure of diabetes; though, by a careful attention to the state of the general system, much may be done to relieve the symptoms, and to maintain the patient in a very tolerable degree of strength and comfort.

In conclusion, we can recommend Dr Pavy's work as a careful and conscientious performance; and, should its author's statements be confirmed, will prove an important contribution to our physiological and pathological knowledge. In any case, it is highly creditable to him, as containing the result of much labour, and minute observation, on an intricate and important subject.

## *A Practical Guide to the Study of Diseases of the Eye.* By HENRY W. WILLIAMS, M.D. Boston, U.S.: Ticknor and Fields: 1862.

IN literature and in life, in our estimate of books or men, we are, virtually, if not confessedly, led to ask two questions regarding the author or the hero; *first*, at what eminence of erudition or success did he aim, by what standard of excellence does he wish critics or posterity to judge him; and, *secondly*, how far has he in his work or his life attained to the eminence or approached the standard.

In the present work, the author, as he tells us in his preface, does not assume to set forth all that is known in respect to diseases of the eye. His aim has been to afford to students and junior practitioners, who may not have had special opportunities for such studies, a practical introduction to diseases of the eye and their treatment, in a form as simple and concise as possible. Judged by his own modest standard, Dr Williams has performed his task well.

The arrangement of the work is simple, the descriptions short, clear, and practical, the treatment sagacious, and, as a rule, guided by ordinary surgical principles, without the fault (too common, especially in the older works) of describing and treating diseases of the eye as if their surgical pathology were a strangely special branch.

After a brief introduction on the method of examination, and the character of the symptoms most important in diagnosis, there follows a short, very short, chapter on the ophthalmoscope and its use. Indeed, throughout the whole work, we might have wished that more reference had been made to the information given us by the ophthalmoscope, as an adjuvant to diagnosis in many of the diseases of the deeper textures.

In the chapter on Remedies and their application, our author expresses his opinion most strongly against two of the most time-honoured and favourite articles in the oculist's pharmacopœia. With considerable justice, he insists that solutions of the acetate of lead should be banished from the list of ophthalmic remedies, as other mineral astringents exist at least equal to it in value, and which do not, as it does, tend to decompose and deposit on corneal ulcers, and thus form indelible corneal opacities.

With rather less reason, however, he makes nearly as fierce an onslaught on the good old nitrate of silver, which he thinks can be in great measure superseded by the use of crayons of sulphate of copper applied very gently to the inflamed eye. It is true, doubtless, that the nitrate of silver used for too long a time may give rise to disagreeable staining of the conjunctiva, and therefore it should not be given to patients to use at their own discretion without proper warning; still, under proper superintendence it proves a remedy for which, both as regards convenience and safety, it will be hard to find a substitute.

The account of the pathology of keratitis is meagre and partly incorrect; the interstitial cloudiness and thickening between the corneal laminæ being described as owing to deposits of lymph, instead of an increase in the quantity and cloudiness of the contents of the spindle-shaped anastomosing cells which constitute the corneal structure, as has lately been proved by the researches of Virchow and Von Graefe.

At page 120, in an otherwise excellent short comparison of the relative advantages of complete or partial excision of the eyeball in extensive staphyloma, one important element has been omitted,—the condition of the other eye. Should much sympathetic irritation exist in the sound eye, remove the *whole* diseased eyeball, and the wound will heal sooner, and the other eye have a better chance; but if the other eye is not much irritated, remove only the *anterior half* of the diseased eyeball, as a better stump for the subsequent adjustment of an artificial eye will be left, though healing of the wound will be less rapid.

The most important practical innovation in this work, and the one which certainly will excite most criticism, is our author's treatment of iritis *without mercury!*

Unfortunately, no exact statistics are given, but Dr Williams, as the result of lengthened and copious experience, tells us that he is confident that cases properly treated get on quite as well, if not better, without mercury than with it; that in cases where lymph has been effused, blocking up the pupil, he has frequently seen this obliteration continue, notwithstanding the lavish, and what in the language of mercurialists would be called the judicious use of mercury; and, again, that in as many cases he has seen absorption of the lymph result under the administration of tonics only. The chief points to be attended to in the "proper treatment" which is to enable us to dispense with mercury are, the free use of atropia to dilate the pupil, and of opium to control the pain. We are not told what is to be done in cases where the thickening and adhesive changes in the iris have gone so far before we see the patient as to render the atropia powerless for good or harm.

This treatment of iritis without mercury by Dr Williams has long been known in Europe: the results are very striking, and if verified by similar results in the practice of others, will go far to upset one of the oldest therapeutical dogmata, and to wrest another stronghold from the now rapidly lessening domain of mercurials. We must not forget, however, that some of our transatlantic brethren who have witnessed Dr Williams' treatment have not been so satisfied with his results, but state that the average duration of the cases was longer, and the result less perfect under the new than under the old system. Even Dr Williams allows that in cases of well-marked syphilitic iritis, complicated with interstitial corneitis, the use of mercurials, especially of the iodide of mercury, is admissible, if not almost absolutely necessary.

In his account of the operation for artificial pupil, Dr Williams recommends in some cases the separation or laceration of the iris from its ciliary attachment (corodialysis of Mackenzie), an operation which most British ophthalmologists regard as "so clumsy, unscientific, and ineffectual" as to deserve "speedily to be consigned to the limbo of obsolete surgery."

Again, when describing the various operations for cataract, we are surprised to find our author recommending the utterly barbarous and unscientific operation of reclination.

He does qualify this recommendation by saying that it is admissible only in cases where the patient is unable to leave his home, and is obliged to put himself under the care of surgeons of small experience and unpractised hands. Should we grant, only for the sake of argument, that such practitioners exist on either side of the Atlantic, we must say that (putting ourselves in the *patient's* place) we should be inclined to doubt whether *any* operative interference with such a delicate organ as the eye would be safe, and would rather endeavour, by strict economy or petty larceny, to obtain dollars enough either to carry us to Boston, or bring Dr Williams to our home.

On the whole, this work will well repay perusal. It contains very many hints as to treatment which no one but an experienced and sagacious surgeon could give, and is written in an easy style, with very few idiomatic peculiarities.

The paper and printing of the work are first-rate, being far above the average products of the American medical press.

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*Pathological and Practical Observations on Diseases of the Abdomen, comprising those of the Stomach, and other parts of the Alimentary Canal, Œsophagus, Cæcum, Intestines, and Peritoneum.* By S. O. HABERSHON, M.D., Senior Assistant-Physician to Guy's Hospital, etc., etc. Second Edition. London: Churchill: 1862.

THIS is an improved and enlarged edition of Dr Habershon's work on the diseases of the alimentary canal; the fact of its having already gone through a first edition is proof that such a work was wanting, and that the author's endeavours to supply it have been appreciated.

Beginning with the œsophagus, Dr Habershon considers in succession the different parts of the alimentary canal; the various morbid conditions of each is treated of, and a very large number (235) of illustrative cases is introduced. These cases, in fact, form the basis of the work, and though individually a great proportion are of much interest, and the groups into which many of these are classified are perfectly natural, the mode in which they have been brought together detracts considerably from their value as clinical illustrations of abdominal disease. Many of the cases detailed were observed during life by Dr Habershon; but in numerous instances he did not see the patients while alive, and it was in his capacity of demonstrator of morbid anatomy that he became acquainted with the lesions they presented, previous to having any knowledge of the history of the case. In many instances indeed the physician who had had charge of the patient supplied him with information as to the symptoms presented during life; still, such a mode of drawing up cases, is never satisfactory; in the great majority of instances the information so obtained is defective, almost valueless for clinical purposes, and only sufficient to throw a modified amount of light on the morbid appearances found. No doubt in a few instances the history of the cases supplied is full and ample; but these are exceptional, and the meagreness of the majority contrasts very unfavourably with the fulness and accuracy of others which had been under Dr Habershon's own observation. For instance, what information does the following history (selected at random) give in a clinical point of view:—"Thomas C., æt. 43, was admitted into Guy's Hospital, December 7th, 1853, and died December 31st. By trade he was a sailmaker, and during his life had been very intemperate. He was admitted with anasarca, and coagulable urine; diarrhœa and wasting came on, and before death he passed into a semi-comatose condition. *Inspection* was made forty-seven hours after death." At the inspection, a portion of the lower part of the ileum was found in a sloughy condition; there was general peritonitis; the kidneys were diseased; the left lobe of the liver was degenerated; and there was lobular pneumonia. But what light does the case throw on the symptomatology of enteritis? This is our main objection to Dr Habershon's book; it is written too much from a pathological, too little from a clinical point of view. Many of the cases are only important as descriptions of morbid specimens.

In some cases, and with great advantage, Dr Habershon departs from his usual mode of arrangement, and treats of morbid conditions from a physiological point of view; thus, under the morbid conditions of the œsophagus, he considers dysphagia, and illustrates the various circumstances upon which it may depend; under the heading "functional diseases of the stomach," he treats of the causation and diagnostic importance of dyspepsia, hæmatemesis, pain, and vomiting. While we have felt bound to make these remarks regarding what we cannot but consider a mistake in the original planning of the work, we willingly admit that it contains much valuable matter, and presents the results of careful and accurate observation.

A new chapter on peritonitis has been introduced into this volume. Dr Habershon denies that this disease has a local character, unless excited by injury of the serous membrane or by the direct propagation of disease. "In relation to its causes," he states, "cases of peritonitis may be divided into three classes: 1. Peritonitis produced by the extension of disease from adjoining viscera, or excited by direct injury, including cases of perforation of viscera, extravasation, violence, etc. 2. Peritonitis connected with blood changes, as when inflammation of the serous membrane occurs in the course of albuminuria, pyæmia, puerperal fever, erysipelas, etc. 3. Peritonitis caused by general nutritive changes of the system, which have been followed by acute or chronic disease of the peritoneum, such as struma, cancer, etc.; and comprising also those cases in which the circulation of the peritoneum has been so altered by continued hyperæmia (modifying its state of growth), that very slight exciting causes suffice to induce acute mischief, as occurs in peritonitis with cirrhosis, disease of the heart, etc."

With these remarks we generally agree, although we have met with a few cases, where, in the absence of any other cause, we have felt constrained to regard the peritonitis as a local affection. Dr Habershon's remarks on the treatment of this disease are judicious; he trusts chiefly to opium, anodynes, or counter-irritants externally, and to the occasional administration of gentle enemata. Mercury in any form he disapproves of, and concludes his observations regarding it by saying, "We are well aware that many instances of acute peritonitis from diseased cæcum, from enteritis, and from ovarian disease, recover after mercury has been given; but as far as the causes we have enumerated can be any guide, and from extensive experience in these cases, we strongly deprecate its use."

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*The Pathology and Treatment of Phlegmasia Dolens, as deduced from Clinical and Physiological Researches.* By F. W. MACKENZIE, M.D., etc. London: Churchill: 1862.

THIS work is a publication in a separate form of the three Lettsomian lectures on Midwifery delivered by Dr Mackenzie to the Medical Society of London (of which Society he is vice-president) during the session 1861-62.

The first lecture includes a short historical account of phlegmasia dolens, as we find notices of it and theories regarding its pathology in the works of Mauriceau, Puzos, Levret, White of Manchester, Trye of Gloucester, Ferrier, Hull, Davis, and Robert Lee.

The author then briefly states the conclusions at which he has arrived, and which he endeavours to establish in the lectures. These are:—

1. Crural phlebitis, in a pure and uncomplicated form, cannot give rise to all the local and general phenomena of the disease, and therefore cannot be its proximate cause.

2. Phlebitis itself is, for the most part, not a primary but a secondary affection; and in the great majority of cases is a consequence of the circulation of impure or morbid blood in the veins.

3. The proximate cause of the disease is therefore presumably a morbific condition of the blood, which I have experimentally shown to be capable of producing not only the lesions of the veins met with in the disease, but all its other phenomena.

The clinical history of the disease, including its causes, symptoms, and morbid anatomy, are now investigated by an analysis of 60 cases of puerperal phlegmasia dolens. In 33 out of the 60, the phlegmasia dolens was preceded by an attack of puerperal fever. The causes (known or suspected) of these fevers are made the subject of careful analysis. The causes of the cases, which were apparently unconnected with puerperal fever, are next examined, and from the great variety of the modes of origin, the deduction is drawn of the probability, at least, of the "existence of some common link or medium" which is sought "in an abnormal condition of the blood."

A distinction is next drawn, and this Dr Mackenzie claims as original, between the *simple* and the *complicated* forms of the disease. The *simple* includes four sub-divisions,—the puerperal, the anæmic, the rheumatic, and the gastric, from their causation and special symptoms. The *complicated* includes endless varieties, according to the nature of their precursory or coincident symptoms.

The lecture closes with an account of the morbid anatomy "*special*" of the limb, "*general*" of other regions.

Lecture II. is devoted to the consideration of the subject from its physiological aspect, and specially of the author's own experiments on *crural phlebitis*, which he selected as being the anatomical lesion of the affected limb most frequently present. From these experiments, which are more fully detailed in the thirty-sixth volume of the Medico-Chirurgical Transactions, two conclusions are drawn:—

1. That a local phlebitis may be produced as the result of a local cause, which will have the general phenomena of phlebitis, and yet none of the phenomena of phlegmasia dolens.

2. That, by vitiating the general mass of the blood, phlebitis may be produced; in which case, however, are superadded all the phenomena of phlegmasia dolens.

From these, again, the conclusion is drawn of the blood-origin of the disease.

Lecture III. contains the practical deductions regarding,—1, Preventive treatment,—*local*, consisting especially in the removal of urinary and fæcal retention, and avoidance of intestinal, mammary, spinal, or pelvic irritations; *constitutional*, by hygiene and dietetics; 2, The curative treatment of the sthenic and asthenic types respectively.

Though there are many points (in the pathological section especially) on which we cannot agree with Dr Mackenzie, and though some of his conclusions are, we think, based upon insufficient premises, still this book is a true monograph, not a mere treatise hurriedly put together for a special purpose, but the deliberate opinion of one who, to great practical experience, has added much thought and many original experiments.

*The Microscope and its Revelations.* By WILLIAM B. CARPENTER, M.D., etc., etc. Third Edition, illustrated by ten plates and nearly four hundred wood engravings. London: Churchill: 1862.

WE are informed in the preface that five thousand copies of this manual have already been sold, but this success has not prevented the author from carefully revising and partially rewriting it. As it now stands it forms an admirable practical guide to the microscopist, describing minutely the structure and the mode of using his instrument with its accessories, and entering into a full account of the most interesting subjects for examination from the various kingdoms of nature. Like all Mr Churchill's manuals this work is carefully got up, and profusely illustrated.

# Part Third.

# PERISCOPE.

## MIDWIFERY.

### DISCUSSION ON OVARIOTOMY.

AT a meeting of the Royal Medico-Chirurgical Society, held on Tuesday, 9th December, a paper was read by Mr Spencer Wells, entitled "History and Progress of Ovariotomy in Great Britain; with Observations founded on Personal Experience of the Operation in fifty Cases."

The author commenced by proving that ovariotomy is an operation of British origin. It was first suggested by William Hunter, was warmly advocated by John Bell, and was first practised by Dr M'Dowell, an American pupil of John Bell. He then proved that its subsequent progress is chiefly due to the labours of British Surgeons. He traced the progress of the operation from its first performance in Scotland by Mr Lizars, in 1823, Dr Granville's attempts in London in 1827, and the first sucessful case in England in 1836, by Mr Jeaffreson, of Framlingham, to several other successful cases by provincial surgeons in that and the two following years. Mr Morgan's attempted operation in 1839, and Mr Phillip's completion of the operation for the first time in London in 1840, were also noticed, as well as the unsuccessful cases of Mr Key and Mr B. Cooper at Guy's in 1843, and the commencement, in 1842, of Dr Clay's long series of operations. This review showed that ovariotomy had never been successfully performed in London twenty years ago, although at least ten successful cases had been recorded by provincial surgeons. The successful cases in London, in 1842 and 1843, by Mr Walne, Dr F. Bird, and Mr Lane, were next noticed, and the first successful case in a large London Hospital, at St George's, by Mr Cæsar Hawkins, in 1846, as well as many other cases in the provinces in the four following years. Mr Duffin's case in 1850, by which he showed the importance of keeping the strangulated portion of the peduncle outside the peritoneal cavity, was alluded to as the inauguration of a new era in ovariotomy. The effect of the various papers published in former volumes of the *Transactions*, and the discussions which had taken place in the Society, were then shown to have had an unfavourable effect upon the progress of the operation, as did a number of unsuccessful cases which occurred between 1852 and 1856; so that when the author began to operate in 1858, ovariotomy was at a very low ebb in professional opinion. After alluding to the introduction of the clamp by Mr Hutchinson, and his own modifications of the details of the operation, especially with regard to the means of securing the pedicle, closing the wound, avoiding the abuse of opium, and supplying fresh air to the patient instead of hot vapour, the author gave the results of his whole experience of the operation in an elaborate table, of which the following is a brief abstract:—

Series I.—In which ovariotomy was completed—50 cases: 33 recoveries; 17 deaths.

Series II.—In which ovariotomy was commenced, but not completed—3 cases. No death from the operation.

Series III.—In which an exploratory incision was made in aid of diagnosis—3 cases. 1 death.

He then attempted to correct some errors which tended to retard the progress of the operation, showing that many of the alleged difficulties of diagnosis were easily overcome: and that, as after other capital operations, the better the general health of the patient, and the smaller the injury that is done in the removal of any diseased part, the greater is the probability of success. He proved that when patients recovered they were restored to perfect health, and lived many years, and had born children of both sexes. In conclusion, he left the Society to determine "whether an operation which has led to such results is still to be stigmatized as unjustifiable; whether they who perform it are necessarily open to the reproach that they do so rather to serve their own selfish purposes than for the good of their patients; whether they who, in the face of evidence sufficient to convince any unprejudiced mind, withhold from their patients a tried and approved mode of curing a disease otherwise incurable and certainly fatal, are not open to still more serious reproach; and whether it does not become us—as men of science, who practise our art, not for our own advantage only, but with the earnest desire to do the very best that can be done for those who are confided to our care, and who trust in our knowledge, our skill, and our honour—no longer to condemn and oppose this operation, but rather to study its past history; to regard it with pride, as an offspring of British genius cultivated by British industry; and to assist its future progress by perfecting our means of diagnosis, and, by investigating the avoidable and removable causes of excessive mortality, reduce it to the comparatively low proportion to which it may be, and will be, reduced, and thus render ovariotomy, in each coming year, more honourable to British surgery, and more useful to mankind."

*Dr Graily Hewitt* remarked that, in common with others interested in the treatment of ovarian disease, he had paid some attention to the subject, with a view of arriving at a conclusion on the important question of the performance of ovariotomy, and he begged to submit some results of his inquiry into the matter. It always appeared to him that the first and weightiest question to decide in reference to the matter was the following one:—What is the natural history of ovarian disease such as would be likely to be considered suitable for operation by ovariotomy, in cases where no such procedure is attempted? Upon the answer to this question turns the whole decision; and the question must be answered, seeing that the argument, that such and such cases would not have ended fatally if they had been left alone, is perpetually made use of by those opposed to the operation. It is evident that this question has been too little considered. Very few attempts have been made to arrive at some definite conclusion on this point. The difficulties involved in procuring the desired information are very considerable, and it can only be in the cases of practitioners of long standing and experience, and who have been, moreover, in the habit of recording their experience, that it can be obtained. Even then the results procured are numerically limited, from the fact, that many cases must pass under observation for a time only, and the record as to the mode of termination of the case is wanting. He had been able to procure but few data on the subject; but such as they were he would now submit. In a valuable series of Clinical Reports on Diseases of the Ovaries, published nine years ago, Dr Lee, a much respected Fellow of the Society, had related his experience, and an examination of Dr Lee's cases gave the following results:—Out of a much larger number of cases of presumed disease of the ovaries, there were 44 complete cases, respecting which the account given left no doubt that the cases were such as could be fairly compared with those which, by individuals in favour of ovariotomy, would be thought proper cases for the operation: that is to say, they were cases in which, from the size of the tumour, its physical qualities, such as fluctuation, the fact of tapping having been performed, or some other distinctive characteristic, it could be stated that they were cases of progressive ovarian disease. Cases were necessarily omitted in which the result of the case was not given, or in which the tumour was so small that the question as to ovariotomy would not have arisen. Now, the data afforded by these 44 cases, as to the natural history of such disease, gave the result, that in 32 instances a positively fatal termination occurred, the cases being left to ordinary palliative treatment, tapping, etc. In 1 case the inference is that the patient died, in 1 the inference is doubtful, in 2 the patients were apparently dying, and in 1 other case the disease was "proceeding rapidly to a fatal termination." On the other hand, in 1 case the disease had not reappeared after a tapping 26 years previous, in 1 little progress had been made in 3 years, in 2 the patients were alive after a period respectively of 2 and 3 years, in 2 death took place from ovariotomy, in 1 death occurred from an exploratory puncture. Thus, the 44 cases are divisible into two series: 84 per cent. of the cases died, no radical measures having been attempted, and 16 per cent. lived (including the cases of ovariotomy and exploratory puncture, which might, perhaps, more fairly be put into the other series) for a period of 2 or 3 years, so far as the report shows, and in 1 case for a period of 26 years. So far as he (Dr Graily Hewitt) had found, there were no data afforded by other observers so extensive as those of Dr Lee's. Of the 84 per cent. of cases with a fatal result, that result ensued, so far as can be judged from the report, within an average of a year and three-quarters. So far for the natural history of progressive ovarian disease. Next, with reference to the results of ovariotomy. The experience of the last few years, which only could be fairly taken as representing what could be done with ovariotomy, showed a favourable result of 68 per cent., 66 per cent., 70 per cent., figures so high as this, in the results of those who had operated most largely. Comparing, therefore, the results of the palliative and of the radical treatment, so far as the imperfect data belonging to the former class of cases allow us to do, it appears that the figures stand in the proportion of 16 to 60 or even 70. He would repeat that this was a most important point to consider in deciding for or against the treatment of ovarian disease by ovariotomy, the question which must be determined in each case being—What are the chances for the patient of surviving the effects of the disease when palliative measures only are adopted, as compared with the chances afforded by ovariotomy? He quite agreed with the author of the paper as to the great improvement that had been made in the diagnosis of cases, and its bearing on the favourable or unfavourable issue of cases. One error in diagnosis which had been of late more frequently committed than any other, was that of mistaking fibrous solid tumours of the uterus or ovary for disease of the more ordinary kind. It would be generally possible to avoid this by considering the history of the case. Large fibrous tumours, whether of the uterus or ovary, are of slow growth, as compared with tumours more properly suitable for ovariotomy.

*Mr Erichsen* said he felt sure that all must have heard with deep interest the very valuable paper that had just been read. The results detailed in it not only reflected honour upon the author, but upon British surgery, in which ovariotomy undoubtedly originated. Dr Graily Hewitt had related the lamentable consequences that result from leaving an ovarian tumour to take its own course, and he (Mr Erichsen) knew nothing more distressing than to witness the misery usually entailed by the gradual and progressive development of such a disease. What resource did surgery offer for its cure except ovariotomy? Medicine had no effect upon a tumour that was an independent organism developing in the interior of the body. The minor surgical means, such as iodine injections, were more fatal than the extirpation of the growth. The opponents of ovariotomy were bound to answer this question—"If you reject that operation, what have you to substitute for it?" But Mr Wells had pointedly put it to this Society to state whether ovariotomy was a justifiable operation or not, and this question ought to be answered here. Now, how are we to judge whether an operation is justifiable or not? By these tests,—by the consequences of the disease being left to itself, by the mortality attending it, and by the result following its successful performance. They had just heard the lamentable consequences of leaving ovarian disease to run its own course, and it was known that medicine was inoperative in checking it. Now, as to the mortality after ovariotomy. Several years ago, before ovariotomy had attained that degree of perfection and of comparative safety which improved methods of diagnosis and of operative manipulation have now given to it, Dr Layman, of New York, published a most able essay, containing an analysis of 300 operations performed up to that time; of these, the mortality was about 42 per cent. Since that period ovariotomy had greatly improved in safety, as was evidenced by the results of the operations of Mr Wells, Dr Tyler Smith, and others. The mortality now was probably not 30 per cent. Let this for a moment be compared with what occurred in other operations, which no surgeon hesitated for an instant to undertake. It was old and trodden ground to compare it with the result of the operations for hernia, ligature of arteries, etc.; and in these cases also the comparison was scarcely fair, as these were operations of necessity, whilst ovariotomy was an operation of expediency, and not of immediate and imperative necessity. But compare it with "amputations of expediency" of the lower extremity. He (Mr Erichsen) would take for this purpose the statistics of a most able paper, published two years ago in the *Transactions* of this Society, giving the results of amputations performed in one of the largest hospitals in London—Guy's,—where it was known, not only that the patients had the benefit of the most eminent professional skill, but all the appliances that wealth could afford for their recovery. In that very able paper, Mr Bryant stated that the mortality after amputation of the lower extremity for tumours was 36 per cent., and the mortality after "amputations of expediency" of the leg was 66 per cent. Compare this result of amputations performed under the most favourable circumstances, by men of the greatest skill and judgment, with those of ovariotomy, and the advantageous position of the operation would be at once seen. Then take the next point—the result of ovariotomy as compared with other operations for tumours, say of the breast. No surgeon hesitated to operate in cases of cancer of the breast, but he knew that the result was most unsatisfactory—certain, and probably speedy, recurrence; and he watched his patient with anxiety for the earliest signs of that return of the disease which he knew must inevitably occur. But how was it with ovariotomy? Why truly—

> "Aut cita mors venit aut victoria læta."

If the patient survived the operation, her recovery was complete and permanent. There were, doubtless, a few exceptional cases, in which the disease might return in the other ovary, but these were so few as scarcely to deserve being taken into account. In the great majority of cases the patient was at once and permanently cured, withour fear of recurrence. Considering, therefore, ovariotomy from all these points of view, it must be pronounced as a proper and a justifiable operation. There were some very important considerations connected with the diagnosis of ovarian tumours, and their conditions as affecting the probable result of operation, which deserved to be carefully studied. These were, the effect of concomitant dropsical effusions, of adhesions, and the influence exercised by the age of the patient on the operation. Errors had, no doubt, been made in the diagnosis of ovarian as in that of all other tumours, external or internal; but these were much less likely to occur, and did actually occur with much less frequency, at the present day, when the question of operation had perfected the diagnosis, than happened formerly, when so accurate a knowledge of the nature and conditions of these tumours was not necessary. With regard to concomitant constitutional mischief, as influencing the propriety of an operation, the surgeon would, of course, be guided by those considerations that influenced him in all cases. But there were two special complications which were of peculiar importance; one was ascites, and the other anasarca of the general cellular tissue. Ascites appeared to him (Mr Erichsen) always to be a serious complication, and he did not much like to operate in such cases. Anasarca had been shown by Dr Clay to be a condition unfavourable to operation, and in this he fully agreed. Then, as to adhesions. These, undoubtedly, were a serious obstacle to the success of an operation. They were an evil in two ways: first, by prolonging the operation, and sometimes causing great difficulties in separating the tumour from neighbouring organs. It was in this way that the intestine had been ruptured, portions of the liver torn away, the peritoneum stripped up from the abdominal wall, and other serious and fatal mischief induced. But adhesions were dangerous in other ways. They often contained bloodvessels which could not retract in the dense substance of the adhesion, and though they did not bleed much at the time of their division, did so afterwards, slowly oozing into the abdominal cavity, and giving rise to dangerous and fatal hæmorrhage. But adhesions were not an unmixed evil. When extensive, they seemed to obliterate the peritoneal cavity, to destroy the serous membrane as such, and to render it less liable to take on inflammatory action than when it was in its normal state. In this respect the peritoneum resembled the synovial membrane of joints. Just as we often see, that when the knee has been chronically inflamed, its synovial membrane destroyed and replaced by fibroid tissues, a great amount of mechanical violence in the way of forcible extension and flexion may be inflicted upon it without exciting inflammation in it; so in the peritoneum, when that has been coated with old adhesions and organised plastic matter, inflammation is little liable to occur. Adhesions, therefore, were very dangerous and troublesome at the time of the operation itself, but rendered the patient less liable to the after-danger of peritonitis. Another important element in the success of the operation was the age of the patient. So far as his experience went, it was entirely in accordance with the deductions made by Dr Lyman from his table of cases, that the operation was much more dangerous in young than in elderly women. Although not contraindicated by early age, it must be looked upon with more anxiety in young women from eighteen to twenty-five than in women beyond the middle age. The most successful age for its performance was after the cessation of the menstrual function; and every surgeon who had had experience in the operation must have been struck by the great success often occurring in apparently most unpromising cases,—in women between fifty and sixty years of age. There were many other questions of the highest importance in connexion with this operation into which time would not allow him to enter; but on reviewing the whole subject, he (Mr Erichsen) thought that the Society must reverse the decision to which it came ten or twelve years ago, and, instead of condemning ovariotomy, give that operation its hearty approval and justification.

*Mr Hutchinson* stated that he thought Mr Wells deserved much credit for prominently bringing forward his facts respecting ovariotomy. He hoped that one great point would be gained by the recent progress in professional favour which this operation had made. In the future we might expect that patients would be operated on at much earlier periods than had hitherto been the case. To this, more than to any other change, he looked for an improvement in ovariotomy statistics. He agreed most fully with Mr Erichsen in considering that the operation was a perfectly legitimate one. He had himself performed it in seven cases, with a result of four recoveries and three deaths. In an eighth case he had operated only on the afternoon of the day of meeting, and, therefore, could not include it. Now, as he had included several very bad cases in those operated on—one, especially, in which Mr Erichsen assisted him, which was at a time almost beyond hope,—he considered four recoveries a very satisfactory result. He could fully confirm the statement of Mr Wells, that when patients recovered they usually regained very excellent health, and were permanently restored to full enjoyment of life. As he did not believe that adhesions were of the slightest possible advantage, but, on the contrary, considered that their influence on the hopefulness of a case were entirely on the other side, he held it to be very desirable that cases should be operated on early, before they had had time to form. In thanking Mr Wells for the support which his facts gave to ovariotomists, he begged also to thank, not less warmly, Dr Robert Lee, since he considered that the paper recently read by him, in which, for the first time, statistics were ignored by an opponent of the operation, was the greatest triumph which its advocates had yet achieved.

*Mr Spencer Wells* said that the importance of the point raised by Dr Graily Hewitt could hardly be exaggerated. No surgeon could be justified in performing any operation necessarily attended by serious risk to life, unless life was seriously threatened by the disease. If an ovarian tumour was not very large, or was not growing fast, no one would think of advising ovariotomy. It could only be justifiable when other treatment was useless, and life was threatened at no very distant period. But all experience showed that—putting aside some rare exceptional cases of women who lived for very many years with or without occasional tapping—it was rare for a patient to live for two years after an ovarian tumour had attained such a size as to raise the question of ovariotomy. He had stated his grounds for this belief in a paper read before the British Medical Association at Canterbury, and it was very interesting to find that Dr Hewitt's analysis of Dr Lee's own cases more than confirmed the accuracy of his estimate of the duration of life under palliative treatment. He did not agree with Dr Hewitt in regarding fluctuation as being necessarily found in progressive ovarian tumours. He had seen many cases of such tumours, which grew very rapidly, and yet fluctuation could not be detected; especially if there was much fat in the abdominal wall. Nor did he think that it could be called a mistake if a surgeon removed a tumour, although he did not feel certain whether it would prove to be uterine or ovarian. The great questions were—"Is it movable, and can it be removed?" In two of his cases great doubt had been felt before the operation. One of them had been alluded to in the paper. The other patient had been examined by at least twenty gentlemen of great experience, and about half thought the tumour was uterine, and the other half ovarian. He himself felt great doubt, after repeated examinations; and though, at the time of the operation, he was inclined to think it was ovarian, he would not have been surprised if it had proved to be a peritoneal fibrous outgrowth from the uterus. Had it proved to be so, it would have been removed quite as easily, and probably quite as safely as it was, though the patient made an excellent recovery. Provided the uterus and the tumour could be moved independently of each other, he thought the surgeon need not be deterred from operating, by any doubt as to whether the tumour were uterine or ovarian; and he certainly could not be accused of a mistake if, before his operation, he felt the impossibility of being positively certain as to his diagnosis. Mr Erichsen's support of the principle of the operation by comparing the mortality with that of other recognised operations was most valuable. With regard to anasarca, he had been led to regard it as an unfavourable sign, and had noticed that many of the patients, in whom it had been present, had done badly; but he thought we were arriving at a sort of law that, when it merely depended upon the pressure of the tumour retarding the return of blood from the lower limbs, it was of no more importance than in pregnancy. But, when it depended on disease of liver, or spleen, or kidneys, or heart, or on leukæmia, then it should lead the surgeon to consider the case as unfit for operation. So with ascites. If it depended on disease of the liver, kidneys, or spleen, or on chronic disease of the peritoneum, the case was very unfit for operation. But if no such disease could be detected, and the dropsy seemed to be caused by the mere pressure of the tumour, or by its movements mechanically irritating the peritoneum, the effusion ceased as soon as the tumour was removed. He had seen several such cases do very well. In one there were fifty-seven pints of ascitic fluid, and in another forty pints. He could quite confirm Mr Erichsen's statement, that patients above 50 years of age recovered remarkably well. He had operated on seven patients between the ages of fifty and sixty, and only one died. But he did not think young women unfavourable subjects for the operation. He had twice operated successfully on girls of seventeen, and of fifteen cases between twenty and thirty, twelve had recovered. Between forty and fifty, of eleven cases, seven had recovered. But, between thirty and forty, of fifteen cases, only six had recovered. They seemed to do better before thirty or after forty, than between those ages. Whether this was accidental, and would be corrected by a larger number of cases, he could not say. He quite agreed with Mr Hutchinson's remarks as to adhesions, although in many cases very extensive adhesions had not appeared to have any retarding influence upon recovery; and he concluded by thanking the Society for the attention which had been paid to the paper.—*Medical Times and Gazette.*

# Part Fourth.

## MEDICAL NEWS.

### MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLII., 1862–63. MEETING I.

*Wednesday*, 19*th November* 1862.—JAMES SPENCE, Esq., President of the Society, in the Chair.

### I. MEDULLARY DISEASE OF THE TESTICLE.

*Mr Spence* showed a testicle which he had removed on account of medullary disease. The patient was a young man of twenty-three or twenty-four years of age, and there was this peculiarity, that the disease had been developed much more slowly than is generally found to be the case. Eight or nine years ago the patient noticed a hard swelling of one testicle, which, however, till recently, attracted comparatively little attention; it scarcely increased in size, caused no pain, but was merely the seat of a little uneasiness. Five or six months ago the tumour began to enlarge rapidly, and when the young man came under Mr Spence's care it presented all the characters of a firm medullary growth. The general health of the patient was pretty good, but there was a peculiar yellowness of the skin, which at once suggested the idea of malignant disease. As the disease seemed to be quite limited to the testicle, that organ was removed; and on section the tumour was found to present an admirable specimen of nodulated medullary sarcoma.

### II. ALTERATION OF THE NECK AND HEAD OF THE THIGH-BONES IN RHEUMATIC DISEASE.

*Dr John Struthers* showed a pair of thigh-bones from the dissecting-room, the upper ends of which were greatly altered in form. The shaft and trochanters were unaffected, but the neck was extremely short, and the head flattened and expanded. The acetabula were correspondingly large, shallow, and irregular. Instead of being an inch higher than the trochanter major, the highest part of the head was three-quarters of an inch lower than the trochanter. The neck was only half an inch in length, and was planted lower down on the bone, especially near the great trochanter. The margin was extended so as to form an overhanging ledge below, behind, and before. The outline of the head was flat below and arched above, having, on the whole, a somewhat square shape, with rounded angles. The surface departed from the spherical. The depression forming the pit and groove for the round ligament was compressed so as to form a mere fissure, elongated vertically. The man, aged fifty, had had several attacks of acute rheumatism. On admission to the hospital he complained of a gnawing pain in the hip and shoulder joints, and the motions of the hip-joints were considerably impaired. He died from exhaustion from a large lumbar abscess. He had been a shoemaker. For this information Dr S. was indebted to Dr Steell, one of the house-surgeons.

Dr Struthers remarked that such alteration in form in chronic rheumatic arthritis was well known, but the question he wished to raise was the meaning of it. It would be observed that the two bones had undergone precisely the same alteration; and those familiar with the form of the upper end of the thigh-bone in the quadruped would recognise the approach to resemblance,—the trochanter rising highest, the shorter neck, and the less extent and the departure from the spherical form of the articular surface. These differences and changes had reference to the more limited motion, especially of a rotatory character, at the joint. Why should the bone assume this particular form from long-standing rheumatic disease? We must recollect that the change was not produced simply by absorption, but that all the textures were constantly undergoing renewal, so that it was not the former bone changed, but a new bone laid down by nature of this particular form. Was it that the parts on which the pressure of the erect posture told were consequently reproduced in a less developed form, or was it that the new form was laid down to suit the more limited motions of the joint, either as a result of previous stiffness from pain and thickening, or with the view of bringing about more limited motions of the affected joint?

### III. ANEURISM OF INNOMINATA FATAL FROM PRESSURE ON TRACHEA.

*Dr Haldane* showed the arch of the aorta with its branches, together with the larynx and trachea of a man who had lately died from the effects of an aneurism of the innominate artery. The patient, a man about fifty-five years of age, by occupation a compositor, of strictly temperate habits, presented himself to Dr Haldane, at the Royal Public Dispensary, complaining of difficulty of breathing and cough. The inspiration was of a markedly laryngeal character, and as there was no tenderness over the larynx, or other indication of laryngeal disease, the idea of an aneurism at once suggested itself. The patient was accordingly recommended to come into the Royal Infirmary, which he agreed to do, and was admitted into one of Dr Haldane's wards. On careful examination, no abnormal fulness or pulsation at the root of the neck could be detected, but there was partial dulness on percussion over the upper part of the sternum near the right sterno-clavicular articulation; the lower part of the trachea was a little pushed backwards, and was very slightly twisted towards the left side. The heart's sounds were natural, and nothing abnormal could be heard over the presumed seat of the aneurism. The right arm was somewhat œdematous, and the pulse at the wrist was almost imperceptible; its great feebleness contrasting strongly with the natural pulsation in the left radial. No observation as to the state of the pupils could be made, as both lenses had been extracted some time before on account of cataract. During the fortnight he was under observation the breathing gradually became more difficult, and the laryngeal character more intense; but, as from the almost total absence of paroxysmal exacerbation, it was presumed that the dyspnœa was due to direct pressure upon the trachea, it was not thought desirable to have recourse to tracheotomy. The patient died gradually by combined asthenia and asphyxia.

On *post-mortem examination*, the arch of the aorta was found natural, but the innominate artery was dilated into an aneurismal sac of the size of a hen's egg. The right carotid and subclavian arteries arose from the posterior surface of the sac about its middle. The sac was lined by a thick layer of firmly coagulated fibrine. The trachea was pushed a little to the left, and its calibre, for a space of about an inch in length, just above the root of the neck, was very seriously diminished. Another aneurism, about the size of a filbert, arose from the posterior wall of the transverse portion of the arch of the aorta, and was applied to the trachea immediately above its bifurcation, but had produced no bulging of its wall inwards. There was no special interference with the pneumogastric or recurrent nerves on either side. The heart was natural; there was scarcely any atheromatous disease of the arterial system.

Dr Haldane remarked that when one examined the preparation (which had been stuffed and dried), it might seem singular that the tumour in the neck had not been made out during life. This, however, could not be done. No doubt whatever had been entertained as to the existence of an aneurism, but direct evidence of the presence of a tumour was wanting. It must, however, be borne in mind that the sac was now more distended than it had been during life; and, besides, such tumours were so marvellously packed among the normal textures that an aneurism of very considerable size might exist without its presence being distinctly recognisable. The absence of pulsation over the sac was, doubtless, to be explained by the presence of the thick layer of fibrine with which it was lined.

### IV. CALCULI IN KIDNEY AND BLADDER.

*Dr Stewart* showed the kidney and bladder of a man whose body he had recently examined in the Royal Infirmary. The patient was admitted suffering from the symptoms of vesical calculus; but, from the state of his general health, operative interference was contraindicated, and he died shortly afterwards. On examination of the body, the pelvis of the left kidney was found completely occupied by a calculus, which sent branches into the calyces. There were one or two cysts in the substance of the kidney. The urinary bladder was contracted and thickened, and its cavity was almost completely occupied by two calculi of considerable size.

### V. MULBERRY CALCULUS.

*Dr P. H. Watson* showed a mulberry calculus, coated with phosphates, which he had recently removed from the bladder of a boy of ten years of age.

### VI. LACHRYMAL CONCRETION.

*Mr Benjamin Bell* showed a specimen of a lesion, interesting from its rarity, for which he was indebted to Dr Cowie, of Lerwick. It was a lachrymal calculus, an inch and a-half long, rough, and nodulated on its surface. The notes regarding the case were rather brief. It was not known in what part of the lachrymal apparatus it had commenced, but, from the size, it was evident have that it must latterly have involved every portion of it. The calculus had been removed in the country, and sent to Dr Cowie.

### VII. CASE OF SUCCESSFUL AMPUTATION AT THE HIP-JOINT.

*Mr Spence* read a paper on this subject, which will be found at page 585 of the present number of this Journal.

*Dr Handyside* (having asked if a microscopical examination had been made of the muscles where cut through to form the flaps, and having been informed that they had been examined by Dr Steell, and found healthy) observed that the Society, and the surgical profession, were indebted to Mr Spence for the communication of a case, interesting both on account of the disease and of the treatment which had been adopted. He quite agreed with Mr Spence as to the necessity of disarticulation in this case, for when a bone was concerned in medullary disease it should never be sawn through. The reason why he (Dr H.) had asked the question as to the condition of the muscle was, that such a lesion was seldom limited to the part from which it sprung; and that in some cases muscle in the neighbourhood of such tumours, which to the naked eye seemed healthy, had, on microscopical examination, been found to contain cancer cells and nuclei. The disease was essentially constitutional, and when the local manifestation had been removed there was always a danger of its return. Yet in this case, as the general health of the patient was good, and as it was clear that to leave the disease was to entail a certain and speedy death, amputation at the hip-joint was the only available mode of treatment. The details of the operation were interesting and instructive, and Dr Handyside had been particularly struck by the benefit derived from the use of Lister's tourniquet for compressing the aorta. No doubt, in a large hospital, where the services of assistants in sufficient number, and of adequate ability, could be secured, they might, by grasping the separate arteries in the posterior flap, restrain the hæmorrhage; but, in other circumstances, where the number of assistants was necessarily limited, Professor Lister's instrument would be found invaluable. The mode of operation since the time of Liston, by antero-posterior flaps, seemed the best; it was performed with greater facility and in shorter time than by any other, and the head of the bone was most readily disarticulated. The mortality following this operation was certainly great; nevertheless, by the improved methods of operating, and by the careful selection of cases, the opposition to it would doubtless be rendered less general, and attention would be drawn to cases where it would be available. Mr Spence's case would do much to bias the surgical mind in favour of the operation; while its performance in improper cases had excited strong prejudices against it.

*Dr Sanders* stated that, as was most probably known to most members of the Society, there was in the museum of the College of Surgeons a preparation almost identical with that just exhibited by Mr Spence, and where amputation at the hip-joint had been successfully performed by Dr Handyside. Recovery was for the time complete, but unfortunately the disease returned in the eyeball and in the stump, about four months and a half afterwards.

*Dr Struthers* remarked that it was interesting, in reference to the shock from amputation at the hip-joint, that in all the cases, so far as he knew, in which it had been performed on account of disease in a thigh stump, the patients had recovered. From the fact of such patients having recovered from the first operation it might be inferred that they had to an unusual extent, originally and constitutionally, the power of resisting shock, still the fact he had mentioned seemed to show that the shock depended more on the extent of organ lopped off than on the size of the wound. While quite agreeing with Mr Spence as to the advantage of disarticulating in preference to dividing the bone through the trochanters, Dr Struthers thought from the appearance of the preparation that the latter operation might have been safely performed, although this could not have been known until a section of the bone had been made. In Dr Handyside's case the trochanters were involved, the neck was thickened, and the head flattened. Dr Struthers well remembered Dr Handyside's operation;

and, after a good deal of experience of surgical operations, he might say in reference to it, and to that now detailed by Mr Spence, that he had never seen two operations better performed.

*Mr Benjamin Bell* considered the case narrated by Mr Spence as remarkably complete, showing how, on the one hand, he was always actuated by sound surgical principle, and on the other by regard for the interests of his patient. With regard to the remark of Dr Struthers as to the success of amputation at the hip-joint subsequent to removal of the limb lower down, was it not the case that in all secondary operations the circumstances were altered? All the vessels must have undergone diminution in size, and this must render the risk of shock less formidable.

*Mr Spence* observed in reference to the remark of Dr Struthers, that the possibility of patients becoming, as it were, accustomed to amputation, must be viewed in much the same light as the alleged possibility of eels becoming accustomed to skinning. There was, however, a vast difference between amputating at the hip-joint when the limb was entire, and where a great portion of the member had already been removed, because, when a large part of the body was removed at once, there was necessarily a great shock to the system, and it was a well-known surgical principle that the smaller the part removed the less was the danger to life.

### VIII. CASE OF DEPRESSED FRACTURE OF THE STERNUM, ETC., TERMINATING SUCCESSFULLY AFTER AN ATTACK OF PYÆMIA.

*Dr Gillespie* read an account of this case, which will be found at page 618 of the present number of this Journal.

*Dr Struthers* well remembered this case, which for some time had been the occasion of much anxiety. While it was considered as probable that the tumour over the sternum was an abscess, the possibility of its being an aneurism prevented its being opened till the very last; the existence in such a situation of a pulsating tumour beating synchronously with the heart being well calculated to give rise to this suspicion. Dr Struthers remembered that this case illustrated well the use of the sternum in serving as a splint to the ribs, as the upper part of the chest moved separately from the lower.

*Dr Handyside* considered this as a very important case, as recovery after such complicated injuries was extremely rare. He remembered, in reference to the existence of a mediastinal abscess, having seen with the late Dr Alison a boy suffering from this affection, and who died on the following day. The pus occupied a space which measured three inches from above downwards, and from one and a half to two transversely. During each pulsation of the heart there was a synchronous escape of sanious pus from the space between the third and fourth intercostal cartilages on the left side. Surgery in this case could do nothing, and the boy sank.

*Dr P. H. Watson* considered that a point of pressing and paramount importance in this case was, as to whether there had really been pyæmia at all. Different meanings in the present day were attached to pyæmia; according to some wherever there was a suppurating wound, with abscesses elsewhere, and a certain amount of constitutional depression, there was pyæmia. In cases of injuries of the head, multiple abscesses sometimes formed elsewhere, the patients recovered; and such cases were long thought different from pyæmia, because the patients did not die. Again, after injuries and operations involving interference with the venous system, abscesses of the surface, etc., formed, and yet recovery took place; according to some, such were cases of pyæmia, while according to others the mere fact of recovery invalidated this. Dr Watson was inclined to consider all such cases as substantially belonging to the pyæmic group, and to attribute the fatal issue of those cases of pyæmia attested by well-marked constitutional symptoms to the situation of the secondary abscesses, occurring in important internal organs, rather than to the mere occurrence of suppuration, or the existence of leukæmia, or of some poisoned condition of the blood to which it had been attributed by some. In Dr Gillespie's case there was so much sympathetic constitutional disturbance from the original abscess that it was difficult to say how much the symptoms had been due to this and how much to the presumed pyæmia. It was worthy of remark that when the original abscess was opened the constitutional disturbance went off. Dr Watson did not say that there was not some peculiar condition present, but it was certainly difficult to see from the symptoms given wherein the pyæmia had consisted.

*Dr Gillespie* said, if Dr Watson would state his definition of pyæmia, he would then be able to decide, whether the case under consideration could come under the appellation.

*Dr Watson* replied that in the living patient he associated the name pyæmia with certain symptoms, and in the dead subject with the existence of multiple abscess, but that it was difficult to state distinctly what the pathological condition in question absolutely was. What Dr Watson desiderated was that Dr Gillespie should explain what he meant by pyæmia, for it was due to his making use of the term with referrence to this case, that Dr Watson had introduced this topic into the discussion.

*Mr Spence* remembered when the definition of Asiatic cholera was, that it was inevitably fatal, and that cases which recovered had not been examples of the disease. It seemed to be the same now with regard to pyæmia. If after amputation we found about the eighth or ninth day that the patient complained of rigors, that the pulse was quick, that irritative fever set in, that the colour of the skin was altered, that abscesses formed on the surface, and that, finally, the patient died, we said without hesitation that he had died of pyæmia. If, on the other hand, as he had several times seen, after somewhat similar symptoms, the patient recovered, such cases were considered ambiguous. Mr Spence had seen disease of the prostate terminate in suppuration, this disappear, and be succeeded by abscesses in different parts of the body; such cases, he must confess, he considered as pyæmia too. Such cases were very much like that described by Dr Gillespie. It seemed impossible to draw a line of demarcation between the two sets of cases, unless, indeed, we went back to the cholera definition.

*Dr Gillespie* stated that among the most marked symptoms presented by his patient were frequent rigors, dry tongue, quick, feeble pulse, and copious perspirations. When the abscess over the sternum had been laid open, another formed over the hip, and another on the forearm; this to him was sufficient to constitute pyæmia. Dr Gillespie did not think that in such cases the system was poisoned directly by pus, but that the constituents which form pus were still in the blood, and led to the formation of abscesses elsewhere.

*Dr Joseph Bell* thought that Dr Gillespie had been rather hardly used in doubts having been expressed as to the patient's having gone through an attack of pyæmia. Great advancements had been made in our knowledge of pyæmia; but as its pathology was still *sub judice*, we must go to its symptomatology. Dr Bell remembered that, some years ago, six cases occurred in Mr Spence's wards, in which it was believed that pyæmia existed. Three of these cases recovered, three died. Dr Bell recollected that Mr Spence's description of the symptoms was somewhat as follows: if, after amputation, the pulse rose above 120, if there were occasional rigors, if the surface was bathed with sweat, if the skin was of a faint yellowish colour, and there was a peculiar hay smell of the breath, the case might be considered as one of pyæmia. Dr Bell did not think that Mr Spence had been wrong in his diagnosis regarding the three cases which recovered, he believed that they were examples of pyæmia, and he thought that Dr Gillespie's case had been of the same nature.

*Mr Spence* mentioned that, long ago, after pyæmia had begun to be much talked of, he had been struck, in looking over Ballingall's Military Surgery, by seeing it stated that what was called fever after operations was analogous to yellow fever. The symptoms were exactly those of pyæmia, and it was evident that this condition had been long known, although it had not yet been defined pathologically.

*Dr Gillespie* begged to recall to Dr Watson's recollection a case of sloughing from urinary infiltration, which they had seen together in the Surgical Wards. In that instance the patient was labouring under long-continued profuse diaphoresis, with frequent rigors, and secondary abscesses on the extremities, besides swelling and tenderness of the wrists, and Dr Watson had remarked, that the patient was dying of pyæmia. That patient was now alive and well. As both cases were under Dr Gillespie's observation, he could state confidently that the pyæmic symptoms were precisely similar, and he therefore hoped, that Dr Watson would now admit, that Dr Gillespie was warranted in regarding the present case as an illustration of recovery from pyæmia.

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### SESSION XLII., 1862–63. MEETING II.

*Wednesday, 3d December* 1862.—James Spence, Esq., President of the Society, in the Chair.

### I. FRACTURE OF FEMUR, WITH LACERATION OF BLOOD-VESSELS.

*Mr Spence* showed a specimen, removed by amputation from a man who had received a severe injury. The wheel of a railway waggon had passed over the lower part of his thigh, and had occasioned a comminuted fracture of the bone. A curious circumstance in the case was, that while the rest of the soft parts had been reduced to pulp, the skin had, to a certain extent, escaped; it had retained its integrity, though it had lost its vitality. When the man was brought into the hospital the limb was cold, and there was no pulsation below the seat of injury. There was also a minor injury of the other leg, and a wound in the perinæum. Primary amputation being the only resource, it was performed immediately below the trochanters, as the bone was splintered a good way up. In this case, Mr Spence again used Professor Lister's clamp, and again with a good result, so far as regarded the stopping the bleeding from the posterior flap. The preparation showed that there was a wound of the popliteal vein, occasioned by a portion of bone. The artery was not torn through, but the internal coats were lacerated, and the external coat was stretched. With a low magnifying power the ruptured and twisted portion of the internal coats could be seen through the external coat; this condition must, of course, have cut off the supply of blood from the limb.

### II. LARGE TUMOUR OF THE NECK REMOVED BY OPERATION.

*Mr Spence* showed a tumour which he had to-day removed from the neck of a woman in the Royal Infirmary. It occupied the right side of the neck and face, from the parotid region to within one and a half inch of the sternum, and had been growing for sixteen or seventeen years. The woman was a native of Lancashire, and had been seen by a surgeon in Manchester when the growth was about the size of an orange. At that time she was rather dissuaded from undergoing an operation. The tumour, however, gradually increased in size and weight, and caused so much deformity that she became willing to submit to any operation for its removal. When the patient came under Mr Spence's care the sterno-mastoid was found to be stretched over the tumour; still, as the growth was tolerably movable, Mr Spence thought it could be removed. The tumour was to-day removed entire, except that a cyst in the parotid part gave way; the whole was, however, dissected out, and it would be seen that the parts fitted accurately, and that nothing had been left. The great vessels were necessarily completely exposed in the course of the operation. The tumour was of fibro-cystic structure, and weighed exactly seven pounds. In reply to a question from Mr Edwards, Mr Spence stated that he had been chiefly induced to operate by the slow growth of the tumour. This showed its non-malignant nature, and hence, though it might have contracted adhesions, it was not probable that it had become incorporated with the adjoining textures; it was, therefore, likely that the sheath of the vessels was not involved, and it seemed probable that by dissecting upon the tumour with the edge of the knife there would not be much danger of wounding them. The one great difficulty in performing such operations under chloroform, in the recumbent position, arose from the slipping down of the patient into bad positions; the old method, with the patient seated firmly in a chair, and in a better position, was certainly in this respect preferable.

### III. CURE OF ABDOMINAL ANEURISMS BY CALCAREOUS DEGENERATION.

*Dr Haldane* showed a very remarkable specimen of disease of the abdominal aorta. The subject of the observation, a woman fifty years of age, was admitted into the Royal Infirmary towards the end of last autumn, in a very exhausted condition. She could give no distinct account of herself, and none of her friends made their appearance. She had almost constant cough, with expectoration of a pneumonic character, and dyspnœa subject to paroxysmal exacerbations. None of the physical signs of pneumonia were present; but there was a little dulness over the upper part of the sternum, with increased pulsation at the root of the neck. There were the physical signs of contraction and incompetence of the aortic valves; the murmurs were propagated to, but were not intensified over, the seat of dulness. The patient remained in much the same state for a week, and then died suddenly, without, however, expectorating blood in any quantity. The diagnosis was aortic disease; an aneurism of the transverse part of the aorta, which had *oozed* into the trachea, and which had, perhaps, ruptured.

On *post-mortem examination*, the aortic valves were incompetent, and vegetations were attached to their ventricular aspect. There was much atheromatous disease, with semi-transparent thickening of the ascending aorta. Arising from the transverse portion of the arch was an aneurism, the size of a small apple. This aneurism was pretty closely applied to the anterior wall of the trachea; no rupture had occurred, and the tube was not remarkably compressed. The left pneumogastric nerve, and the commencement of the recurrent, were a good deal stretched over the aneurism. There was a moderate degree of atheromatous and calcareous disease of the descending portion of the thoracic aorta. The lungs were congested, and contained several patches of pulmonary apoplexy. So far, the appearances found were not very different from what had been anticipated, but the most remarkable lesions were still to be discovered. The whole of the thoracic aorta had been removed with the heart and respiratory organs. After these had been examined, the finger was introduced into the commencement of the abdominal aorta, and, having been passed down a short way, an impression was produced as if the vessel was in great part filled with calcareous matter. The vessel having been removed, and examined, was found in the following condition:—Three sacculated dilatations or aneurisms arose from the anterior wall of the abdominal aorta within the space of about three inches. The *first*, involving the origin of the cœliac axis, and rather less than an inch in diameter, was completely filled with calcareous matter, presenting a considerable resemblance to a mass of coral. The *second*, rather larger than the first, and merging into it superiorly, involved the origin of the superior mesenteric; it was almost completely lined with a smooth layer of calcareous matter, but at its upper part, and to the right side, was a mass of the same coraline-looking material as filled the first. The right supra-renal artery arose from the lower and right border of this sac. The *third*, smaller and less marked than the others, involved the origin of the left supra-capsular and left renal arteries; it was lined, except at the points of emergence of the arteries, with a rough layer of calcareous matter. The *cœliac axis*, arising from the first sac, and dilated at its origin, was completely obliterated up to the point where it divided into its branches; its coats were calcareous, and its calibre was filled with cretaceous matter projecting into it from the coral-like mass. The origin of the *superior mesenteric artery* was completely obliterated, one of the calcareous scales which lined the sac being most closely and accurately applied over it. The *right supra-renal artery* was pervious, but its origin was contracted. The *left supra-capsular* and *renal arteries*, arising from the third sac, were pervious; the origin of the former vessel was contracted, that of the latter was natural. The calcareous degeneration of the vessel ceased abruptly at the lower border of the third sac; below this was only a single patch of cretaceous deposit, on a level with, but at a little distance from, the inferior mesenteric artery. The *inferior mesenteric artery* was healthy, and of the normal dimensions.

Dr Haldane remarked that he had never met with a case exhibiting such extreme calcareous degeneration of the arterial system; and, from the appearance of the deposit in the upper of the three sacs, he could only suppose that it had resulted from the cretaceous degeneration of layers of fibrine which had filled the sac. Fibrine became calcified rapidly enough elsewhere, and there was no special reason why this change should not take place within the arterial walls. In the case of the two lower sacs a cure of the aneurisms had been effected by a calcareous degeneration of their walls. It was somewhat remarkable that no obliteration of vessels had been produced by the washing away of small portions of the cretaceous matter, which, when fresh, was not nearly so firm and consistent as it now appeared, when dried. Another point worthy of notice was the mode in which the collateral vessels must have carried on the circulation through the abdominal viscera, which must have been seriously disturbed by the complete obliteration of two such important vessels as the cœliac axis and the superior mesenteric.

### IV. MUSICAL MURMUR PRODUCED BY A LOOSE CARDIAC VEGETATION.

*Dr Haldane* showed a recent preparation of the aortic valves, where a loud musical murmur had been audible during life. The patient, in addition to the signs of a thoracic aneurism, had presented, during life, the signs of disease of the aortic valves. A loud systolic murmur, with a well-marked musical character, was audible at the base of the heart; a less marked bruit, without musical character, could be heard with the second sound in the same situation. The patient died from a pulmonary complication determined by the aneurism. On examination, the aortic valves were tested by passing water down the vessel, and were found to be slightly incompetent; when the vessel was laid open the valves were found much more extensively diseased than the limited degree of incompetency might have been supposed to indicate. A tongue-like vegetation, four-fifths of an inch in length, and about half an inch in breadth at its base, from which it tapered slightly to its apex, arose at the margin of the posterior sigmoid valve. Before the vessel was laid open this vegetation lay above the valves, and was quite freely movable. The remaining portion of the free edge of this valve was loosened, and slightly ulcerated at one point. The right semi-lunar valve was of normal dimensions; it was perforated by an ulcerated opening of the size of a barley-corn, to which a small vegetation was adherent. The left valve was natural, except that a small vegetation was attached to its corpus arantii. Immediately below the junction of the right and the posterior sigmoid valves was a small ulceration of the endocardium. Dr Haldane observed that he entertained no doubt that the systolic musical bruit had been occasioned by the large vegetation described; when the blood passed up the vessel the vegetation was thrown freely into vibration, and thus modified the character of the sound. On the other hand, during the diastole the vegetation fell back over the margin of the valves, and prevented such an amount of regurgitation as, without its presence, the diseased valves might have permitted. Hence, the bruit with the second sound had been inconsiderable, and little or no secondary change had been produced in the heart; as, though the lesion was of considerable standing, the organ had not become hypertrophied, its weight being only nine ounces.

### V. FRACTURES OF THE NECK OF THE THIGH-BONE.

*Mr Edwards* showed two specimens of fracture of the neck of the thigh-bone. The *first* was an example of the lesion first delineated by Sir Astley Cooper, where the neck of the femur was driven into the trochanter and shaft of the bone. The old woman, from whose body the specimen had been obtained, had suffered from many of the symptoms of fracture within the capsule; still she had walked about and had not experienced much suffering or inconvenience. In the *second* case the patient had also been able to walk about a little.

### VI. ON A CONSEQUENCE OF IMPERFECT ABORTION.

*Dr Matthews Duncan* read a paper on this subject, which will be found at page 589 of the present number of this Journal.

### VII. ON THE EMPLOYMENT OF SARRACENIA PURPUREA IN SMALL-POX.

*Dr Haldane* stated that, having had charge of a considerable proportion of the cases of small-pox in the Royal Infirmary, admitted during the present epidemic, he had thought it right to give a trial to the alleged specific remedy of the North American Indians, the sarracenia purpurea. The specimen of this drug first obtained consisted of the leaves with portions of the roots attached. No effect was produced; but as it had been said by some that the leaves were inert, and that the root alone contained the active principles of the plant, a fresh series of experiments was instituted. A supply of the root was accordingly obtained from Messrs Savoy and Moore, and a decoction prepared in conformity with the directions laid down by Mr Miles, in a paper in the *Lancet*, in which he strongly recommended the employment of the drug. This decoction was administered to six patients, and careful notes were kept of these cases; and Dr Haldane had become satisfied that the root of the sarracenia, at least in its dried state, was perfectly useless. The cases ran their course in the ordinary way without the slightest modification; no change was produced in the character of the eruption; and no physiological effects whatever were produced. Dr Haldane might add that he had been careful to select cases unmodified by vaccination or a previous attack of the disease, for such irregularities were met with in the case of the modified disease that it was difficult or impossible to draw from them any conclusions as to the value of therapeutic measures; and he considered it probable that some of those who had spoken favourably of the sarracenia had mistaken the progress of the modified disease for an important result produced by the medicine. Dr Haldane's cases had been of medium severity, one had died, the others had gone through the disease in the usual way. He had therefore abandoned the use of the drug, as the price (twenty-eight shillings a pound) was rather too high to pay for an utterly useless preparation.

### ELECTION OF OFFICE-BEARERS.

The following gentlemen were elected office-bearers at the meeting of the Society held on Wednesday, December 3, 1862:—*President*—James Spence, Esq., F.R.C.S.E. *Vice-Presidents*—John Coldstream, M.D.; Professor Maclagan; Andrew Wood, M.D. *Councillors*—Professor Miller; Thomas Keith, M.D.; Thomas Williamson, M.D.; William Turner, Esq.; William Finlay, M.D.; Charles Dycer, M.D.; William Menzies, M.D.; John Gillespie, M.D. *Treasurer*—John Struthers, M.D. *Secretaries*—James D. Gillespie, M.D., 45 Castle Street; Patrick H. Watson, M.D., 10 Charlotte Square.

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## PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

### SESSION XXI.—MEETING XIII.

*July* 9, 1862.—Dr PATTISON, *President*, in the Chair.

### I. SUDDEN DEATH DURING PREGNANCY FROM SUPPRESSION OF BILE.

*Dr Parker*, of Nova Scotia, contributed the history of a case of sudden death in a young woman about seven months pregnant, apparently from suppression of bile. "She had suffered," said Dr Parker, "throughout her pregnancy from vomiting, accompanied by slight mental excitement. I was called to see her the day before she died, and found her perfectly yellow from jaundice, over her whole body; at one time excited and again in a semi-comatose condition, with a feeble pulse, which grew weaker as confirmed coma came on. She had constant vomiting, and the ejected fluid was "black vomit," offensive, black, grumous, and containing large quantities of blood corpuscles. When too weak to vomit it, this fluid ran out of her mouth until she died. The fœtal heart could not be heard when I saw her, but the placental bruit was distinct. Weak as she was, I was desirous of making an effort to relieve her of her child; but the medical man in consultation, perhaps judiciously, considering her prostrate state, dissuaded me from attempting it. While jaundice is not uncommon, is not death under such circumstances rare? We have had several cases of yellow fever in our harbour this summer, and several took the disease on shore; an unprecedented circumstance. Had this young woman been seized in August or September with the symptoms here so briefly narrated, I should, I think, have looked upon her case as one of yellow fever. No examination was obtained, and of course the condition of the liver and gall-bladder could not be ascertained."

*Dr Pattison* remarked with reference to the case of jaundice, that he had never seen the blood so defibrinated in that disease so as to produce hæmorrhage during confinement.

*Dr A. Simpson* doubted whether the case was one of simple jaundice. We had often yellowness of the skin making its appearance a little before death, owing to the presence of some poison in the blood. The suddenness of the attack, together with the fact that the sister-in-law and husband of the patient were subsequently attacked with similar constitutional symptoms, pointed to yellow fever. At the same time, as Dr Parker was a man of great experience, it was difficult, in the absence of farther information, to come to a satisfactory conclusion.

In answer to a question from the President, Drs Wilson and Keiller stated that they had seen numerous cases, in 1843, of relapsing fever attended with abortion. One woman, seen by Dr Keiller, was six months pregnant when she was attacked by the fever, and aborted before she died. He (Dr K.) made a post-mortem examination, and found the placenta still adherent to the uterus. The disease, as was generally the case with all fevers, was most fatal in pregnant women.

### II. TARTRATE OF ANTIMONY AS AN OXYTOXIC.

*Dr Parker* contributed the following notes:—Tartarized antimony has long been, as you are aware, in use in obstetric practice. Lecturers and authors have informed us, and still continue to do so, that the result of its administration is to advance the labour,—1*st*, By overcoming muscular rigidity; 2*d*, By causing a free flow of mucus, and thus lubricating the vaginal mucous membrane. While not confining its use to primiparæ, they speak of it as more especially serviceable in this class of cases.

With this much theoretic knowledge of its action I, sixteen years since, commenced testing it practically, and it was not long before I observed that, in addition to the actions previously attributed to it, a very important end was effected in a large majority of cases by its administration. I refer to its influence on the uterine contractions; in other words, to this agent as a powerful stimulant to the involuntary muscular action of that organ. While relaxing the circular or sphincter fibres of the os uteri, I observed that it produced more powerful propulsive efforts, by stimulating the longitudinal and other muscular fibres concerned in effecting delivery.

As early as the years 1846 or 1847 I called the attention of some of my medical friends to the facts, and requested them to keep a record of cases in which this auxiliary agent was prescribed.

It is unnecessary that I should occupy the time of the Society, by detailing minutely reports from my case-book. A brief summary of what I have observed in reference to this matter in a very large number of instances, and the deductions drawn therefrom will suffice, and equally subserve the end I have in view.

1*st*, Tartarized antimony relaxes both voluntary and involuntary muscular fibre concerned in resisting and retarding labour; in other words, it overcomes the rigidity of the os uteri and perineal muscles.

2*d*, By increasing the vaginal mucous flow, and lubricating that surface, it essentially aids the progress of parturition.

3*d*, *It stimulates to increased contractile action the oblique, longitudinal, and other muscular fibres concerned in expelling the child.* It may be said that this increased contractile action is only apparent, and that the advance of the labour is due to the decreased rigidity and resistance of the os and more external parts, in consequence of the tartarized antimony having produced the results stated in the two preceding propositions. This objection is at once removed and proved to be incorrect by the results following its administration, in cases not primiparous, where there is "inertia uteri" with the os fully dilated, the vagina patent, and the perineum not rigid. In just such labours I have been in the habit of prescribing the tartarized antimony instead of ergot, with the most marked results.

4*th*, Unlike ergot of rye it does not produce continuous contractions, but, in the majority of cases, enhances the power and force of the regular pains, and admits an interval of ease.

5*th*, It continues to some extent to exert its contractile influence after the expulsion of the child, and hence reduces the risk of hæmorrhage. Of course this result will be modified by the interval that has elapsed between the administration of the antimony and the delivery.

6*th*, Unlike ergot, it does not interfere with the extraction of the placenta, by producing irregular uterine contractions. The superior and propelling portion of the organ, it is true, is generally more firmly contracted than natural after delivery; but the sphincter (the os) is mobile and yielding, and but little difficulty (no morbid condition of the parts being present) is experienced in this particular; abdominal pressure, and traction in the proper axis will almost invariably effect the end without injury to the cord.

7*th*, While perhaps more applicable to primiparæ, tartarized antimony may be administered with like results in subsequent labours, and that at any age or stage of labour, without risk of injury to the uterine texture.

8*th*, Administered as I shall presently state, its emetic effects are not troublesome; not more so when they do occur than from ergot, when this latter, as it very frequently does, produces vomiting. This effect of the antimony, when present, will promptly yield to cold oatmeal, coffee, a mustard cataplasm over the stomach, or a dose or two of the trisnitrate of bismuth.

9*th*, I have never known depression of the vital powers, or more than temporary inconvenience, to follow its use when administered in the mode hereinafter mentioned; although I can readily conceive that there are cases in which it would not be admissible In the selection of appropriate cases, and discriminating in this matter, medical men must have recourse to, and be guided by, their experience and general principles.

10*th*, Tartarized antimony, by facilitating and shortening the process of parturition, as above mentioned, without risk to the uterine apparatus or general system, will, we may confidently expect, by conserving the vital powers and reducing the nervous shock, show, on more extensive trial, favourable statistical results in the mortality attending obstetric practice.

The foregoing observations and conclusions have not been arrived at hurriedly, and they are not deductions drawn from a limited number of cases, or, as you are aware, from a limited field of observation. While they will be found correct in a large majority of cases, I am free to admit that tartar emetic will not in all cases produce the same results. And, indeed, of what therapeutic agent can we write in other language? Once only (six or seven years since) I have met with a single recorded remark that would lead me to infer that the same *contractile influence* to which I have referred has been noticed by others. I cannot now lay my hand on the periodical which contains it, but I recollect the writer states that, after injecting two grains (I think) of tartarized antimony into the rectum in a case of tedious labour, for the purpose of overcoming muscular rigidity, *the pains very shortly became stronger and more effectual, or words to that effect.*

The rationale of its action is undoubtedly to be referred to the influence tartarized antimony exerts on the sympathetic system of nerves. The uterus, like the stomach and intestinal canal, is composed of involuntary or unstriated muscular fibre, and is, like the other abdominal organs, largely dependent for nervous supply on the sympathetic system. Now, we all know as the action of antimony on these hollow abdominal muscles or canals: that, when administered in sufficient quantity, forcible contractions of the circular and longitudinal fibres composing them, and relaxation of the sphincters, take place; just what I have remarked and recorded as occurring in the uterus. Similarity of texture and nervous supply would then analogically point to that which experience and observation has taught me is in reality the case in reference to the action of this agent in the uterus in the act of parturition.

Ergot of rye, while acting primarily and with more energy on uterine muscular fibre, exerts at the same time a secondary or inferior influence on the stomach, often producing, as I have before remarked, troublesome vomiting.

Now, the deduction one would naturally draw from the above observations is, that all, or most of what are termed "specific emetics," would, through the agency of the sympathetic system, exert their contractile influence on the uterus, and *vice versa*, as just illustrated in the case of ergot,—*i.e.* therapeutic agents which cause uterine contractions will have a similar action on the stomach and intestinal canal. I have not administered ipecacuanha with the same object in view, but have very little doubt that if tried it will be found to produce like results. Its taste and bulk would, however, other things being equal, give a preference to its more convenient ally tartar emetic; while, on the other hand, its depressing effects would not be so marked in cases where a continuance of the medicine should be considered advisable. In this connexion I may remark that it is suggestive, and at the same time corroborative of the views here stated, that nausea and actual vomiting, occurring naturally during labour, very generally advance, and often materially shorten the process, by increasing the contractile power of the uterus. I administer the tartarized antimony as follows:—From one to two grains are dissolved in a wineglassful of water, and a dessertspoonful given every ten or fifteen minutes *ad nauseam;* and if the pains are not increased sufficiently, and there are no contra-indicating circumstances present, it is continued beyond this point. The rectal mode, to which reference has been made, has its advantages, and, at the same time, its inconveniences; but there are cases in which it should have the preference.

It only remains for me, in conclusion, to ask the members of this Society to give the subject their consideration, to note at the time the results attending its administration, and not to report their conclusions until they have given the matter a fair and somewhat extended trial.

*Dr Pattison* said, with regard to the alleged action of antimony as an oxytoxic, he had never used it without always requiring to employ the forceps subsequently.

*Dr Cochrane* thought that antimony, by exciting vomiting, might act mechanically on the uterus and induce contractions.

*Dr A. Simpson* remarked that antimony, by depressing the circulation, produced excitation of the involuntary muscles and relaxation of the voluntary muscles. In this way antimony might hasten labour by stimulating the uterine fibres along with the involuntary muscular fibres, and relaxing the pelvic muscles, but it did not appear to have any special action on the uterus such as was believed to be possessed by ergot.

*Dr Wilson* had used antimony frequently in cases of rigid os with good effects. He found, however, that natural nausea had more power in inducing relaxation of the os than that brought on by the administration of antimony.

*Dr Keiller* said the practice of giving antimony in cases of rigid os was not new; but, as bleeding in the first stage so as to induce relaxation of the cervix uteri was seldom practised now, we ought, perhaps, to use antimony for this purpose more frequently than we do. In chloroform, however, we now have a most valuable substitute.

*Dr Pridie* stated that he knew a practitioner and a pupil of Dr Campbell's who administered antimony in every third or fourth case.

### III. CASE OF DYSMENORRHŒA.

*Dr Stephenson* read the following notes:—

Mrs P., 22 years of age, 4 months married, with every appearance of strong robust health, consulted me for bilious vomiting, to attacks of which she had long been subject, and for which she had been treated with blue pill, and various other medicines, without relief. Upon inquiry, I found that ever since she began to menstruate she had been perfectly regular, but always suffered intense pain at those periods, and that the vomiting occurred only at such times, though not with every menstrual period, and never during the intervals. To each menstrual period she looked forward with the greatest anxiety and dread, suffering the most intense agony, and never sure but that it might be aggravated by severe sickness and vomiting, continuing for four or five days, and quite unfitting her for work. The menstrual fluid, she said, came away in clots and shreds, though she did not remember of ever noticing any distinct large membrane. Before each clot came away she always suffered an aggravation of the pain, and before turning unwell and during the period, she had a feeling of weight in the pelvis. Considering the vomiting of a reflex nature, and due to the dysmenorrhœa, and that the latter was of the congestive type, I told her that there was no use in giving any medicine in the meantime, but to pay great attention to keeping her bowels open, and when she felt herself turning unwell to apply two leeches to the anus. She was to call on me then and get a prescription for some medicine.

She called and informed me that having turned unwell that day, a day before she expected, she had not applied the leeches, and asked whether she should still apply them; I said she did not need at this time, but that we would try the effect of some medicine alone. I accordingly ordered the following mixture to be taken during her illness:—℞ Aquæ ammon. acet. ℥ijss.; tr. hyoscyami, ʒi.; sp. aeth. nit. ʒss.; aquæ, ℥iv. A tablespoonful four times a-day.

Two days after she called again, in the greatest joy. Though she was suffering great pain when she first called, and the discharge was not coming free, the day after she began to take the medicine, the pain became very slight, and on the second day it had entirely gone. She was now menstruating freely, without any clots; felt as she had never felt before while unwell; and for the first time in her life knew what it was to menstruate without pain. I expressed my satisfaction at the beneficial effect of the medicine, but warned her not to consider herself cured, that the next time she might be as bad as ever, and that she may have yet to submit to an examination, and perhaps operation. She expressed herself willing to undergo anything necessary for her complete cure, and left, promising to return in four weeks.

Anxiously I waited for her return, but the time passed without any appearance of her. I accordingly called, and found that she had menstruated freely at the natural time and for the regular period, without any pain or inconvenience whatever.

Again the four weeks passed, and again I called. She now told me that she had not menstruated at the proper time, nor felt any signs of its return, that one day previous, while at her usual work, she had fainted, and that since then she had felt sick at times. Considering the fainting due to conception having taken place, and corroborated by the non-appearance of the menses and the sickness, I told her that I believed she was pregnant, and that time would show. She is now in the eighth month of utero-gestation.

I was induced in this case to try the liq. ammon. acet., from a recommendation I saw of it in an American work—Dr Bedford's Obstetric Clinique. It is an old remedy, and one used by many, I believe, in this country; but, so far as

I can ascertain, reference to it as possessing any special effects in such cases is not made in any of our books, nor so far as I have heard taught in our schools. Surprised at my success I have therefore taken the liberty of bringing the details of this interesting case before you, that I may obtain the opinions of the much older, more experienced, and learned members of this Society.

*Dr Gordon* thought that some beneficial effect might be attributed to the henbane.

*Dr A. Simpson* said it was not certain whether the spirit of Mindererus effected the cure in this case. It was true that in some instances marriage brought on and aggravated dysmenorrhœa before conception, but the reverse was sometimes the case.

*Dr Stephenson* said that he had had another case since the one above described, in which he had administered the same remedies with a similar result.

### IV. TEDIOUS LABOUR, WITH PREPARATION.

*Dr Bruce* communicated the following case:—

Mrs C. was taken in labour with her first child on the 28th September 1860. When called to see her, everything appeared to be going on favourably, and I anticipated that there would be no obstacle to a speedy delivery, the more especially as the pelvis was roomy enough, and the child premature—the seventh month.

In this expectation I was disappointed, for, after the full dilatation of the os uteri, the head did not advance so quickly as it should have done, considering the strength of the pains and the condition of the passages; in fact, some assistance was required by means of traction with the hand to complete its expulsion. After this, instead of the shoulders following with the next pain, there was no further progress made whatever.

The head was then taken hold of, and moderate traction employed, but without any effect; I then exerted greater force, but without any further result. I could not find any obstacle to account for this state of matters, and having ascertained the child to be dead, I had no hesitation in using still greater force; but after a little, instead of bringing down the child any further, its neck gave way, the arms were then drawn down, and my hand passed up as far as possible to try and ascertain the cause of the obstruction, which I suspected must be caused by some abnormal condition of the fœtus itself.

In pressing up with this object I felt that the abdomen was greatly enlarged, and at the same moment a gush of liquid came away, probably from 1 oz. to 2 oz., and immediately thereafter the delivery was terminated. I was unable then, for want of time, to make any particular examination, but merely saw that the abdomen of the child had been filled with fluid, and that the intestines were in an undeveloped state. I was not allowed to take away the infant, and did not get any further opportunity for inspection. I informed the mother that it was a peculiar condition, which was not the least likely to occur to her again, and that she might keep herself quite easy upon that point.

On the 12th December 1861, I was again sent for to see the same patient in labour with her second child at the full time. She progressed very favourably this time also, until the head distended the perineum, and was partially protruded; then the same difficulty was experienced in effecting its delivery, though I employed traction with every pain, and it was some little time before it could be accomplished. After this there was remarkably slow progress, and even by exerting very considerable traction on the head with each pain it was about half an hour before the child was extruded. The cause of the delay was at once apparent in the greatly enlarged abdomen of the infant.

*Sectio.*—The peritoneum was very much distended with a clear serous fluid, and the intestines were seen lying at the back about the capacity of crowquills. In opening the peritoneum a small incision was made into the bladder accidentally, owing to its great distension, and it was also seen to be filled with the same clear fluid; its coats were much thickened as well as enlarged. Altogether, there was apparently not less than a pint and a half of fluid present.

I have no doubt whatever that in this patient's former pregnancy the state of matters was much the same as in this, and to me it appears extraordinary how this condition should occur in two successive pregnancies in the same individual.

### V. HYDATIGENOUS DEGENERATION OF THE PLACENTA.

The *Secretary* showed a specimen of hydatigenous degeneration of the placenta, sent by Dr Figg.

### VI. VARIOUS FORMS OF OVARIAN DROPSY.

*Dr Alexander R. Simpson* exhibited a preparation of a large ovarian tumour, which had been removed at the post-mortem examination of the body of a patient who died on the 6th instant. He had been first asked by Mr Cameron to see the patient, an emaciated woman of upwards of sixty years of age, about three months before, and found her suffering from a multilocular ovarian dropsy of enormous size, which had not attracted her special notice till about fifteen months previously, although, for aught she knew, it might have existed for a much longer period. It had been steadily increasing in size from the time when she had at first observed it, and now the pain and inconvenience she suffered were so great that she was anxious that some measures should be adopted for her relief. A first tapping was had recourse to in the early part of April. About 320 oz. of a dark brown gelatinous fluid were drawn off, and the diagnosis which had previously been arrived at was confirmed—that there were a number of secondary cysts of various sizes, and that the surface of the tumour had become extensively adherent to the abdominal parietes and viscera. The prognosis was, of course, of the gloomiest; not that the immediate risks of that first tapping appeared peculiarly great, but from the certainty that the cyst would refill, and that repeated tappings would end in the patient's death. Six weeks subsequently, a like quantity of similar fluid, somewhat lighter in colour, was drawn off. A third tapping was had recourse to, ten days before the patient died. The quantity of fluid was not so great, but there was some pus mixed with it; and flocculent masses of plastic lymph and of the broken down interlocular septa prevented the complete evacuation of the cyst, notwithstanding an attempt that was made, and with some success, to promote the evacuation by the cautious introduction of a quantity of tepid water. The patient remained comparatively well for some days; but inflammation having set in acutely in the sac, she speedily sank and died.—At the autopsy, which was made by Dr Berryman, the tumour was found to be adherent by its whole anterior surface to the abdominal wall, and at other places to the liver and various coils of the intestines, and the adhesions were for the most part so firm as to require considerable force for their separation. The pedicle was about four inches broad and very thin. He (Dr A. R. S.) believed that some of their English brethren who had had the most experience in the operation of ovariotomy would not have shrunk from its performance in this case, where, even in the dead body, it seemed so difficult; and he could not divest himself of the idea that, had the patient been seen at an earlier stage of her history, the removal of the tumour might have been effected with the most favourable result. It was, at all events, very evident that none of the other plans of treating ovarian dropsies with which we were acquainted could here have been followed out with any reasonable hope of a successful issue. The tumour, as the Fellows would perceive, was a fine specimen of the colloid tumour of the ovary, with different parts of it in different stages of development. An examination of it would, he believed, tend to confirm the views entertained by Professor Virchow, that the large cyst was formed by the destruction of the walls of multitudes of smaller cysts, which takes place as each cyst goes on filling and expanding with the growing mass. As individual cysts enlarged, their walls for the most part became atrophied and attenuated, and finally disappeared either entirely or in part, so as to cause them to coalesce into one large cavity, whilst the stronger septa remained more or less perfect, and gave the appearance of smaller secondary cysts projecting into the larger cavity. Hence the dark colour of the fluid in the large cavity, due to the admixture of altered blood which had escaped from the vessels injured during the destruction of the interlocular dissepiments; and hence the connecting bands of fibrous tissue, not of inflammatory origin, but of the same anatomical structure as was presented by the unbroken septa, which could in this case be seen to unite some of the secondary cysts, and which could not be accounted for if we adopted the theory of Hodgkin, that in such tumours one primary cyst was first produced, and that the smaller secondary ones subsequently grew from its inner surface like so many parasites.

Case 2.—Another specimen of ovarian disease, which he (Dr A. R. S.) wished to bring before the notice of the Society, had been that day obtained from the body of a woman of about forty years of age, who had died two days previously. She had been under treatment for tumours in connexion with the uterus or its appendages, which she had believed to exist for perhaps sixteen years, and from which she had suffered more or less pain and inconvenience during the last eight years of her life. During this period, she had been in the habit of coming from time to time to consult Professor Simpson, who thought that in all probability she was suffering from fibroid tumours of the uterus. There were two distinct, firm, unfluctuating masses, one imbedded in the pelvis, though reaching above it, and moving synchronously with the uterus; the other lying more in the abdominal cavity, and admitting of freer, independent motion. She had been able to continue her avocations till within the last eight or ten months of her death, when she had begun to suffer from ascites and occasional slight attacks of bronchitis, which necessitated her confinement to her room, and finally for many weeks to her bed. About two months before her death, the abdomen had become so distended with fluid that it had been deemed necessary to tap her in order to afford her some relief. The ascitic fluid, however, had rapidly re-accumulated, and in five weeks had again to be drawn off with the trocar and canula. After the evacuation of the abdomen, he (Dr Alexander R. Simpson) had examined the tumours very carefully by means of the stethoscope, but he had not been able to discover any distinct bruit.—At the post-mortem examination, the tumours had been found to be ovarian, as would be seen by looking at the preparation. The right was applied to the posterior surface of the uterus, and so firmly adherent to it as to seem quite incorporated with it. The uterus itself was of normal size. The left ovarian tumour had a longer pedicle, and had acquired some very firm adhesions to the omentum. The tumours were of a very rare pathological nature, and could be studied with the more interest when compared and contrasted with the more common form of ovarian dropsy already described. In this instance there were no prominently large cysts of the tumour in either side. If one of the tumours were handled, it was felt to be made up in great part of solid masses, with cysts of from the size of a hen's egg to twice or three times that size imbedded in it, and projecting on the surface. On opening these cysts, there escaped a yellow creamy-looking fluid, consisting in large proportion of fatty matter. The walls were found to be very firm, and of leathery consistence; and on their inner surface the cysts were studded all over with small, rough, wart-like granulations, of a pinkish hue, spread singly for the most part over the interior of the cysts, but sometimes collected into small projecting masses.

Case 3.—The third preparation was a specimen of ovarian tumour, of different character from either of the others. It was an example of the unilocular dropsy of the ovary, and had been removed from the body of a patient who had died a few weeks previously. The case presented the usual history of simple ovarian dropsies, developing in one side till it became of such size as to interfere with the functions of the abdominal organs, and necessitating the operation of tapping. The first tapping was performed in the end of August 1861; the sac rapidly filled, and, in October, she was received into Professor Simpson's ward in the Infirmary, where she was again tapped on October 15th, six weeks after the first operation. The fluid was still clear, but inflammation was set up in the cyst afterwards, and the patient became very much reduced. The cyst had become quite distended, and, though the patient appeared almost moribund, it was again evacuated on November 8th, as this seemed the only means likely to afford the patient any relief. A small quantity of tincture of iodine was injected, in the hope that the further secretion of pus might be checked; and to some extent this result was obtained. The cyst filled again, however, but discharged itself to some extent, from time to time, through the point where the wall had been perforated. A quantity of gas became developed in the cyst, and the fluid that escaped was of a very offensive odour. As it was again accumulating, the tumour was tapped for the fourth time on March 20th, but it soon filled up again, and when next it was tapped, April 25th, the canula was left in to keep up a continuous drain. The sac contracted somewhat; but the canula having fallen out, the orifice closed, and a re-accumulation of fluid took place. On June 4th, one of Chassaignac's drainage-tubes was passed into the cyst, and a quantity of very fetid pus escaped. It continued to drain away for some time, till at last the cyst seemed to be quite collapsed; the secretion of pus ended; and, when the tube was finally withdrawn after three weeks, it would have been difficult to determine that any ovarian or other tumour had ever existed. Under the long-continued suppurative process, however, the patient's health had been getting undermined; phlegmasia had set in in the left leg; frequent attacks of bronchial catarrh had distressed the patient, and caused sleepless nights. She suffered often from diarrhœa; extensive bed-sores formed in the back; the urine had been found to be albuminous; and it was believed that in all probability the kidneys had become the seat of waxy degeneration. Under these circumstances, it was not surprising that, notwithstanding the complete arrest of the purulent discharge from the ovary, the patient never rallied, but sank two weeks afterwards, under an attack of diarrhœa.—At the post-mortem examination, the liver, spleen, kidneys, and intestines were all found to be the subject of advanced amyloid or waxy degeneration. The sac, though growing from the left ovary, was found to be lying quite to the right side of the mesial line, having been tied to that side by a fibrous band. It was, as could be seen, collapsed to the size of an ordinary small bladder, and contained about a couple of ounces of thin purulent fluid. It was remarkable with what facility it could be separated from its connexion to the abdominal wall, notwithstanding the length of time that the canula and the drainage-tubes had been worn, and the consequent supposed intimacy of the adhesion that had been formed between them.

## ROYAL COLLEGE OF SURGEONS OF ENGLAND.

We are happy to announce that Dr James Pettigrew, a laureate of the University of Edinburgh, who has highly distinguished himself by his researches on the muscular structure and the nervous supply of the heart, has been elected Assistant Conservator to the Museum of the Royal College of Surgeons of England.

## ROYAL COLLEGE OF PHYSICIANS.

At the Annual Election Meeting of the Royal College of Physicians of Edinburgh, held on Thursday, 4th December 1862, the following office-bearers were elected for the ensuing year:—*President*—Dr David Craigie. *Vice-President*—Dr Alexander Wood. *Council*—Dr William Seller, Dr Alexander Wood, Dr J. G. M. Burt, Dr T. S. Wright, Dr A. Keiller, Dr S. Somerville. *Examiners*—The President, Drs Seller, Alex. Wood, Douglas, Paterson, Wright, Keiller, Pattison, Cumming, Duncan, J. W. Begbie, Haldane, Sanders, Wilson. *Treasurer*—Dr Andrew H. Douglas. *Secretary*—Dr D. R. Haldane. *Librarian*—Dr J. Matthews Duncan. *Curator of Museum*—Dr T. S. Wright. *Clerk*—Christopher Douglas, W.S. *Auditor*—K. Mackenzie, C.A. *Under Librarian*—John Small, M.A. *Officer*—Thomas Marshall.

## CONVERSAZIONE OF THE ROYAL COLLEGE OF SURGEONS.

On the evening of Friday, the 19th December, the President and Fellows of the Royal College of Surgeons held a conversazione in the hall of the College. Dr Newbigging, President of the College, occupied the chair. Dr John Smith, F.R.C.S.E., delivered a lecture on "Dental Organisation and its principal types in man and animals." The lecturer, after describing the development of the teeth as organs and as tissues, showed their determinative significance in zoological classification, and the services of palæontology in this respect. From the indications afforded by a tooth, he sketched in chalk the carnivorous and herbivorous type of animals; and concluded by describing some of the more remarkable varieties of teeth in existing and extinct species. The lecture, which was illustrated by diagrams and geological specimens, was very interesting, and was frequently applauded. At the close, the Lord Provost, seconded by the Hon. Bouverie Primrose, moved a vote of thanks to the lecturer,—a proposal which was cordially responded to.

## MEDICAL APPOINTMENT.

Dr W. T. Gairdner, Professor of the Practice of Physic in the University of Glasgow, has been appointed Physician to the Royal Infirmary of that City, in room of Dr Joseph Bell deceased.

## PUBLICATIONS RECEIVED.

Abeille,—Traité de Maladies a Urines Albumineuses et Sucrées. Par le Docteur J. Abeille. Paris, 1863.
Brande and Taylor,—Chemistry. By W. S. Brande, D.C.L., etc., and A. S. Taylor, M.D., etc. London, 1863.
Charrière,—Quelques Rectifications, etc. By F. Charrière. Paris, 1862.
Female Medical College, Fourteenth Annual Report of the New England. Boston, 1862.
Gay,—On Intestinal Obstruction by the Solitary Band. By John Gay, F.R.C.S., etc. 1861.
Gordon,—China in a Medical Point of View, etc. By Charles Alexander Gordon, M.D., C.B. London, 1863.
Jurors' Repórts,—On Surgical Instruments. London, 1862.
Laurence,—Lectures on Surgery delivered in St Bartholomew's Hospital. By W. Laurence, F.R.S., etc. London, 1863.
Marston,—On Ophthalmia. By Jeffery A. Marston, M.D.
Pathological Society of London, Transactions of the, for 1861-62.

## PERIODICALS RECEIVED.

American Journal of the Medical Sciences, —October. Philadelphia, 1862.
Births, Deaths, and Marriages, Monthly Return of, Nov. 1862.
Boston Medical and Surgical Journal,—November 13, 20, 1862.
British Medical Journal,—Nov. 29, Dec. 6, 13, 20. London, 1862.
Bulletin Générale de Thérapeutique,—Nov. 15, 30. Paris, 1862.
Donders and Berlin's Archiv.—No. 3, Vol. 2. Utrecht, 1862.
Dublin Medical Press,—Nov. 26, Dec. 3, 10, 17, 24, 1862.
Gazette des Hôpitaux,—Nos. 136 to 149. Paris, 1862.
Gazette Hebdomadaire de Médecine, etc.—Nov. 28, Dec. 5, 12, 19. Paris, 1862.
Gazette Médicale de Paris,—Nov. 29, Dec. 6, 13, 20, 1862.
Health, etc., of Manchester,—Oct. and Nov. 1862.
Henke's Zeitschrift für die Staatarzneikunde,—No 4. Erlangen, 1862.
Journal de Médecine et de Chirurgie,—Dec. Paris, 1862.
Journal de la Physiologie,—July. Paris, 1862.
Medical Times and Gazette,—Nov. 29, Dec. 6, 13, 20. London, 1862.
Nederlandsch Tijdschrift voor Geneeskunde, —April to Sept. Amsterdam, 1862.
Norsk Magazin for Lægevidenskaben,—Nos. 8, 9, Vol. xvi. Christiania, 1862.
Revue de Thérapeutique Medico-Chirurgicale,—Dec. 1, 15. Paris, 1862.

# Part First.

## ORIGINAL COMMUNICATIONS.

### Article I.—*On the Cerebro-Spinal Origin and the Diagnosis of the Protrusion of the Eyeballs termed Anæmic.*

By Thomas Laycock, M.D., etc. etc., Professor of the Practice of Medicine and of Clinical Medicine, and Lecturer on Medical Psychology and Mental Diseases, in the University of Edinburgh.

(*Read to the Medico-Chirurgical Society of Edinburgh, January* 7, 1863.)

During the last few years the attention of the profession has been directed to a disease in which there is more or less of a general debility resembling anæmia, considerable and varied nervous disorder, greatly increased activity of the heart and of the arteries of the head and neck, a vascular enlargement of the thyroid gland resembling bronchocelè, and staring eyes, with protrusion of the eyeballs, giving a peculiar expression to the face. This latter appearance, known as exophthalmos, exophthalmia, and proptosis, has attracted especial attention. No cause could be very exactly assigned for it; but since goitre is observed to be frequently associated with struma and other cachectic conditions, and anæmia with palpitation, the state of the eyes and the symptoms in general have been attributed to like causes. Hence the names which have been given to the affection. Thus Dr Mackenzie designates it anæmic exophthalmia; Datin, serous (or dropsical) exophthalmia; various French and German writers, cachectic exophthalmos and exophthalmic cachexia; Henoch, Schoch, and other Germans, *exophthalmos ac struma;* Basedow, the goggle-eyed cachexy. Amongst the foremost investigators, both in date and success, is our co-member Dr Begbie, the title of whose valuable essay on the disease, lately published, sufficiently indicates his opinion of its chief cause.[1] In expressing my dissent from his theory of causation, I am bound to say that I think he has exhausted the arguments in favour of it. The objections to the anæmic theory are, however, weighty. Cases have been observed in which there were not only none of the admitted symptoms of anæmia, but in which bleeding, restricted diet, and purgatives had a directly favourable influence on the disorder. Dr Begbie has disinterred such a case from the first volume of Johnson's Medico-Chirurgical Review.[1] Then the symptoms and signs which are relied upon as diagnostic of anæmia are of questionable value. These are, pallid countenance, œdema or anasarca, rapid pulse and faintness, breathlessness on slight bodily or mental exertion, and the so-called hæmic murmurs in the large vessels of the thorax and neck and the jugular vein. But these diagnostic phenomena are by no means so trustworthy as has been usually believed, if we attach any definite meaning to the term anæmia. If by this be meant degradation or diminution in number of the red corpuscles, or increase of white corpuscles or degeneration of the fibrin and other elements of the blood, known variously as leukæmia, leucocythæmia, spanæmia, hydræmia, and the like, then it is certain that none of the signs or symptoms commonly mentioned is pathognomonic. Of these, perhaps, the soft blowing murmur heard in the precordial region and the palpitations are the most relied on; yet those physicians who have specially directed their attention at the bedside to these so-called hæmic murmurs and anæmic palpitations, have had serious doubts whether in fact they are really consecutive to anæmia or spanæmia as a cause. In the clinical wards of the Royal Infirmary, this class of cardiac murmurs are, according to my experience, of quite infrequent occurrence as compared with the organic, and yet the cases of anæmia and spanæmia are numerous. And in the worst kinds of blood-diseases, such as splenic leukæmia or confirmed Bright's disease, when, according to the theory, we should surely observe them, they are comparatively absent. These objections apply with equal force to the arterial pulsations and the breathlessness which accompany the palpitations. Nor are the physiognomical signs more conclusive. Pallor is, according to my experience, by no means a necessary proof of impoverished blood. I have examined the blood microscopically of very many patients, nearly 300 I think, and have often been struck with the discrepancy in certain cases between the microscopic and physiognomical signs. In cases of remarkable pallor of the face and lips, I have been surprised to find that the blood under the microscope, so far as the state of the corpuscles can be ascertained, was healthy. Œdema is another sign greatly relied on as pathognomonic of anæmia, especially if conjoined with pallor; yet it is open to the observation of every one, and I have often pointed out the fact in the clinical wards, that there may be no œdema in the worst cases of blood degeneration, as proved by the microscope, and with extreme pallor; or the œdema, if it occur, may be of the most trifling character; while, on the other hand, extensive œdema may come on with no corresponding change in the blood. I have already published facts which prove that in one class of cases there would be no œdema or anasarca but for certain changes in the nerves of the part affected, or in the central axis, and that such changes will even cause œdema of the most extensive character to disappear.[1] This remark applies, indeed, equally to the other alleged signs and symptoms of anæmia; they may all result from disorder of the nervous system.

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[1] "Anæmia and its consequences: Enlargement of the Thyroid and Eyeballs;" in "Contributions to Practical Medicine," p. 116.

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[1] Under the head "Medical Miscellanies." Dr Parry also gives a case—"Collections from Unpublished Writings," etc., vol. ii. p. 125.

The same mistake in diagnosis is made as to the causes of, and the condition of the blood in, purpura and purpura hæmorrhagica. These are often consecutive to leukæmia and the worst forms of spanæmia, and are almost pathognomonic of the blood degeneration known as scorbutus; hence it was concluded, and indeed is still very generally held, that they are always connected with cachectic conditions. Yet, Dr Parry showed, fifty years ago, that there was a sthenic form of purpura to be relieved by blood-letting and antiphlogistics, which came on in persons who had been in no degree or manner exposed to the causes of scorbutus.[2] Further, there are some fallacies as to the causes of anæmia itself. In many of the recorded cases of anæmic exophthalmos, nay, in the majority, the patient was first subject to conditions mental or bodily which powerfully influenced the nervous system, then nervous symptoms arose, and then anæmia. Here the chain of events points to a morbid condition of the nervous system as the cause of the anæmia. That blood degenerations arise in this way is, I venture to say, as certain as that they arise from the causes ordinarily specified.

There are two forms of anæmia, it will be admitted by all, in which cardiac and vascular symptoms like those we have been considering do certainly occur. The one is that which follows upon very profuse loss of blood during parturition, and the other the anæmia of young women, termed chlorosis. But it is a very significant fact, that in the hæmorrhagic form the vascular disturbance is not restricted to the vessels of the head and neck, as it is in the disease under consideration, while in the chlorotic there are circumstances in the causation and course which render it doubtful whether it should not be classed with that disease. There is one sign, however, which merits special notice, because it is of as frequent occurrence as either the blood-defect or the vascular and nervous symptoms, and that is the increased production of heat. It is usually described as flushings or burnings, but it occasionally is so great as to excite a longing for cool air, or "air-hunger," as the Germans term it.[3] Now this is exactly like what is seen in cases of hæmorrhagic anæmia, in which the increased heat of body is most distressing to the patient. And it is most significant of the true character of this symptom, that this morbid increase of temperature is not only sometimes a certain forerunner of death, but in one class of cases is observed to take place after death, as in epidemic cholera and yellow fever. Perhaps the most striking example of this kind on record, is one mentioned by Dr Davy in his recently published work,[1] in which the post-mortem temperature (it was a fatal case of cellulitis with multiple abscesses) rose to 113° Fahr. Such a condition would seem to be conclusive as to the anæmic origin of the disease under consideration; yet when the inquiry is pushed further, even the increased heat is found to be a result of nervous lesion, as I shall shortly prove.

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[1] Clinical Lecture on Physiognomical Diagnosis. Med. Times and Gazette, May 31, 1862.

[2] Edinburgh Medical and Surgical Journal, vol. v. p. 7.

[3] Such a case of chlorotic exophthalmia is detailed by Withusen, in Dr Moore's Translation, Dublin Medical Press, July 13, 1859.

Looking at all these facts and considerations, it is not surprising that although the anæmic theory of causation is very generally adopted, there are those who think that the group of symptoms are more or less of nervous origin. Dr Stokes, in particular, entertained the opinion at an early period of the inquiry, and has expressed it in his valuable work on "Diseases of the Heart." The disease, according to his view, is a special form of cardiac neurosis, while he looks upon the exophthalmos and thyroideal enlargement as secondary phenomena, due to the cardiac disturbance. In this particular he agrees with Dr Begbie and other observers. Very lately, however, even these have been attributed to nervous disorder. The pathology of the affection was discussed, in this sense, during the year 1860 in the Paris Hôtel Dieu by Professor Trousseau.[2] Previously to this date, however, M. Kœben, in Archives Genrales for 1859, had attributed the affection to a lesion of the sympathetic, and M. Aran, the eminent physician to the Hospital St Antoine at Paris, lately deceased, expressed his concurrence in that opinion. Very recently, the pathology of the disorder has had a full examination in the Royal Academy of Medicine in Paris, on the occasion when a report upon it was presented to the Academy by Professor Trousseau, as convener of a committee appointed to investigate. The conclusions of the report were, that it is a *neurosis* analogous to hysteria, having its seat in the vaso-motor nerves and ganglia, and characterized by local congestions of nervous origin.

Other causes may be very plausibly assigned. In a certain class of cases, the symptoms and course seem to point to a rheumatic origin and complication, falling to be classed in this respect with certain kinds of chorea. In another class, the spleen seems to have been predominantly implicated; and in another, and indeed very numerous class, the generative system. As to my own experience and conclusions I may say, that although the exophthalmos has attracted the greatest attention, it appears to me very doubtful whether it is to be held as a pathognomonic sign, or as more than a local symptom and complication of a more general disorder, in which a certain definite tract of the cerebro-spinal axis is involved. In short, I think it is to be classed with squints, oscillating eyeballs or nystagmus, drooping lids or ptosis, and similar affections of the ocular-motor system, and which indicate either local or general lesion of the nerve-centres. As such, it is just as commonly a coincidence as a consequence of anæmia, and due to the same causes.

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[1] On some of the more important Diseases of the Army.

[2] Clinique Medicale de l'Hôtel de Paris.

Hence, besides the scientific interest attaching to the affection, it has a high practical value. Indeed, while investigating carefully the pathology of this curious group of morbid conditions, the conviction has been forced upon me that a knowledge of it will include the elucidation of some of the most fundamental and most important points both of the theory and practice of medicine. And with this end in view I shall, on the present occasion, consider the exophthalmos chiefly, reserving the bronchocele, the palpitation, and the other morbid states for separate and future consideration. It will be useful, however, to obtain, in the first instance, a general idea of the morbid states to be investigated. The histories of the recorded cases differ much as to details, but I think that which Dr Begbie first brought before this Society may be taken as affording a typical example of the affection when fully developed. The patient was a woman and aged thirty-two: she was childless, although married for six years and enjoying subsequently to marriage a fair state of health. For some time, however, she had had profuse leucorrhœa, and she had also suffered from continued anxiety and distress of mind. For four or five months previously to coming under observation, she had experienced inordinate palpitation of the heart, which was greatly increased by muscular efforts of any kind; at which times her face, otherwise pallid, flushed, and she became confused. The eyes were prominent and staring, giving a wild and startled expression to the countenance, the albuginea unusually visible and darker than natural, from a highly-developed vascularity. There was a painful sense of distention of the eyeballs, but no dimness of vision. These conditions were aggravated when the face was flushed, or the heart more violently excited. An enlargement of the thyroid was manifested *simultaneously* with these symptoms; but in the majority of the recorded cases it preceded the enlargement of the eyeballs, yet was consecutive to the palpitation. The gland rapidly increased in size, but varied much in this respect according to the state of mind, rest, or palpitation. It appeared to be highly vascular, and conveyed to the touch the sensation of an erectile tumour. The condition of the heart and vascular system was striking: the pulse generally ranged from 100 to 130; it was small and jerking, and, on the occasion of excitement, accompanied by a thrill. A beating was felt in the head, and there was a state of almost constant watchfulness. There was much breathlessness, frequent faintness, severe headache, vertigo, tinnitus aurium, a high degree of nervousness, and much derangement of the digestive functions. A soft bellows murmur was heard at the aortic orifice, and in the carotid and other large arterial trunks, but no increased sense of dulness, nor peculiarity of impulse. The catamenial discharge was imperfect and irregular, and leucorrhœa occupied the interval. The case terminated in health, under a tonic plan of medicine, diet, and regimen.

*Paroxysmal Exophthalmos.*—The exophthalmos and bronchocele may, however, be so trivial in extent, or so transient in occurrence, being in fact paroxysmal, as not to attract notice, while the other symptoms may attain to an alarming importance; and the blood-degeneration may be manifested by local hæmorrhages, the extreme palpitation may be associated with intense pain in the back or the precordial region, the increased vascular activity in the head and neck be accompanied by globus and other hysterical symptoms. When, therefore, as to such cases the exophthalmos and thyroideal enlargement are constituted pathognomonic signs, a misapprehension as to the true nature of the disorder may easily arise from the trivial or paroxysmal character of the morbid changes in the eyeballs and thyroid body. This misapprehension occurred, I think, in two cases of paroxysmal exophthalmos which came under my observation many years ago, and one of which was published.[1] One patient was a young girl of highly nervous temperament, who, at the age of eighteen, was admitted into hospital for a supposed inflammation of the knee. This had come on after a fright, at the age of seventeen, at the moment she was menstruating for the third time. It was accompanied by pain in the hip and back, shooting down the thigh, and was in fact an example of the "hysterical" knee described so well by the late Sir B. C. Brodie, for which she had during several weeks been heroically treated. Soon after admission she perspired so profusely that her bed and body linen would be completely soaked, and she would lie at the same time with only a sheet over her, on account of the great heat of the surface. This condition continued for many months under my own eyes. She also complained of dreadful palpitations, which were so severe sometimes as to stop her breath. The action of the heart was exceedingly tumultuous, and the pulse was from 115 to 155 per minute. From time to time the fingers and thumb of the right hand presented a mottled, livid appearance, and there was an aching pain along the arm; at other times the left hand was affected, becoming purple, while a deep-red blush extended along the forearm, with a sense of aching and smarting. At these times the perspirations were incredibly profuse, the pulse 144 to 158 and undulating. The action of the heart was distinct and forcible, but no murmur audible. Dyspnœa was very distressing, with acute pain under the left mamma, frequent syncopes, and globus. This state, during the latter periods of observation, passed into a sort of convulsive fit, when the throat swelled very much, and the eyes started from their orbits. No attention was paid to these appearances at the time, as they were attributed (and I am now satisfied erroneously) to the strangulation she experienced. They were evidently part of the paroxysm. This patient had a wonderful variety of symptoms in addition to those mentioned. The surface of the body was sometimes exquisitely sensitive; the tongue was always dry and brown, except at the edges, so that practitioners who saw this and felt her pulse invariably diagnosed the case as one of intense fever. Hæmatemesis, apparently vicarious, frequently occurred, and pain in the region of the spleen; loss of appetite was strongly marked, with a most unconquerable dislike to animal food,—a symptom noted by Dr Begbie as characteristic of the cases generally. Although the perspirations were so profuse and continuous, and the appetite bad, she was not at all emaciated. The head symptoms were like those in other cases—extreme wakefulness, vertigo, and the like, but no mental disturbance. The face was pallid when not suffused paroxysmally, which was often the case, when the perspiration would stand upon it in large beads. The sequel was never published, but she recovered a moderate share of health (having only a small bronchocele), married, and had children.

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[1] Case of Elizabeth Camidge, Edin. Med. and Surg. Journal, April 1838, p. 441.

The other case of paroxysmal exophthalmos that came under my notice was that of a respectable youth aged sixteen, who was extremely pallid, and had a small enlargement of the thyroid body. He was very nervous and excitable, and troubled with severe palpitation, which from time to time was so aggravated as to threaten suffocation. At this time the swelling in front of the throat became much larger, and the eyes, which always had a wild expression, became prominent and staring. The nervous and cardiac symptoms were attributed to anæmia and hysteria, and the exophthalmos and thyroideal enlargement to congestion, the result of dyspnœa. The patient recovered under change of air and tonics.

*Emotional Exophthalmos.*—The exophthalmos may be caused by emotion. A third case that came under my notice was pointed out to me in the street by a non-professional friend as a great curiosity; it was a woman, aged about forty, who was passing with large goggle eyes, which, he said, "had been forced out of her head by a fright." I afterwards took the opportunity of seeing her, and found that she had experienced a fright when pregnant, and that the eyes became prominent shortly after. She had also a large bronchocele, which, she alleged, also came on during pregnancy. She was anæmic-looking; her pulse very quick, her manner exceedingly nervous. Her case was of long duration, but how it ended I know not. The difference of age and of physiological condition due to married life and childbearing are important elements in a comparison of this case with the others. A fourth example may perhaps be referred to this head, which came under my notice, in the case of a young gentleman, aged twenty-three, a student of the university, who was in a state of great anxiety about his success at his examinations. He complained of extreme nervousness and violent palpitation. His face was suffused, his eyes had a peculiar glitter and stare, and on baring his throat I found a bronchocele the size of a St Michael orange. Pulsations were felt in the tumour, as well as in the carotids, in which strong vascular action was observable. He had tinnitus aurium, vertiginous sensations, and other uncomfortable head symptoms, all of which were much aggravated by study. The tongue was loaded, the breath offensive, the nights almost sleepless. There were marks of a rheumatic habit and a history of rheumatism, but no physical signs of heart-disease. Such a series of symptoms, however, in association with heart-disease and dropsy, especially in women, are not of unfrequent occurrence, and are, I think, due to the same causes as in others, but arise as complications.

*Classification of Cases of Exophthalmos.*—It will facilitate inquiry if the various conditions under which the exophthalmos occurs be arranged and classified. Now, taking an example like that observed by Dr Begbie as a standard of comparison, we can differentiate those that occur, or are likely to be met with in practice, into groups as follows:—

1. *Neuralgic and Hysterical.*—In this group there are neuralgic affections of different degrees of intensity. Sudden spasms or pains in the precordial region may accompany the palpitation, or orbital pains may complicate the exophthalmos. Or there may be the most striking hyperæsthesia of the skin generally, and of the special senses.

2. *Paroxysmal.*—The exophthalmos and bronchocele may be developed paroxysmally to a considerable extent, but subside so much in the intervals as hardly to be recognisable, or this may be the case as to the exophthalmos only.

3. *Orbital and Facial.*—The nervous symptoms, and especially the palpitations, may be less marked, but myopia, weakness of vision, and orbital and frontal neuralgia, accompany the exophthalmos. In these cases there may or may not be bronchocele.

4. *Cardiac and Cephalic.*—The symptoms referrible to the head and heart may be strikingly predominant, but the exophthalmos and bronchocele be little marked, especially in the earlier stages.

5. *Thyroideal and Cervical.*—Bronchocele, with pulsating thrills in the carotid arteries and the vessels of the thyroid, may be strikingly marked, but no urgent cardiac symptoms or exophthalmos.

6. *Complicated Cases.*—Complications may either precede or follow the local affections. (*a*) There may be uterine disorder. (*b*) The anæmic condition may be associated with hæmorrhages, especially meningeal, gastric, intestinal, and uterine. (*c*) Rheumatism and rheumatic affections may coincide. (*d*) Diathetic degenerations, chiefly atheroma of the arteries involved, as the aorta and ophthalmic and thyroideal arteries, and structural diseases of the thyroid, heart, and pericardium. (*e*) Disease, consecutive to these, especially dropsies and Bright's disease.

A true pathology of the disease, or group of diseases, must necessarily include an explanation of both the nature and course of each of the leading symptoms and their relations to each other, and of their complications. At present I refer only to the exophthalmos.

*The State of the Eyes in Exophthalmos.*—The affection is symmetrical, unless there be an orbital complication; the eyes are staring, —that is, the eyelids are more widely apart than natural, and the globe protrudes. But it is by no means necessary that there shall be both the stare and the protrusion. The lids only may be affected, so as to show the white of the eye preternaturally, and thus give a fallacious appearance of protrusion. The extent to which either symptom takes place varies remarkably with the emotional condition of the nervous system, and this is easily induced. Various mechanical theories have been advanced to explain the apparent or real protrusion; as, for example, dropsy of the globe, venous turgescence within the orbit, unusual deposit of fat behind the globe, and the like, but pathological anatomy has shown that none of these theories are sufficient. The change is doubtless in the motor mechanism of the eyelids and eyeball; but even the theory of this view is imperfect, partly because the morbid action of the palpebral muscles has not been taken into consideration, and partly because it seems probable that we do not yet know all the motor mechanism of the orbit. Mr Turner, assistant to Professor Goodsir, has lately demonstrated that there is an unstriped muscular structure in the orbit hitherto overlooked, which, from its attachments, he designates the periosteal muscle. If Mr Turner's dissections be confirmed, this periosteal muscle must be held to have considerable influence in protruding the globe.[1] What, however, we certainly can say, is this, that both the paralytic and spasmodic action of the muscles involved must be considered; so that retraction of the eyeball is the correlative of protrusion, and a drooping eyelid or ptosis of a wide-open eyelid or proptosis.

*Causes of Exophthalmos.*—These may be investigated either by observation or experimental research, and here it will be useful to remember that protruded and staring eyes and their correlatives are by no means of uncommon occurrence. In cases of death from strangulation the eyes are seen staring and prominent. Under a powerful emotion of fear or terror, and even when severe bodily pain is felt, the eye stares. Then there are cerebro-mental diseases involving the emotions and the intellect, in which the same appearance is so common that the expression of the countenance in cases of exophthalmos is described by several writers as being wild and maniacal. As to all these various instances it is a fair presumption that in each the same mechanism of the eyeball is influenced through the nervous system. There are other kinds of symptomatic exophthalmos which may arise from somewhat different conditions; as, for example, that which is so strikingly manifested in some cases of chronic mania complicated with epilepsy. Sir A. Morrison gives, in his "Physiognomy of Mental Diseases," an illustration of this kind (plate xi.) It is the case of an epileptic, aged 60, subject for many years to periodical attacks of furious mania. In plate E of the Appendix there is a delineation of the staring eye, with insane fear, after Hogarth. A remarkable prominence of the eye is also observed in certain forms of mania with general paralysis, and this, according to my experience, may be seen even in persons predisposed to the disease. In none of these symptomatic forms of exophthalmos is there reason to think the eyeball is enlarged, any more than in the emotional exophthalmos. The symptom is due, therefore, to changes in the motor mechanism of the eyeball and eyelids occurring consecutively to changes in that part of the nervous system more particularly connected with the eye; and, consequently, the primary questions for solution are what are the nerves, and what the nerve-centres involved in the affection?

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[1] Natural History Review, January 1862, where Mr Turner ably discusses this question.

*Pathological Anatomy.*—Now, I shall proceed to show that a solution has been rendered at least possible of late years, by a series of brilliant researches into the functions of the nervous system, conducted by Stilling, Claude Bernard, Schiff, Budge and Waller, Brown-Séquard, and others. And I think in bringing these under the notice of the profession, and showing some of their varied applications to the practice of medicine in general, as well as to the elucidation of the symptoms under consideration, I shall be excused some little divergence from a bare statement of facts. A very distinguished anatomist and physiologist in his day, Pourfour du Petit, was the first, in 1727, to show the influence of injury of the cervical portion of the sympathetic system on the eye.[1] He observed contraction of the pupil, retraction of the globe, and redness of the conjunctiva, to result. These observations were confirmed by numerous subsequent experimenters; but, in 1845, M. Biffi of Milan added the important fact that when the iris was contracted in consequence of division of the cervical sympathetic, it became dilated if the upper end of the nerve was galvanized. In 1852, Professor Claude Bernard further elucidated the question. He ascertained that the surface became more sensitive and the vessels fuller of blood; that the small arteries pulsated with greatly increased force; and that the temperature was greatly raised on the side of the head operated on, both externally and as to the parts within the cranium; that as to the eyelids and eye there was contraction of the pupil, a narrowing of the opening of the lids, retraction of the eyeball, a projection over it of the third eyelid which the animals operated on possess, and a flattening of the cornea. But when the upper end of the nerve was galvanized these conditions were reversed, and there resulted diminished heat and vascularity, dilatation of the pupil, enlargement of the opening of the eyelids (or staring eye), and protrusion of the globe; in short, he produced exophthalmos.[2] And the tendency to it was so forcible under these circumstances, that the phenomena were manifested in an animal which held the eye tightly shut, in consequence of a drop of liquor ammoniæ having been dropped into it. Here, then, we have a clue afforded as to the nerves and the muscular mechanism involved in nervous exophthalmos.

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[1] Histoire de l'Acadmie des Sciences, 1727. "Memoire dans lequel il est démontrè que les nerfs intercostaux fournissent des rameaux qui portent des esprits dans les yeux."

[2] Comptes Rendus, tom. 36, p. 375.

Nor is this all. In the increased sensibility, heat, and vascular activity of the side of the head operated on, there is a proof of the influence of local morbid states of the nervous system on the development locally of those phenomena as symptoms. Now, these are leading points to be investigated, and at the first glance it might be supposed (as I certainly was led to think), that the exophthalmos and increased vascular activity in the head, neck, and face observed in these cases, might be attributed to the pressure of the enlarged thyroid on the cervical sympathetic, or to some other lesion of that nerve sufficient to influence its functions. The objections to this theory, however, are manifest. Not only have we numerous examples of very large bronchocele without such results, but exophthalmos frequently occurs when there is no bronchocele. Besides, if this cause be admitted (and in some instances at least it may be a cause), how comes the heart to be in pathological relation with the eyes? In truth, until the last few years, the attempt to solve the questions thus arising was hopeless; happily experimental research has demonstrated a hitherto unknown physiological connexion between the eyeball and lids, and a definite tract of the spinal marrow. Two eminent physiologists working together, viz., Dr Waller and Professor Budge, discovered that if they galvanized that portion of the spinal cord of a rabbit which extends between the first cervical and the sixth dorsal vertebræ, they excited dilatation of the pupil. They, therefore, designated this region the "cilio-spinal region." The point of maximum intensity was found to correspond in the frog to the articulation of the second and third dorsal vertebræ, and to the origin of the second dorsal pair of nerves.[1] Following up these researches, Prof. Claude Bernard has recently traced a connexion in the dog between the anterior roots of the two first pairs of dorsal nerves and the muscular mechanism of the eye. If these were divided on one side without injuring the spinal cord or the sympathetic in the thorax, the eye of that side is affected, the pupil contracts, the opening of the eyelids narrows, and the eyeball is retracted and diminished; but there is no increased sensibility, heat, or vascular activity induced. And when the peripheral ends of the divided nerves are galvanized, the same results follow in the eye as when the upper end of the cervical sympathetic is galvanized,—that is to say, widening of the eyelids, dilatation of the pupil, and exophthalmos. Hence the conclusion, that the motor nerves of the eyes of the dog originate and decussate in the spinal cord at the dorsal region. It is necessary, however, according to the results of Prof. Claude Bernard's researches, to distinguish between the sympathetic and spinal nerves of the eyes. When he divided the ascending trunk of the sympathetic on the side of the vertebral column between the second and third rib, carefully avoiding the spinal cord and nerves, he induced increased sensibility, heat, and vascularity of the ear and lateral half of the head on the same side, but no change in the eyelids, eyeball, or pupil. Hence the conclusion, that the nerves subservient to heat and vascular activity (the vascular and calorific nerves) are sympathetic, those which regulate the movements of the eye are spinal. But these motor nerves are equally with the vascular (including the cardiac nerves), and the calorific in relation with that portion of the cerebro-spinal axis which influences the circulation and muscles of expression when emotions are experienced; and this is true, even of the emotional state of feeling termed pain, for Claude Bernard found that if any sensory nerve be pinched from the sciatic to the fifth, reflex ocular movements result, constituting dilatation of the pupils and widening of the eyelids. I do not propose on this occasion to explain the connexion between these conclusions and the causes of the palpitations, arterial pulsations, vascular bronchocele, heat of surface, and increased sensibility, and other symptoms which accompany the exophthalmos, because the subject merits a separate consideration. I would only observe that it has been long known that the heart and vascular system are the seat of various functional disorders of a painful kind, in cases of so-called spinal irritation, especially in young women predisposed to hysteria and hysterical affections. In such patients, nervous and hysterical palpitations, in all respects resembling those of the disease in question, exclusive of the bronchocele and the exophthalmos, are very frequent. Many years ago, I pointed out the fact that the motor fibrils of the heart are derived from the spinal cord as well as from the sympathetic, and thus tried to explain the character and origin of these nervous or hysteric palpitations, and indicated the diagnosis.[1] All the most recent researches on this point tend to establish these views, and to prove that the heart is in direct relation with a special motor tract of the spinal cord, commencing and continuous upwards with this oculo-spinal region. Consequently, any sufficient causes acting morbidly upon that portion of the cord, or upon the cerebral connexions within the cranium (as during emotions), or upon the motor roots of the two or three first pairs of dorsal nerves, would tend to induce not only the "oculo-pupillary changes," but also increased vascular activity in the region of the cervical sympathetic, and palpitation of the heart. These remarks apply with equal force to certain cephalic symptoms, and which are quite as prominent as the conditions just examined; such as tinnitus aurium, flushes, nervous headaches, neuralgia of the face and orbits, and wakefulness; and also to the precordial anguish, painful sensibility of the trunk, and neuralgia of the spine and upper

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[1] Comptes Rendus, tom. 33, p. 372.

[1] On the Nervous Diseases of Women, p. 270. London, 1840.

extremities. Some of these are due to morbid conditions of the cervical sympathetic primarily, others to affections of the ganglia on the posterior roots of the spinal nerves (the intervertebral ganglia), and of the sensory tract, occurring either consecutively to the vasomotor neuroses, or arising independently. The whole subject is, however, too large in extent to be examined now.

*Orbital Exophthalmos.*—It is necessary, however, to an accurate diagnosis of the various forms of staring and protruding eye, to examine the relations of the eyeball to the Gasserian ganglion, which is the ganglion of the posterior roots of the nerves of the face; for, undoubtedly, a species of exophthalmos arises in consequence of diseased states of that ganglion. This connexion was first demonstrated experimentally by Majendie in 1824, and pathologically by M. Serres in the following year. Of later years Schiff has very successfully investigated the subject. As the experiments of Professor Claude Bernard are more accessible, I need only remark that, by an ingenious method of operating, he divides the trunk of the fifth nerve in rabbits and dogs: the result is, that a degree of exophthalmos immediately takes place, and the cornea becomes more conical.[1] It is, however, of much diagnostic importance to observe that the vascular phenomena which accompany this kind of protrusion of the eyeball are very different from those already described as of spinal or sympathetic origin; for, though turgescence of the vessels of the conjunctiva takes place, the heat and vascular activity of the tissues involved are not increased, but strikingly diminished; so that, when the animal is in bad health, inflammation of the conjunctiva, with opacity and necrosis of the cornea, follow, and the eye is destroyed. And there is a probability, at least, that the effects of the injury are felt in the brain beyond the orbital tissues. Now, as structural disease of the ganglion has been proved by Serres and others, as also my own observations show, to be followed by similar changes, we have in these facts a ground for diagnosing trigeminal or orbital exophthalmos from sympathetic or spinal. In several recorded cases there were myopia, neuralgia, œdema of the eyelids and conjunctiva, passive congestions and inflammations of one eye, or of one predominantly, if both were protruded. Such phenomena enable us to distinguish a trigeminal from a sympathetic or spinal exophthalmos, because, in the sympathetic kind, the nutrition of the eye is not only wholly unaffected, but the increased vital activity of the tissues enables them to resist cold and the other causes of inflammation, even although not covered by the lids. This peculiarity of the exophthalmos under consideration is particularly noticed. When the fifth is affected, the results are wholly different. The case communicated to Mr Fisher by Dr Désgranges was probably of this trigeminal kind.[2]

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[1] Leçons sur la Physiologie et la Pathologie du Système Nerveux, 1858, tom. ii. p. 51, et seq.

[2] Archives Generales, 1859. Exophthalmie Cachectique, *Obs.* v. p. 533.

*Sources of Fallacy in Diagnosis.*—It is probable, however, that in a certain class of cases both kinds of nerves are involved, and then the diagnosis will be doubtful. For, when this takes place, there will probably be a remarkable modification of the phenomena as caused by either singly. If, for example, congestion, a lower temperature, defective nutrition, and other changes, have occurred in the eye from injury of the fifth nerve, a lesion of the cervical sympathetic will relieve the congestion and restore the temperature, or, in other words, will antagonize the morbid influence. Claude Bernard found that the temperature of the left ear of a dog fell 3° C. below the right ear after division of the fifth on the left side; but, when he divided the left cervical sympathetic, the temperature of the left ear rose 6° C., while that of the right fell 6° C.[1] If, therefore, a case were met with in which both kinds of nerves were involved, the pathognomonic signs of neither the one nor the other class of lesions would be manifested, and thus the singular result would follow of serious nerve-injuries so antagonizing each other as to remain totally obscured. Another source of fallacy in cases of centric origin will be found in a physiological fact almost wholly, if not altogether, overlooked by physiologists, but which I have seen so often illustrated by disease, that I am satisfied it is of frequent occurrence, and that is the crossed action of the sympathetic nerves and ganglia, owing to decussation of their fibrils and commissures in the cerebro-spinal centres.[2] I have seen remarkable examples of this crossed action in anasarca; as, for example, in a woman in the clinical wards in November 1861, who had valvular disease and the most extreme œdema of the extremities. She had an attack of hemiplegia of the left side, which was due, as the post-mortem examination proved, to plugging of the right middle cerebral artery. Now, there was increased œdema of the right or non-paralyzed leg and arm, but the left leg, which was wholly useless, ceased to be anasarcous, and became quite small. Here the injury to the right hemisphere plainly led to the change in the left limb. To what extent these central changes in the vaso-motor system influence the states of the circulation in distant parts is as yet wholly uninvestigated, but I am inclined to think they modify the production of exophthalmos and the vascular changes in at least certain cases of general paralysis.

*Ptosis or Drooping Eyelid.*—The condition of the eyelid, as distinct from that of the eyeball, is of importance in diagnosis; there may be a staring eye without a protruding globe. Now, taking the results of Professor Claude Bernard's experiments as a guide, I think it may be inferred that the drooping or partially closed eyelids may be as pathognomonic as the widely open; nay, perhaps more conclusive, inasmuch as paralysis of the cervical sympathetic is much more likely to occur as a morbid state than that half-tetanic, excited condition, which galvanization induces. And it is perhaps for this reason that the staring eye is comparatively so rare. A drooping eyelid, therefore, with or without retraction of the eyeball, together with greatly increased vascular activity in the neck, head, and eyes, may be more pathognomonic than the staring eye with its widely open lids. In the case of paroxysmal exophthalmos or proptosis already referred to, symmetrical ptosis or drooping lid was a very striking symptom when there was no excitement. When one eyelid is affected the lesion is probably local. I had recently an example of this kind in the case of a female patient sent to me from Ireland by a medical friend. He informed me that she had long suffered from dull heavy frontal headache, aggravated by stooping, and accompanied by vertigo, muscæ volitantes, and ringing in the ears, and sometimes with nausea and vomiting. At the same time she had a good deal of thirst, and habitually cold feet. A confined state of the bowels and weakness of the back had come on during the last three years, and, within the last three months, drowsiness, tenderness of the eyes on pressure, intolerance of light, and ptosis, particularly of the left eye, leading to the suspicion of obscure cerebral disease. On examination I found that there was a thick neck and a small bronchocele, of which the right half was larger than the left. The ptosis was now limited to the left eye, the patient being unable to raise the lid, and there was increased heat on the left side of the head. After treatment with extract of chamomile, nux vomica, and sulphate of iron, in small doses, the ptosis and other symptoms disappeared, and the enlargement of the thyroid was now restricted to the right lobe. It is worthy of notice that thyroideal and glandular enlargements had occurred in three or four of this patient's relatives.

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[1] Leçons, etc., tom. ii. pp. 480–482.

[2] See this question mooted in my "Mind and Brain," vol. ii. pp. 400 and 405.

*General Conclusions.*—It may be inferred from these facts, 1. That the exophthalmos under consideration is specially due to disorder of the nervous system. 2. That it varies in character and diagnostic significance accordingly as it is associated or not with other phenomena involving the vascular system of the heart, and of the eyes, head, and neck—the carotideal as distinct from the vertebral system of capillaries. 3. That it is sometimes of spinal, sometimes of cranial origin; and that in either case its nature and seat may be diagnosed. 4. That it occurs under a variety of morbid conditions of the nervous system.

If it might be permitted to theorize on the causes of symptomatic exophthalmos from these data, we might conclude that, when it occurs in strangulation, it is probably due to mechanical injury to the cervical sympathetic by the tightened cord or other violent means used; in the emotional form the condition is probably like that when the sympathetic is galvanized, the face being pale, and the eye staring; in certain morbid cerebral conditions, such as mania, with epilepsy and general paralysis, the lesion is probably in the first instance paralysis of the sympathetic, and subsequently of the fifth and seventh; and, finally, that in the class of cases under consideration, when the exophthalmos is symmetrical, it is spinal; the cervical and upper dorsal region being the seat, together with the corresponding cervical and dorsal divisions of the sympathetic; but when unsymmetrical, it is due to disease of the trigeminal ganglion and branches of the fifth pair.

[*Note as to the state of the Eyes in cerebral mental diseases.*—Three years ago, my friend Dr Rorie, superintendent of the Dundee Asylum, examined, at my request, the eyes of patients under his care, in respect to their prominence, state of pupil, and other matters. Of each sex, 86 were examined; of the females, 26 had prominent and 4 very prominent eyes, together, nearly 35 per cent.; of the males, 11 had prominent and 6 very prominent eyes; showing the considerable excess of 15 per cent. amongst the females. It is required to know, however, what is the natural proportion, absolute and relative, before we can determine how far these states amongst the insane are morbid. Changes in the pupils were also observed by Dr Rorie, and he found that there was a difference in the two eyes. Of 7 females, the left pupil was more dilated than the right in 5, and of 8 males 6; or, in other words, the left was more dilated than the right in the proportion of 11 in 15. But then he found also that the same difference could be observed in persons apparently healthy. One only in 172 patients had sunken or retracted eyes, and he was formerly subjected to paroxysms of rotatory movements, and one, an epileptic male, had the left eye more prominent than the right.]

RUTLAND STREET, *Edinburgh, January* 1863.

## ARTICLE II.—*On the Epidemic Influenzas of Iceland, especially the last one of* 1862. By JOHN HJALTELIN, M.D., Medical Officer of Iceland.

AMONGST all epidemical diseases of this century, few are more interesting than the epidemic influenzas, especially since it is evident that they get more and more frequent. In the history of this disease, Iceland has hitherto, on account of its natural isolation, been overlooked by the epidemiologists; but I think, nevertheless, that there are some interesting hints regarding this malady, which, if carefully collected, might be of some interest to medical science, and it is therefore that I venture to give some aphoristic description of the Icelandic influenzas, and of the late epidemic of this kind.

On entering on a short historical account of Icelandic influenzas, I must remark, that although the Icelandic people have been very sedulous, and even accurate, in collecting historical notes from all the northern countries of Europe, their ignorance in medical sciences has prevented them from treating our epidemiology with due accuracy and precision; and it is therefore that we in our long, and, in many respects, very interesting annals, must rest satisfied with short hints on this subject.

The first account I have been able to find out in our annals regarding epidemic influenzas goes back to the year 1627, when the malady is stated to have been prevailing in the northern part of this country, where it is said to have occasioned a great mortality. About the same time, the influenza raged in Italy also; but forty years later, or in 1669, the influenza most likely again made its appearance in our country; for it is stated that, during the winter, there was a very common sickness, with some roseola, which very often is still associated with the Icelandic *grippe;* and it is well-known from the writings of Fanossius Guido, in his Monography, "de Morbo Epidemico Hactenus Inaudito," that influenza in this same year raged over a great deal of Germany and many other northern countries. Again, in the years 1705 and 1719, influenza is mentioned in our annals; but being only said to have visited the northern part of this island, I am inclined to think that this has perhaps been a malignant catarrh. In 1730, an epidemical disease is said to have begun in the south part of Iceland, going from thence to the country districts and the more northern regions, which is the most common course for our influenzas; and this disease has therefore undoubtedly been an influenza, which supposition is strengthened by the well-established fact, that this same disease one year before, or in 1729, was spread over the whole of Europe. It happens very often, as we afterwards shall see, that the most malignant influenzas arrive one year later in our country than they do in the continent, and are therefore prevailing here when they have already finished their ravages in the other countries of Europe. Few years afterwards, or in 1735 and 1736, when the renowned European influenzas of 1732, 1733, and 1737 had made their ravages on the continent of Europe, and in America, Iceland was also visited by two epidemics, which, no doubt, have been of the same nature.

During the year 1775, an epidemic influenza visited most of the countries of Europe, and in 1776 it reached this island; but being then, as it very often is, associated with roseola, or rubeola spuria, it was looked upon as a sort of morbilli. I doubt very much whether the great and renowned European influenza of 1782 also reached this island or not; some believe it did so in the same year as it ravaged Europe; and it is said to have been complicated with epidemical diarrhœa and dysenteria, especially in children. Again, in 1802–1805, when an epidemical influenza was very common in Russia and France, Iceland was also in 1809 visited by the same disease.

In this century the influenza has about seven times visited our country, namely, as aforesaid, in the years 1804, 1816, 1825, 1839, 1843, 1855, and this year 1862. Only two of these epidemics, viz., that of 1816 and that of 1855, seem to have been confined to this country, while those of 1834 and 1843 were spread over the whole of Europe and America; and it is, moreover, very remarkable, that while their visit to the continent is commonly of one or two years' earlier date than to our country, their appearance here is always of the same date as in North America. It is, moreover, an undeniable fact, that all our epidemical influenzas, which coincide with those of the continent, are more malignant and dangerous than those which only are confined to our island. Many epidemical influenzas are mentioned in the medical annals of Europe which never reached Iceland. The influenza of 1837, which I myself observed in Copenhagen, did not touch Iceland, although it ravaged a good deal of Europe.

The epidemical influenzas of 1843, which made great ravages in this country, seem also to have been very bad in North America, but did not, as far as I know, invade the continent of Europe. The epidemic of 1855 was very like that of 1816, for although very common, they were both mild, and only occasioned a small mortality. The epidemics of 1825, 1835, 1843, and 1862 have all, on the contrary, been very malignant and dangerous.

After this short account of all our known epidemical influenzas, I now proceed to describe the last epidemic, which began in the month of May this year, and continued about three months to travel over the whole island; but, in so doing, I shall, as far as I am able, describe all the circumstances which anteceded its outbreak.

After a mild winter, we got a very cold spring; the average temperature of April being only 2° of Celsius (28°·4 F.) Heavy northerly gales swept over the country, and the thermometer fell often during the night down to —8° of Celsius (17°·6 F.) The barometer indicated a constantly heavy atmospheric pressure, and the ozonometer showed a great deal of ozone in the air. About the end of April the season at once grew milder, and about the 6th of May the thermometer rose up to +10° of Celsius (50° F.) At this time, I was travelling on horseback from Reykjavik to Cape Reykjanes, in order to look at some patients affected with typhus. On the 10th of May, I was returning to Reykjavik on a clear, hot, and beautiful morning; but when I had nearly half-finished my way, a dry offensive fog, thick and ill-smelling, surrounded me; this fog was very cold, and so thick, that the beams of the sun could scarcely penetrate it. My guides, who instantly felt the bad effect of this fog on their respiratory organs, proposed to stop at a farm near us, but, although I had the same bad feeling, I was not inclined to do so, and ordered them to continue our voyage until we reached the town. On the 11th of May, which was Sunday, the inhabitants of Reykjavik made in the church, by their hard, dry, and frequent cough, a very bad noise during the holy service; and on Monday, which was a hot and agreeable warm day, many of the inhabitants were confined to their beds. That same day, prodigious swarms of flies, viz., the "Blapsater," filled up the small streets of our little town, and continued like dark clouds to surround the houses during two days, after which time they covered the streets with their dead bodies, like a heavy fall of a black augitic or volcanic sand. This phenomenon is, I think, very remarkable, for it is many times before observed during the outbreak of influenza. Thus Jussien states that the grippe of the spring 1733 was anteceded in France by offensive fogs, "more dense than the darkness of Egypt;" and Petit says, that the influenza of 1775 was ushered in by thick and noisome fogs. In the same year, a continual dark fog, particularly smoky, anteceded the outbreak of this disease in Scotland. More authors, but especially Dr Darwin, have recorded the dark and dry fog which obscured the sun before the ravages of the influenza in 1782, and he supposes "that the material which thus rendered the air unwholesome and muddy, probably caused the epidemic catarrh which prevailed in that year." Some sensible people in this country have, moreover, told me, that the influenza in 1843 was in like manner ushered in by continuous dark and noisome fogs. Be this as it may, there is, in my opinion, no doubt that this our last epidemical influenza was in some way or other occasioned by the aforesaid dense and offensive fog; and it was, moreover, very remarkable, that the smell of this fog, being quite unlike common fogs, was that of a highly concentrated ozonic gas, which immediately irritated the mucous membrane of the lungs, and most likely also the other mucous membranes of the body.

When the epidemical grippe had lasted for four days, most of the inhabitants of Reykjavik and the whole parish suffered more or less from this disease.

The influenza was generally ushered in by a sudden attack of a sense of cold down the back, and between the shoulders, succeeded by heat and dryness of the skin, prostration of strength, and some slight pain in the head. Even those who had the slightest attack of the malady found their strength much abated, and were indisposed to walk or work. All complained of a severe soreness in the throat, with sense of constriction around the chest, copious defluxion from the nostrils, frequent sneezing, with epistaxis and hard and dry cough. The cough was usually followed by an expectoration of a copious glairy fluid, which at a later period became an opaque mucus. When the constriction of the chest or the soreness in the throat was in a high degree, they were always succeeded by a very frequent and troublesome cough, with difficulty of breathing and lividity of the lips. The respiration then became accelerated and short, even in those who were still able to leave the bed and walk a little about, or take care of their inside business. The auscultation then showed a very much increased but incomplete dry and harsh respiratory sound, with sibilant and sonorous rhonchi, clearly audible on forced inspiration. In all those who were seriously attacked the vesical murmur became very soon extremely indistinct, and was mixed with a dry subcrepitant rhonchus, which, in the most serious cases, was associated with "bruit de frottement."

One of the most common symptoms, in many of those who were seriously attacked, was the great mental depression which very often terminated in such an apathy that the patients seemed careless of their sufferings and approaching death. This was especially the case with patients more advanced in life, and this symptom was clear and distinct enough to make the physician aware of the approaching danger. When this apathy (as it generally was in more or less degree) was by the nurses taken as a sign of an insignificant catarrh, it often happened that I was first requested to look at the sick when the agony had just begun. Sometimes the patients themselves declined to take any medicine, because they believed that it was of no use: "We have no pain" was their answering, "and there is therefore no reason for us to take medicine; could you only give us something to relieve our cough, we shall very soon get better." Others who had great dyspnœa suffered much during their restless nights, and complained of the harsh cough and difficulty of breathing, which compelled them to take a more erect position in the bed, and prevented them from sleeping.

The feverish symptoms showed a great deal of variety: the pulse was sometimes very frequent, from 100 to 120 beats in a minute, seldom very tense, but generally easily compressible. The skin was generally moist, and sometimes covered with copious perspiration. All the feverish symptoms had a marked type, subsided in the morning and aggravated in the evening, followed by restlessness and neuralgic pains, especially lumbago, otalgia, odontalgia, neuralgia intercostalis, hemicrania, and facial neuralgia.

All the mucous membranes of the body were, in the more severe cases, in an evident hyperæmic state. Not only the mucous membranes of the air-passage, the stomach, and the whole intestinal canal, but also the mucous lining of the uropoetic organs, were in a state of hyperæmia and congestion. The consequences of such a morbid state in the kidneys were, in my opinion, very bad, and gave birth to dangerous accumulation of urea in the system. The symptoms of a prevalent and even malignant catarrh, not only of the bladder, but also of the whole uropoetic organs, were clear enough. The urine voided in the most severe cases during twenty-four hours seldom exceeded six or eight ounces; it was of a great specific gravity, and deposit a good deal of earthy phosphates; when voided, it sometimes occasioned a burning sense in the urethra, and all patients who were thus affected complained of a severe pain across the loins, and even in the lower part of the abdomen, and there was therefore a great suspicion of a quite specific renal affection, most likely consisting in an hyperæmic state of the calyces renum and tubuli uriniferi. I am, moreover, inclined to think that this specific renal affection is a very suspicious complication in influenza; and it seems to me that the great loss of muscular power, which is so very common a symptom of that malady, may often arise or increase from the accumulation of urea in the system. In the meantime, it is well-known now-a-days that the retention of urea in the blood, as pure urea, must be abandoned, and that the poisonous symptoms, formerly ascribed to the presence of this stuff in the system, must now be attributed to the carbonate of ammonia, into which the urea is liable to be transformed in the blood; for it is proved that an injection of a solution of the carbonate of ammonia into the veins of animals produces the same symptoms as were formally attributed to the presence of urea in the blood. The great mental depression formerly spoken of may, I believe, arise from the same cause, but exact observations regarding this are still wanting. I am, in the meantime, inclined to think that this may be proved by the unexpected action of some remedies against this symptom. It happened sometimes that I made use of chlorine-gas, in order to purify the impure atmosphere in those small fishermen's huts, where many patients were crowded in small rooms; and I several times observed that their dyspnœa and depression were much mitigated. This may, I think, be explained by the destruction of carbonate of ammonia in contact with chlorine-gas, which in such cases would be converted into chlorate of ammonia. Be this as it may, the fact was clear enough, and was, besides this, combined with another experience of somewhat analogous but also unexpected nature. I had, namely, in several patients used diluted sulphuric acid in water, or the so-named "mixtura acidi mineralis," and this medicine was given in order to lessen the profuse and debilitating perspiration. This physic did the patients a great deal of good, and seemed generally to work better than the usually given common anticatarrhal remedies, as kermes, (sesquisulphuret of antimony), squilla, ipecacuanha, liquorice, muriate of ammonia, senega, tartar-emetic, colchicum, etc. etc. This was many times observed by myself and my assistant physician; so we very soon got the conviction that we might in the beginning of influenza use this remedy as well as any other medicine, and it was therefore generally ordered much diluted, in combination with some mucilage, and one or another sweet syrup. When the urinary secretion was very scanty, I had it combined with sweet muriatic naphtha (spiritus salis dulcis), so that it might act as a mild diuretic drug.

There is no doubt that the fever which followed the influenza was better mitigated by this drug than by most other remedies, and it never aggravated the cough, as one might have imagined.

It often happened that the mucous membrane of the intestinal canal was also severely attacked: mucous diarrhœa was then the result, with great tenderness in the colon, white furred tongue, cardialgia, and vomiting; such cases seldom finished in true dysentery, but lasted as chronic diarrhœa in a few cases for a longer period.

Besides the affections of the mucous membranes, the serous membranes of the body were also acted upon by the influenza, and often indeed in a most severe manner. Serous effusions in the pericardium and in the cavity of the pleura were extremely common, and the collection of the fluids sometimes happened without any clear signs of previous inflammation. It struck me often, in the beginning of the epidemic, to find patients who one day seemed only attacked by a severe bronchitis, with its usual auscultatory signs, but had next day all the symptoms of serous effusions either in the pericardium or in the cavity of the pleura. I first doubted in my diagnostic of such an unexpected event; but the autopsy made me soon believe my own observations, and encouraged me to rely better on the percussion and auscultation. I have little doubt that this phenomenon was in direct combination with the lessened urinary secretion, and was due to some paralytic affection of the absorbent system. This was the most dangerous and insidious complication I met with in this epidemic, and it must have been very fatal where no medical aid could be opposed to it.

As to the real cause of this epidemic, I must refer to my aforesaid opinion; but, although I generally look upon it as an atmospheric miasma, I must also insist upon its contagious nature. The proofs of such a real contagion were clear enough, and could not be denied: the malady advanced in many places very slowly, step by step, and was evidently transported by travellers who came from the first infected places to the more remote ones. This is an undeniable fact, which may easily be observed and stated in a country like Iceland, where people are living at great distances with rare and very slow communication. The same phenomenon has been observed in all the former epidemical influenzas, and there cannot be the slightest doubt about their contagious nature. The influenza had this year quite finished in Reykjavik before it began its ravages in the west part of our island. When the influenza is occasioned by contagion, its latent stage seems to be very short, and the outbreak of this malady follows, as it were, immediately after a sick person's arrival in a sound locality.

Regarding the mortality in this our last epidemic, it was in many places very high, and in some parishes no less than 4–6 per cent. of the whole population. In the parish of Reykjavik it was little more than 1 per cent., and, as the women were more liable to it than men, the mortality amongst them was greater than amongst men; children between one and two years, and old men and women, were most generally badly off, and the mortality amongst them was far greater than in young people or individuals about the middle age.

My treatment of the influenza was, in the beginning of the epidemic, very variable, according to the severity of the cases. The milder antirheumatical and anticatarrhal remedies as nitre, chlorate of ammonia, ipecacuanha, senega, or linctus pectoralis, and decoction of salep, were first called into use; but when they seemed to have very little effect to alleviate the symptoms, stronger remedies, as calomel with opium or digitalis, golden sulphuret of antimony and kermes, were administered; and when feverish symptoms were very urgent, the aforesaid "mixtura acida" was also used. The most effectual remedies were, in the meantime, sulphate of quina and veratria. On account of the strong recommendation of quina by Sir Henry Holland, I was at first induced to try this remedy in our epidemical influenza of 1855; and as I then was fully convinced of its great power against that great prostration of strength which always follows every severe attack of influenza, I made an extensive use of it. Every day during the epidemic, I took myself five grains of bisulphate of quina, and I think that it was by virtue of that remedy that I could bear the many hardships I had to undergo during the course of the malady, not only in the town itself, but also in the neighbourhood, where I was the whole day in activity, from 7 and 8 o'clock in the morning until 11 and 12 o'clock in the evening, without being one single day confined to the bed. My assistant physician, Thorvaldur Jonison, who also from daybreak until night-time was obliged to be in continual activity, made, according to my advice, use of veratria, of which he took one-sixth part of a grain every morning, and sometimes twice a-day, which evidently conserved his strength, so that he could daily go through his business pretty well. My servant, who had to take care of my riding ponies, but got sick, was also ordered to take strychnia daily, which cured him in a short time. Many others who during the epidemic were obliged to be in the common business of their calling, made, according to my advice, use of veratria, and all agreed in its efficacy of preserving their strength, lessening the cough, and making the respiration more easy.

It seems to me that there are many reasons to try bark, the sulphate of quina, or veratria against this malady; as there can be no doubt that the epidemical influenza, at the same time as it tends to an intermittent character, also acts on the whole nervous system, and, perhaps, especially on the pneumogastric nerve and its branches, like a poison, we must try to find some remedies which by their catalytic virtues may work against that poison. Many favourable experiments speak for the use of sulphate of quina, and it ought never to be omitted when the epidemic has a marked intermittent or malignant character; and inductive arguments speak in many cases also for the administration of veratria. It is a well-known fact that Dr Aran and Ghiglia made a bold use of veratria in acute pulmonary affections with the best effect (*See* Bulletin Géneral de Therapeutique, 1859, p. 109); and we know, moreover, that it is an old and renowned practice amongst the veterinarians to cure the "febris catarrhalis epizootica canum" by high doses of "veratrum album," but that "epizootia" is evidently the "influenza of the dogs," accompanied, as so many autopsies have shown, by a heavy congestion to the throat of these creatures, with serous effusions in the sac of the pleura, mucous infiltration in their respiratory organs, and great loss of strength. We may therefore, by induction, calculate that these two diseases are of similar origin and nature, depending on similar causes, and will therefore be cured by similar remedies. Experience has at least convinced me of the utility of veratria in influenza. I tried it in very numerous cases, and found it highly serviceable.

One remedy which I often ordered against hard and dry cough was boiling water vapour; it generally gave much alleviation, and made the expectoration more easy and copious. Blisters on the breast had seldom that good effect which I had supposed, and which they generally give in the latter stages of pneumonia and common bronchitis.

REYKJAVIK, 1*st Sept.* 1862.

## ARTICLE III.—*Two Cases of Murrain* (*Aphtha Epizootica*) *in Man.* By J. B. HISLOP, F.R.C.S. Edin., Houston, Renfrewshire. Communicated, with Remarks, by GEORGE W. BALFOUR, M.D. St And., F.R.C.P. Edin.

(*Read to the Medico-Chirurgical Society*, 21*st January* 1863.)

THE following two cases of the occurrence of murrain (*aphtha epizootica*) in man have been sent to me for insertion in the Edinburgh Medical Journal, with the twofold object of eliciting information in regard to the spread of contagious diseases from cattle to man, and of pointing out the necessity of great caution being exercised by those who are brought into contact with cattle suffering from contagious disease.

"About the end of August last, Mrs X., the wife of an extensive farmer, came under my care on account of an eruption of bright red spots, one-eighth of an inch in diameter, covered with a thin white desquamation, which were so densely sprinkled over her feet, legs, thighs, and the lower part of her body, as to leave only minute interspaces of sound skin. As Mrs X. had, within the last three years, suffered from several attacks of hepatitis, followed by jaundice, and consequently was extremely liable to derangement of her liver and digestive organs, I concluded that the eruption just described might be thus originated, and I prescribed for her alterative purgatives, along with five grains of bromide of potassium three times a-day. This treatment made no impression on the symptoms.

"On a subsequent visit to my patient I found her husband complaining of sore mouth and throat. Upon examination I found the mucous membrane of his lips, mouth, tongue, and throat, studded with small ulcers giving off a white slough, which left behind it a clean but highly sensitive cup-shaped cavity; his forehead was also covered with an eruption similar to that upon the lower extremities of his wife. As this peculiar combination of symptoms in parties so closely connected was, to say the least of it, remarkable, I made strict inquiries, and distinctly ascertained that the only cause that could be assigned for this peculiar affection was the circumstance that the whole of Mr X.'s cows were at that time labouring under the vesicular murrain (*aphtha epizootica*), a highly contagious cattle distemper, in which the mucous membrane of the lips and mouth, particularly on the under surface of the tongue, is covered with vesicles and ulcers analogous to those by which Mr X. was affected. Mr X. informed me that while examining one of his cows which was suffering much from this disease, and while in the act of pressing back its lips, he observed two or three pimples on the upper lip to burst and eject the matter to a considerable distance, and that he received a portion of this matter on his hands and cheek.

"Knowing the high moral respectability of the parties, and having carefully weighed all the circumstances and peculiarities of the case, I came to the firm conclusion that Mr and Mrs X. had been independently inoculated with matter discharged from the apthous sores of the cattle; and this opinion was very much strengthened by my subsequently ascertaining upon inquiry that various other individuals employed about the cattle had suffered from similar symptoms though in a less degree.

"I now put both my patients under the internal administration of corrosive sublimate and iodide of potassium in solution, and touched the ulcers on Mr X.'s mouth, throat, and tongue, freely and frequently, with nitrate of silver, at the same time ordering the frequent use of a chlorate of potash gargle.

"Under this treatment the disease gradually but steadily declined. During its continuance Mrs X. had a smart attack of conjunctivitis in both eyes, which yielded to the employment of the bichloride of mercury, collyrium, and covering the lids with the belladonna ointment.

"I may mention that the eruption upon my patients was never observed to be vesicular, as it is in cattle; the spots made their appearance as slightly elevated reddish prominences (*papulæ*), which gradually became bright red, then threw off a thin white silvery-like scale, and again gradually disappeared.

"The cases were indubitably independent of each other, as far as the contagion is in question; both parties, as well as all those affected in a lesser degree, had gone freely about the byres handling the diseased cows, of which there were at times upwards of a dozen ill at once, and the peculiar virulence of the symptoms in Mr X.'s case, seemed to depend upon the circumstance that the matter caught upon his hand and cheek was derived from one of the worst of the diseased animals. Mrs X. had slight inflammation of the fauces, which yielded readily to the application of nitrate of silver and the use of chlorate of potash as a gargle. Several of the children about the house were also affected with sore throats, but the symptoms in their case were mild and easily overcome. Mrs X. and family were in the habit of using the milk freely fresh from the cows, though she did think "it could not be good;" what remained was churned and sent to market. On talking over the matter with Mrs X., I ascertained that the only way in which the introduction of the disease among their cattle could be accounted for, was the fact that they had allowed them to be fed upon clover which grew two miles and a half from their own byres, in close proximity to a field in which the cows of a neighbouring dairy were grazing, the said cows having the disease very bad at the time; and they therefore thought that the disease was brought in the clover carted from this field two miles and a half distant. The disease was not known to exist nearer, and no other mode of conveying the contagion could be discovered. I may add that I have observed many cases of herpes (ringworm), caught from diseased cattle, and I doubt not but that a better acquaintance with the diseases of our fourfooted friends would reveal the source of many otherwise obscure affections in man.

"JOHN B. HISLOP."

These two cases seem to me to possess very considerable interest both for the profession and general public. They present two well-marked instances of the occurrence in man of a loathsome, at least, if not always very fatal epizootic disease, in which the influence of contagion has been distinctly traced by an independent medical observer, who was previously entirely ignorant of the possibility of such an occurrence, and whose observations are therefore all the more valuable as the results of unbiassed inquiry, while they entirely corroborate the earlier investigations of continental medical men, and the more recent observations of veterinarians in this country, as I shall presently point out.

The word MURRAIN is of doubtful origin and of still more doubtful significance,—perhaps its earliest appearance is in 1389 (Anno xi. regni Ricardi II.) in its Latinized form: "*Murrena* damarum ferarum."[1] And in the Twysden Glossarium, we have this explanation given,—"*Murrena:* lues, tabifica lues, vulgo *Murraine;* a graeco μαραινω, *i. e.* tabefacio, ut Casaubonus jam observavit." Haesinger[2] prefers to derive it from the Sanscrit root *mr*, whence the Latin *mori* and the Celtic *muire*, etc. While considering the multiplicity of the significations attached to the word, a good deal might be said in favour of its derivation from the good old word *murr*, meaning coryza, and obviously derived from μυρω, to drop, to distil. To myself the latter derivation, which is original, or that of Twysden, appears to be much the most likely, while Haesinger's is obviously far-fetched, and apparently based upon the Septuagint version of the word murrain in Exodus ix. 3, which is θανατος in the version of Lambertus Bos (Franequaræ, 1709), the

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[1] Henric. de Knyghton, de Event Angl. Scr. Hist. Angl., p. 2693.

[2] Recherches de Pathologie Comparé. Cassel, 1853.

Oxford manuscript quoted by him *in loco* reads Λοιμος, one of the words employed by Thucydides in his history of the Athenian plague (ii. 54); while the original Hebrew (of Michaelis—Halæ, Magdeburgicæ, 1720), דבר, does not countenance any of these suppositions, as the primary notion of this root is *agere* or *ducere;* and from it we have through the Gothic *dreiban*, the English word *drive*, a pestilence, because it drives men (or animals, as in this instance) to their graves.[1] But, however uncertain the origin of the word *murrain*, its meaning is still more obscure, as it has been employed to signify epidemic disease among cattle of every possible character, from the dreadful carbuncular typhus—of which, and of its spread by contagion to animals of every class and also to man, Virgil has given such a striking description at the close of his third Georgic[2]—down to the simplest and mildest epidemic catarrh. From its fortunate insular position, Britain long remained comparatively exempt from any of these cattle plagues, and when, about twenty years ago, an outbreak of epizootic aphtha occurred, this was generally dignified by the name of *the murrain;* since then, however, the true position of this disease has been recognised, and it is now more qualifiedly and correctly termed the *vesicular murrain;* and it is this disease, the *aphtha epizootica*, *maladie aphthongulaire*, *mund-und-klauenseuche*, which Dr Hislop describes as being conveyed by contagion to man. The symptoms of aphtha epizootica, as observed in cattle, are at first merely those of general febrile disturbance of the system, with frequent rigors, general soreness of the body and unwillingness to move, and when made to move, the animal is found to be very lame. There is generally a harsh and frequent cough; but this symptom is not invariable; the mucous membrane of the mouth is swollen, and exhibits little reddish elevations; there is considerable flow of saliva from the mouth, and in about twenty-four hours from the first appearance of the disease, a crop of vesicles is found to be thrown out across the upper part of the mouth, along the sides of the tongue, within the lips, on the muzzle, and in the nostrils. Vesicles are also occasionally found around the roots of the horns and on the external parts of generation, while they are more common in the interdigital spaces and on the udder and teats, and these latter organs are often very much involved in those animals which are far advanced in gestation or in those giving milk. These vesicles are irregular in form, and have neither the central depression nor the distinct inflammatory areola observed in true cowpox.[3] They are at first about the size of a millet-seed, but gradually increase in size to that of a kidney-bean, or larger. The contents of these vesicles are at first pure serum, but within a few hours this becomes more or less opaque from the admixture of shreds of lymph and pyoid corpuscles. Sometimes this fluid is absorbed and the cuticle desquamates, leaving a red raw surface, at other times the vesicles burst and scabs are formed, while in severer cases ulceration occurs which may take eight or ten days to heal. These symptoms all increase till about the third day, after which they commence to decrease, and in mild cases the animal is well in little more than a week. The treatment consists chiefly in giving nourishing food, the use of mild astringent or disinfectant washes, with tonics subsequently. This disease, though long well-known on the continent, was yet wholly unknown in Britain till the spring of 1839.[1] It is not known how it first arrived hither, and the mode of its communication has been often disputed. In densely peopled countries, where much intercommunication exists, it is often difficult to trace the spread of diseases by contagion alone. The spread of measles, for instance, gives perhaps one of the most striking examples of this difficulty. With us it is one of the commonest diseases of childhood, which almost every one passes through in their youth once for all, and scarce any one thinks of inquiring whence it comes; yet, in the Færöe Islands, no case of measles had occurred for upwards of sixty years, till it was taken thither by a labourer from Copenhagen in 1846. Its spread could be distinctly traced from man to man, and was only checked by establishing a rigorous "cordon sanitaire" round the affected localities.[2] That it was thus checked is of itself sufficient proof of the purely contagious character of this disease. There is no question but that the vesicular murrain is contagious, because it is well-known to be inoculable, and whole herds have been often artificially inoculated with the view of giving them a milder disease, and getting them more rapidly through it; and, considering that this is truly a panzootic disease affecting animals of every class, cattle, sheep, deer, pigs, dogs, cats, horses, geese, ducks, pigeons, fowls, hares, rats,[3] etc., it is surely more consistent with reason to

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[1] *Vide* Parkhurst's Hebrew Lexicon. Lond. 1799.

[2] In imitation of Thucydides and Lucretius, say some commentators; more probably derived from personal observation in the terrible year 43 A. C., described in his first Georgic.- -*Vide* Heyne's Notes.

[3] Under this head it may be well to mention, that true cowpox is always distinguished by an umbilicated vesicle surrounded by a rosy areola. There are several kinds of spurious cowpox described by veterinary authors, distin-

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guished by the conoid form of the vesicles, the absence of areola more or less complete, and the generally anomalous course of the disease. The worst that can attend the inoculation of man with matter from these spurious vesicles is absence of success; it is otherwise with that from the aphthous vesicles, as we see that disease may be actually produced. M. Prinz of Saxony mentions an anomalous variety of vesicular murrain in which the vesicles were placed upon a hard and inflamed base.—Clarus and Radius, Beiträge, ii., p. 78. *Vide* also Hildenbrand, Institutiones Practico-Medicæ; Vienne, 1833, vol. ii., p. 310, etc.

[1] Veterinarian, vol. xiv. p. 184.

[2] *Vide* a most interesting paper by Dr Panum of Copenhagen. Archiv f. Phys. Heilkunde, t. ii. It is also published in the Edinburgh Monthly Journal, June 1851, p. 589.

[3] Krieg Casper's Wochenschrift, 1839, p. 561. Schneider Erfahrungen über die Maul-und-Klauenseuche. Freiburg, 1840; etc., etc.

suppose that the contagion has, even in the most obscure case, been conveyed to the animal by some dog or rat,[1] than to imagine that it forms a mysterious exception to a well-known law. Dr Hislop's two cases corroborate the fact that man forms no exception, but is equally susceptible to this contagion with other animals. Continental medical men have long been well acquainted with this fact, and numerous instances are found recorded in their journals.[2] The disease had scarcely made its appearance in this country when it was found affecting man in the person of a young farmer, who was inoculated by means of a wound on his finger, and who had the characteristic vesicular eruption on his mouth and nose, attended by very severe constitutional disturbance and great prostration of strength.[3] A similar but somewhat milder case was observed about the same time by Mr Duncan of Colinsburgh, in Fife, in the person of a boy employed in attending on diseased cattle.[4] Many other cases have been since recorded,[5] and they are indeed so numerous when ferreted out, that there is scarce a cattle-dealer who has not either had the disease himself, or knows of some who have had it. Fortunately in this form it is chiefly found to affect robust men; and it is probably never fatal, though it is just possible that some cases may be buried under the name of *diphtherite* in the books of the Registrar-General.

But there is another and a much more insidious manner in which the contagion of this disease may be communicated to man, and in which it may prove unquestionably more injurious, first, because it is thereby more especially conveyed to those who are young and weakly, and who are thus less able to stand it; and, secondly, because the symptoms produced are apparently much less distinctly referrible to the actual cause,—I refer to the use of the milk of the diseased animals. Whether the milk of animals thus diseased is injurious or no has been already the source of much controversy both at home and abroad; but in this, as in every other similar question, a few positively affirmative cases outweigh whole bushels of negative ones. "*Tous les veaux nourris par des vaches ayant des*

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[1] Anker Einleitung zur Erkenntniss, etc., der Maul-und-Klauenseuche. Bern, 1839.

[2] So early as 1707 we find it recorded,—Idem malum (vesicular murrain) ex impuritate maligna seri simili affecit modo dispositos, præsertim juniores homines quosdam cum deliriis, quosdam absque deliriis, omnes vero febre catarrhali maligna vires et partes citissime consumente. Ephem. Nat. Curios. Cent I. et II., p. 156. *Vide* also Andreæ über die gutartige Maul-und-Klauenseuche. Magdeburg, 1839, etc., etc. *Vide* Haesinger, op. cit., passim,—where very many cases are related, observed both by medical men and by veterinarians.

[3] Veterinarian, vol. xiv. p. 152.

[4] Quarterly Journal of Veterinary Science, August 1862, p. 506. The case, as related to me in a letter from Mr Duncan, was a peculiarly well-marked one. Mr Duncan also mentions having observed a horse affected by the same disease; and the like has been often observed as the result of direct contagion, though probably not more often than in man.

[5] Veterinarian, 1846, pp. 112, 117, and 148.

*aphthes aux mamelons ont peri*," says M. Binet.[1] Andreæ (op. cit.) relates many instances in which calves, pigs, and dogs, fed with murrain milk, died from diarrhœa, accompanied by convulsions, and several cases of death among calves from the use of the milk of diseased cows are mentioned by R. H. Watson of Kelso, in the Edinburgh Veterinary Review for August 1862, p. 505. All these animals were found on dissection to labour under gastro-enteritis; and this was also found to be the case in many pigs killed at once on being observed to be ill, as mentioned by Mr Thombs.[2] Dr Hislop mentions that the children of the farmhouse he refers to suffered from sore throat. Mr Watson (loc. cit.) mentions that several of the farm-servants' children suffered from sickness, pain in the bowels, and considerable diarrhœa, which was cured by a dose of castor-oil, and stopping the diseased milk, the use of which, if persevered in, might have carried them off as well as the calves. Kolb[3] says, "Lac potum in infantibus pluribus vomitum acidum et diarrhœam ciebat." Krügelstein[4] mentions that children who drank murrain milk were seized with violent fever, swelling of the face, and vesicular eruption in their mouths. Mr Gamgee[5] mentions the death of eight out of nine calves from inflammation of the throat and alimentary canal. Erdt[6] also mentions many instances of disease arising in men, and particularly in infants, from the use of murrain milk. And innumerable other instances might be given[7] in which disease, chiefly in the form of gastro-enteritis, was produced in men, but particularly in children, by the use of butter, curds, or milk, from diseased animals, many of which might have proved fatal but for the timely cessation of the use of the milk. And yet it is indubitable that murrain milk must have been often used in various forms without anything more than at the most temporary inconvenience, and this is very easily explained; for when the disease is at its height, the milk is apt to be rendered useless by the admixture of blood, or to be so much altered in appearance, or lessened in quantity, as to be rendered unusable, or at least to require large dilution before being sold, and dilution, of course, lessens very much the virulence of the poison. That the milk is really injurious is proved not only by the cessation of the symptoms on its use being discontinued, but also by their immediate production by the experimental drinking of the milk, as has been proved by Jacob[8] and Hertwig.[9] At the height of the disease the milk is very often sour.[1] Microscopically examined it presents granular bodies and agglomerations of globules,[2] precisely resembling the milk of newly calved cows; but there is one point in which, so far as I know, it differs from all other milk, and that is in its coagulation on being boiled, or having its temperature only very slightly raised by being mixed with hot gruel, etc.; and the coagulation is something quite peculiar, and not to be forgotten when once seen, as it runs into innumerable little fibrous or flocculent masses,[3] leaving the fluid part like a milky whey. It is a very general opinion among continental veterinarians, that when the milk can be boiled without coagulating, it is no longer dangerous to use; and Hildebrandt, in Magdeburg, expressly states that he had never observed a single case of injury to health from the use of milk which had been boiled without coagulating.[4] This, therefore, is an important point to remember, and it quite agrees with my own experience. Last winter the milk supplied to my family disagreed with the children, and particularly with an infant being brought up on the bottle. On examination, I found that it presented the microscopic and chemical peculiarities described above, and I at once taxed the dairy people with having murrained cows. The reply was, that there were no murrained cows in the dairy, but a number of new-calved ones. This statement quite agreed with the *microscopic* character of the milk, although, so far even as I then knew, it did not explain its *chemical* peculiarity of easy coagulability. Still, as at that time I was unable to investigate the matter more accurately, I contented myself with only making use of the cream, and that very sparingly, and combined with a certain amount of lime-water, till this symptom of easy coagulability had passed off. I may add, that at that time I was not aware that this easy coagulability of the milk had been so much insisted on by continental authors as the result of murrain, and as a proof of its possessing injurious qualities.

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[1] Annales de la Soc. des Vet. du Depart. du Finisterre, iv. p. 12.

[2] Veterinarian, xiv. p. 127.

[3] Aphtharum Pecorinarum Historia Succincta, etc. Tubing. 1828, p. 8.

[4] Henke's Zeitschrift f. d. Staatsarzneik., xxxvii. p. 239.

[5] Highland Society's Transactions, 1859, p. 87.

[6] Magazin f. Thierh., x. p. 34.

[7] Gurlt u. Hertwig's Magazin f. d. g. Thierh., vi. (1840), p. 175; *vide* also Die Milch in Medicinisch-Polizeilicher Beziehung. B. Nauheimer. Giessen, 1860, etc.

[8] Journal de Médicine Vétérinaire, pub. à l'École de Lyon, t. ii., 1846.

[9] Medicinische Vereinszeitung, 1834. No. 48, p. 226.

I have no remarks to make as to the use of the flesh of murrained animals as food. The vesicular murrain is productive of so small a mortality, that it is comparatively little dreaded, and probably but few cattle are slaughtered because of being seized with it; and, besides, the whole question of the use of diseased meat for food is one of too great importance and of too vast proportions to be entered on now.

I have only two remarks to make in conclusion: The first is, that it is wrong to suppose that a thorough investigation of all these matters, and particularly of the relations and injurious influences exerted by epidemic disease among cattle upon man, can possibly be injurious to any class of individuals. All farmers, cattle-dealers, and dairymen would gain far more in the end by the prevention of epidemic disease, which such an inquiry, properly carried out, would be sure to lead to, than their primary loss would amount to, even supposing mankind were to refuse to testify their sympathy for them by eating diseased meat and drinking diseased milk,—a state of matters showing a phase of mind closely allied to that of the South Sea islander, who, standing by the deathbed of his chief, proposed, through his tears, to evince his sense of the greatness of his loss by eating the body without salt! Besides, the pecuniary loss could be made up in various ways, particularly, as is the case in Holland, where cattle is the chief dependence of the people, by a system of mutual assurance. The other remark I have to make is, that the day has gone past for an isolated individual or craft to avert pestilence, as Empedocles did when he shut out the sirocco by stopping a mountain-gap, and removed intermittent fevers by changing the course of the river Hypsa. These large and beneficent operations are in our day reserved for GOVERNMENTS; and our duty as a profession is to urge upon Government, by means of our own governing bodies (who exist for that very purpose), the necessity of undertaking *the prevention* of epidemic disease among both men and animals, to point out the best modes of securing this prevention, and to see that these measures, when become law, are properly carried out. In a word, it is our duty not to appropriate to ourselves, as is too often erroneously done, but to endeavour to impress upon our rulers the sentiment so nobly urged upon Cæsar by Tully, "Homines enim ad Deos nulla re propius accedunt quam salutem hominibus dando."[1]

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[1] Die Milch, etc., p. 34.

[2] Rayer, Archives de Med. Comparée, No. 3. M. Donné, etc., Memoires de l'Academie des Sciences, tom. xvii.

[3] "Faserigen clumpen." Andreæ, *op. cit.*

[4] Gurlt u. Hertwig's Magazin, vi. p. 179.

## ARTICLE IV.—*Statistical Report of One Thousand Midwifery Cases.*
By S. LAWRENCE, M.D., Montrose.

"THE pathway to truth in medicine," says an eminent living physician, "lies through statistics." To the same effect, but more generally, a recent divine expresses himself thus:—" By observation, facts are accumulated; by a comparison of these facts, principles are elicited; and by a comparison and arrangement of principles, science is established." Sir John Herschel concisely enunciates the same truth when he remarks that "numerical precision is the soul of science." At the present day it is happily unnecessary, whether by appeal to authority or otherwise, to vindicate a proposition which not only commends itself to reason, but which is ratified by universal experience. Doubtless, to a certain class of minds, isolated facts may suggest theories, or seem to indicate laws; but those facts must be repeated and multiplied ere the truth of such theories, or the reality of such laws, can be held as established. We have only to look to the history of medicine in confirmation of this remark. For if we examine the numberless theories, the ruins of which lie strewn along the pathway of that history from its beginning downward, what have they been? Either mere hypotheses, fabricated in the closet by speculative brains; or fantastic structures reared on the narrow basis of isolated facts, and which, having thus no adequate foundation, have been speedily overthrown. To the accurate observation of facts, therefore, their faithful record, and their patient and long-continued accumulation, must we, in the first place, address ourselves, if general principles deserving of our confidence are ever to be attained. The labour which this involves will doubtless make us know what Mr Ruskin entitles the "cost of truth;" but it will ultimately yield us the satisfaction and joy of finding it.

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[1] Pro Ligario, cap. xii.

A good many years ago[1] I drew up and submitted to the notice of my professional brethren the statistics of 360 midwifery cases which had occurred in my practice, and of which I had kept brief memoranda. I am now enabled, from a longer experience, to attempt a similar undertaking on a larger scale. Of the midwifery cases which, since the period referred to, have come under my care, I purpose in what follows to report upon 1000 which occurred consecutively, and the chief incidents of which I have preserved on record. So far as time and ability will enable me I shall endeavour to analyze this series of cases, so as to discover what doctrines or practical lessons may be fairly deduced from them. I am far from imagining that 1000 cases is an adequate number to *settle* any of the many interesting questions which the obstetrical department of medicine presents. But, in the first place, they cannot fail, if faithfully recorded and correctly analyzed, to throw *some* light on such topics of inquiry; and, secondly, they will at least serve the humbler, but scarcely less useful purpose of forming a part of that larger induction by which a higher, and therefore truer, generalization may be ultimately reached. I have therefore in view, in the present paper, not only to state facts truly as they came under my observation, and to endeavour to interpret them justly, but, in addition to this, to persuade, so far as my voice and example may avail for this purpose, a goodly number of my professional brethren over the country to undertake a similar task. During the last forty or fifty years we have been accumulating pretty ample records of hospital obstetric practice; but of the statistics of midwifery in private practice, we have, up to the present time, but a comparatively scanty store. And yet the lessons and doctrines taught by the latter, did we possess them on a scale of sufficient magnitude for safe induction, might be found considerably to modify those taught by the former. What I would therefore take leave to suggest is, that *fifty* practitioners throughout Scotland, possessing the requisite materials for the task, should each report the statistics of 1000 cases of childbirth.[1] In the course, say of the next five years, with an ordinary amount of industry, this might be accomplished. We should then be in circumstances to pronounce upon some questions which are still matter of doubt or controversy, and to test, and possibly somewhat modify, not a few of the obstetric canons which have issued from our hospitals and schools, and have long enjoyed currency amongst us. Meanwhile, I beg, with all deference, to take a share in this work, and to contribute a *fiftieth* part to the desired store.

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[1] Monthly Medical Journal, February 1849.

Let me first summarize the facts which constitute the groundwork of the following report. 1*st*, The series of cases of labour attended was, as remarked above, 1000. For a reason which will afterwards appear, I have included in this number all cases of birth occurring after the period of quickening, 32 children having been born abortive or premature, of whom 18 were dead and 14 living. 2*d*, Of these 1000 labours 220 were primiparous, and 780 non-primiparous.[2] 3*d*, The number of children born was 1017 (there being 17 cases of twins), of whom 553 were males, and 464 females. 4*th*, The accidents of labour, or of the puerperal state, proved fatal to five mothers, *i.e.* to 1 in 200. Of these five mothers, two were previously in an unsound state of health, and in consequence succumbed all the more readily to the attacks which carried them off. 5*th*, The number of lingering, tedious, or difficult labours, was 83. 6*th*, The forceps was employed 28 times to accomplish delivery. 7*th*, There were 34 cases of preternatural presentation. 8*th*, Of labour with different forms of complication there were 21 cases. 9*th*, There were 45 children, mature and immature, born dead.

## I. TEDIOUS AND DIFFICULT LABOURS.

I have classed 83 labours out of the 1000 as belonging to this category. This number, being in the proportion of 1 to 12·04, is certainly high. But, in the first place, the great majority of these 83 cases had no element of "difficulty" in them, but only that of "tediousness;" and, secondly, several of them possessed even this latter character only in a modified degree. Indeed, each practitioner will take his own view of what constitutes either a difficult or tedious

[1] I was not aware when writing the above, nor indeed until after this paper was completed, that the suggestion here made had been so far anticipated by Dr Paterson of Partick, in an excellent and instructive communication in the Glasgow Medical Journal for October last, entitled "An Analysis of One Thousand Cases of Labour occurring in Private Practice." I have since perused that communication, and have been gratified to find so much "unity in diversity"—so much substantial agreement of opinion as the result of our independent observation and reflection.

[2] In a practical point of view, this division of labours into primiparous and non-primiparous is of much importance. It is of small moment to distinguish labours into second and third, fourth and fifth, and so on, compared to the importance of arranging them simply into first and subsequent ones. This will be seen when the statistics of primiparous cases are placed side by side with those of non-primiparous.

labour. Of the 83 cases 38 were primiparous, and 45 non-primiparous, thus giving the following ratios:—

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|---|---|
| Primiparous, . . . . . | 38 to 220, or 1 to 5·78. |
| Non-primiparous, . . . . | 45 to 780, or 1 to 17·3. |

This class of labours is thus seen to be about three times more frequent in first than in subsequent labours. A large proportion of the entire number owed their tediousness or difficulty simply to feeble uterine action, or to this, in combination with rigidity of the soft parts. In primiparous labours these causes of delay or difficulty were found to operate about once in every six cases; in non-primiparous, about once in every 21 cases. The other causes in operation determining this class of labours were referable to malposition, or largeness of child, or contracted pelvis. Some of the facts elicited by examining these 83 cases in detail are the following, viz., 1*st*, There were 21 of them in which neither medicinal nor mechanical aid was employed, but which were left to, and successfully terminated by the powers of nature. 2*d*, Twenty-eight cases were delivered by the forceps. 3*d*, Ergot of rye was administered thirty-seven times (always in the second stage, and in the usual way in infusion), nineteen times with apparent benefit, and eighteen times with no perceptible effect. There was only one child born dead after the use of the ergot; but not, I consider, "post hoc ergo propter hoc," for the head was so much squeezed from the difficulty experienced in the second stage that death seemed fairly attributable to cerebral pressure. 4*th*, Following a suggestion of Professor Simpson (Monthly Medical Journal, July 1850), I gave the tincture of cannabis indica in some of these cases, by way of testing its powers as a uterine stimulant. I administered it altogether five times, twice with seeming benefit, and thrice with none. I subjoin brief notes of the five cases in which it was administered.

CASE 1.—Primipara. Labour tedious from deficient uterine action. About midday twice administered ergot of rye with no perceptible effect. At 3.30 P.M., the pains having been very inefficient for two hours before, I gave twenty drops tinct. cannab. indic., and at the end of twenty minutes, as no effect had followed, thirty more. In five minutes after this second dose there was a marked increase in the strength of the pains, and by and by they became strongly expulsive, and they continued so till the birth of the child at 6.15 P.M. The intermissions between the pains were very distinct, although short.

CASE 2.—Non-primipara. Face to pubis. On my arrival the os uteri was well dilated, but the pains were infrequent and feeble. After waiting half an hour without any improvement of uterine action, I gave thirty drops of tinct. cannab. indic. In five minutes after, the pains became vigorous, and in twenty-five minutes more labour was terminated.

CASE 3.—Non-primipara. Lengthened attendance. Pains deficient in strength and frequency till within half an hour of the birth of the child. I twice gave 30 drops of tinct. cannab. indic., with an interval of an hour between the doses, but no apparent effect was produced.

CASE 4.—Non-primipara. Tedious. Gave thirty drops tinct. cannab. indic., in half an hour fifteen more, and again in five minutes other fifteen, but without perceiving any effect from either of the doses. The child was not born until two hours after the last dose.

CASE 5.—Non-primipara. Pains for several hours very inefficient. Gave three doses of tinct. cannab. indic., thirty drops to the dose, at intervals of half an hour, without any perceptible effect. An hour after the last dose I gave ergot of rye, and pains soon after became strong and urgent.

Such is the entire extent of my experience of this alleged oxytocic. It is of course too limited to enable me to speak with certainty regarding it; but, so far as my testing has been carried, it appears to me quite as capricious in its operation as the secale cornutum. It is possible, however, that the conditions of its success may be more accurately determined by some future experimentalist. *Lastly*, In a few cases, where the delay was due to dryness and rigidity of the os uteri (occurring chiefly in primiparous labours), tartrate of antimony, given in small and repeated doses, proved of much service.

## II. FORCEPS CASES.

The number of cases terminated by the aid of the forceps was 28. Of these 21 were primiparous, and 7 non-primiparous, giving thus the following numerical proportions:—

| | |
|---|---|
| Forceps cases, . . . . | 28 to 1000, or 1 to 35·7 |
| Primiparous do., . . . | 21 to 220, or 1 to 10·47 |
| Non-Primiparous do., . . . | 7 to 780, or 1 to 111·4 |

There is thus brought out the instructive fact that the forceps were required more than eleven times more frequently in first than in subsequent cases.

*Results to Mothers.*—All the mothers in these 28 cases recovered, excepting one, in whom the labour from contracted pelvis was peculiarly severe. Uterine phlebitis occurred a day or two after delivery, and ran a short, and, as usual, fatal course. It was the patient's second labour. Her first, owing to the same cause, had also to be terminated by the forceps. In some of the cases, where the labour had been considerably protracted before the forceps was employed, recovery was slow; in the great majority, however, it was as rapid, and, in all respects, as satisfactory as in ordinary cases.

*Results to Children.*—Four of the children were born dead, twenty-four alive. In regard to three of the dead-born children I feel quite persuaded that an earlier use of the forceps would have saved them. I give their particulars in a few words. One of them was my own patient from the commencement, the other three were, in the first place, under the charge of midwives. No. 1 (my own case). Primiparous—labour tedious—head for eight or ten hours low down in the pelvis, and making no progress—strength and spirits of patient getting exhausted—put under chloroform,[1] forceps applied easily, and delivered in a few minutes. Child dead. At the time this case occurred I was less impressed than I now am as to the danger (especially in *first* cases) of such delay as is here stated, otherwise I should have used the forceps some hours earlier, and in all probability have delivered a living child.—No. 2. Non-primiparous. Midwife had been previously fourteen hours in attendance. Uterine action teazing and inefficient—head well down for hours—patient's strength getting exhausted. Forceps applied at once, and child delivered, but dead. Recovery tedious.—No. 3. Called to this case after a very lengthened attendance by midwife. Head well down, but had for hours been making no progress. Applied forceps, and delivered. Child dead. Mother's recovery very protracted.—No. 4 (this is case No. 2 of puerperal convulsions. See sequel). In this case the child perished from convulsions, and from the position of the head, when I was called to assist, I do not think it would have been practicable to have saved the child by means of the forceps, even had I been earlier in attendance. Whether turning when the convulsions commenced, had I then had an opportunity of performing it, would have saved the life of the child, is, of course, aside from the point at present under consideration. My infantile mortality in forceps cases was thus 4 to 28, or 1 to 7; but, for the reason above stated, I consider this by no means fairly represents the safety and advantage with which that instrument may be used. So far, therefore, from agreeing with Dr Murphy, "that so far as children are concerned (in protracted labour) the proportion of stillborn is very much the same whether the forceps be employed or not; the difference being in favour of leaving those cases to nature," all my experience and reflection force upon me quite an opposite conclusion. Professor Simpson, in a discussion in the Edinburgh Obstetrical Society, reported in the Medical Journal for October 1862, remarked that "he had never doubted that the chief danger from delay (to the child) began when the fœtal head had begun to be compressed in the maternal canals; but it was quite impossible to bring this out in any statistical table, as data enough were not to be had." There is, however, it seems to me, one way in which such a proposition admits of being invested with almost the force of a demonstration. It is to compare the practice of those who hold such a view with that of those who either reject or disregard it. Different beliefs necessarily prompt to different modes of practice, and the truth or error of the former becomes consequently embodied in the latter.—"By their fruits ye shall know them." Not to enter into any elaborate and extended comparison, just let us take, on the one side, Dr Hamilton of Falkirk, who most earnestly believes this proposition, and on the other, the Dublin Hospital, where it is disbelieved or disregarded. I take the latter as represented in the report of Drs Hardy and M'Clintock (1848). Dr Hamilton tells us of his 90 to 100 forceps cases out of 731 labours, being 1 to every 7 or 8 cases. Drs Hardy and M'Clintock relate 18 forceps cases out of 6634 cases of labour, being 1 to 368·5 cases. In other words, taking Dr Hamilton's average as 1 to 8, we find he uses the forceps forty-six times for once they are used in the Dublin Hospital! Let us see the results of such contrasted modes of practice. In all Dr H.'s 90 to 100 forceps cases the children were born alive. In the 18 forceps cases in the Dublin Hospital the mothers perished in 5 and the infants in 8! It will not do to throw the blame of this startling mortality, as is so readily done by the reporters, on the ergot of rye. Others use this drug as a uterine stimulant, without finding it possessed of such deadly potency as it seemed to exert across the channel. We must seek elsewhere for the cause of such disastrous results, and we shall only find it, I fear, in Dr Murphy's principle having been acted on of "leaving to nature," for an unduly protracted period, case after case, which a timely use of the forceps would have conducted to a prosperous issue. My own experience is directly corroborative of what I have here advanced. During the first half of my 1000 cases I used the forceps only eight times, or once in 62·5 cases; during the latter 500 cases I used them twenty times, or once in every twenty-five cases; that is to say, just two and a half times more frequently in the latter than in the former. And what has been the result? The whole of the four fatal cases occurred during the former period! In other words, during a series of 500 cases, at a period when I considered the forceps only called for if the labour was considerably protracted, I used them eight times, and in these eight cases had four dead-born children;[1] while during another, a series of equal magnitude, at a period when I believed that in midwifery, if anywhere, "delays are dangerous," I used them twenty times, and in these 20 cases all the children were born alive! I leave this fact to teach its own lesson.[1]

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[1] In all my forceps operations, with, I think, but one exception, I made use of chloroform; my experience in this matter being quite opposed to the views propounded by the Philadelphia professor of midwifery, Dr Meigs, that the patient's retention of sensibility to pain is an important means of securing her safety in the use of this instrument, especially of guarding against injury to the maternal passages. Of the perfect safety and great advantage of this invaluable anaesthetic in all such operations, I have had the most satisfactory proof. In ordinary cases of labour I have so generally found it retard uterine action, and so frequently suspend such action altogether, that I now never administer it in such cases, unless pressed by the patient to do so, and then only when the pains are becoming powerful and expulsive toward their close.

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[1] Strictly speaking, I should only count three dead-born children as due to the delayed application of the forceps, inasmuch as the death from puerperal convulsions was not owing to this cause. The general argument is but slightly affected by the admission. If it should be said that in two of the remaining three cases, the time when the forceps were applied, did not depend upon my abstract views of such cases, but upon the period at which my assistance was sought by the midwives in attendance, I have to reply that such personages, in their conduct in these matters, will be found to reflect the opinion of those whose help they solicit. How came it to pass that in all the after cases wherein midwives asked my assistance in their difficulty, the use of the forceps saved the child? Simply because the necessity of asking such assistance at an earlier period had been impressed upon them.

In the light of such facts, and others of kindred character constantly accumulating, it appears to me that there is no presumption in asserting that the teaching of many of our standard works on the use of the forceps requires revision. It would occupy too much space to cite passages by way of exhibiting the character of that teaching; suffice it to say, that in enumerating the circumstances which ought to prompt us to the use of this instrument, *danger to the child from protracted labour* is not dwelt upon with the force and emphasis it deserves. The perils which beset the mother from such a contingency are doubtless faithfully detailed; and hers, as being the more valuable life, ought ever to occupy the first place in our anxieties as practitioners of midwifery. To save our patient many weary hours of bodily pain and mental suffering, and so to place her in circumstances more propitious to a favourable recovery, would assuredly be a sufficient motive for the timely use of the forceps, even could no other be adduced. But the welfare of her progeny is also of urgent importance, and the measures needful to secure it demand our most serious thought. Be it observed, then, that often long before the development of symptoms menacing the life or welfare of the mother, the duration of the labour may seriously endanger the life of the child. A close investigation of the statistics bearing on this point will be found to fully establish the statement here made. There may be neither any notable exhaustion of the general powers of our patient, nor any such local heat, dryness, or swelling, as to indicate that the pressure of the fœtal head is compromising the safety of the maternal structures, and yet *the parturient process may be passing the limit in point of duration at which it is compatible with the birth of a living child.* It, therefore, is my thorough conviction, that danger to the child from protracted labour ought to enter more than it has ever yet done as an element into our calculations when deliberating on the propriety of using the forceps. The danger to the mother from the use of the forceps is often insisted on as an argument for their sparing employment. That much mischief may be done by them, if rashly and unguardedly handled, is at once admitted. But I am not arguing for their careless, any more than for their indiscriminate employment. If intelligently and cautiously introduced and used, as they ought always to be, the risk of injury from their employment will be found to be to a great extent imaginary. Every one who has had much experience in their employment will corroborate this averment.

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[1] The argument might be made even stronger than I have presented it. For instance, out of the 259 cases of tedious and difficult labour recorded by Drs Hardy and M'Clintock, there were twenty-two deaths of mothers, or 1 to 11·77; and nineteen out of the twenty-two were in primiparous labours! I can scarcely conceive a more significant statement, and it were well that every student and practitioner of midwifery should deeply ponder it.

While the general principle of the benefit accruing from the timely use of the forceps may be held as clearly established, the question is left open whether, in any given series of successful cases, they may not have been occasionally unnecessarily employed. Whether, in other words, a proportion of the cases in which they have been used, would not have been equally fortunate without them? Of course it would not be easy to answer this question, nor is it needful to do so; for, provided it can be shown that the practice pursued has resulted beneficially to the infants, without any present or after ill-consequences to the mothers, this is enough. By all means let the *nimia diligentia* be unsparingly censured, where it can be unequivocally proven. But we ought ever to remember that while to *cavil* against any given line of practice is an easy attainment, *to prove it wrong* is always a more difficult task. I should state that the forceps I have long used have been those of Dr Ziegler of Edinburgh, the excellence of which is too well-known to need any commendation. I may further mention that in two of the 28 cases in which the fœtal head was well down in the pelvis, I made trial, in the first instance, of Dr Simpson's "Air Tractor," (more euphoniously termed "Suctorial Tractor,"[1]) but found it quite unavailing for the purpose of extraction. The forceps had to be finally used in both. I have not learned whether the fortunes of this simple and *theoretically* happy contrivance have been at all more prosperous in the hands of others; or whether it has to any considerable extent superseded the use of the forceps, even in the practice of its ingenious inventor.[2]

## III. Preternatural Presentations.

By some authors malpositions of the head are not reckoned among preternatural presentations, but are treated of simply as varieties of natural labour. Any deviation from the first of Baudelocque's six positions doubtless interferes with the easy progress of parturition; but "face" and "face to pubis" presentations are so decidedly causes of difficult and tedious labour, that I have followed the example of those who class them as *preternatural*. Proceeding on this principle, the cases of preternatural presentation I met with may be arranged as follows (I exclude twin cases, which are given as a class *per se*):—

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[1] See Medical Journal, February and March 1849.

[2] A new question has recently been raised in connexion with the use of the forceps, namely, whether in saving the life of the child they are not unfrequently the cause of destroying its intellect. If the affirmative of this could be shown to be true, even in a limited proportion of cases, the fact could not fail considerably to embarrass obstetricians in their employment of this instrument. I am not aware of a single instance within the range of my own experience, or that of my brethren in this district, giving any countenance to this idea. The children delivered by myself by the forceps, so far as my acquaintance with their after-history goes, have been at least on a level with their contemporaries in point of intelligence. And I am not aware that they have ever presented any morbid phenomena which could with fairness be attributed to the temporary pressure they had sustained from their artificial delivery. Query,—Has the obstetric history of any considerable number of the idiots throughout the country been inquired into, so as to discover whether it goes to support or refute this new etiology of their mental state?

*Preternatural Presentations.*

| | Primiparous. | Non-Primiparous. | Total. |
|---|---|---|---|
| 1st Division,— | | | |
| Breech, | 2 | 9 | 11 |
| Footling, | 3 | 2 | 5 |
| Arm, | 0 | 2 | 2 |
| Funis, | 2 | 3 | 5 |
| | 7 | 16 | 23 |
| 2d Division,— | | | |
| Face, | 0 | 1 | 1 |
| Face to Pubis, | 2 | 8 | 10 |
| | 9 | 25 | 34 |

These preternatural presentations, viewed with reference to the risk they involve to the child, require to be grouped as above under two divisions; the first comprising those cases in which such risk arises from pressure on the cord; the second, those in which it is due to severe or protracted compression of the head. We must, however, eliminate from the table as above given such breech and footling cases as were *premature*, before we can institute a fair comparison as to the relative frequency of such presentations in primiparous and non-primiparous births. The reason of this is obvious. In the earlier periods of pregnancy, it is well-known that presentations of the pelvic extremity are of great frequency as compared with such presentations at the full time,[1] and, consequently, with reference to any breech or footling case occurring in the 6th or 7th month, we are entitled to say that had it gone on to the end of the 9th month, it might have become changed into a natural presentation. On this principle there fall to be deducted two cases of non-primiparous breech presentations which occurred at the 6th and 7th month respectively; and one primiparous footling, which occurred at the 6th month. The corrected table would therefore stand thus,—

*Preternatural Presentations at the Full Time.*

| | Primiparous. | Non-Primiparous. | Total. |
|---|---|---|---|
| 1st Division,— | | | |
| Breech, | 2 | 7 | 9 |
| Footling, | 2 | 2 | 4 |
| Arm, | 0 | 2 | 2 |
| Funis, | 2 | 3 | 5 |
| | 6 | 14 | 20 |
| 2d Division,— | | | |
| Face, | 0 | 1 | 1 |
| Face to Pubis, | 2 | 8 | 10 |
| | 8 | 23 | 31 |

---

[1] Before the end of the 6th month, head presentations are only as 52 in the 100; at the end of the 7th month, as 63 in the 100; during the 8th and 9th, as 76 in the 100; and at the end of gestation they are as 96 in the 100 (Dubois).

Let us now see what numerical ratios we obtain from such data.

1*st*, 31 cases of preternatural presentation at the full time to 1000 labours is 1 to 32·25.

2*d*, 8 such primiparous cases to 220 is 1 to 27·5.

3*d*, 23 such non-primiparous cases to 780 is 1 to 33·9.

We thus find that such cases, viewed as a whole, are of about a fifth more frequent occurrence in primiparous than in non-primiparous cases.

4*th*, 6 primiparous cases in which the presentation involved danger to the child from pressure of the cord to 220 is 1 to 36·6.

5*th*, 14 non-primiparous similar cases to 780 is 1 to 55·7.

There are thus fully a half more cases in primiparous than in non-primiparous births in which the child is exposed to risk from compression of the cord. The relative frequency with which, in primiparous and non-primiparous labours, the child is exposed to danger from undue cerebral compression is not brought out by the above table, inasmuch as it includes only a small proportion of the cases where such undue compression was exerted. The proper proportion of such cases has been already stated under the head of tedious and difficult labours. I would now notice the chief points of interest pertaining to each of these kinds of preternatural presentation in the order above stated.

A. *Breech.*—Of this presentation the numbers in primiparous and non-primiparous cases respectively, at the full time, were 2 and 7.

2 to 220 is 1 to 110
7 to 780 is 1 to 111·4

As regards this presentation, therefore, there is thus seen to be a small excess of frequency on the part of the primiparous cases. Let us next look to the results to the children in such labours. Of the two primiparous breech cases, the child was saved in the one, but lost in the other. In the first it was born "*still*," but by the adoption for a few minutes of the Marshall-Hall method for suspended respiration in the drowned, breathing was established. In the second, twenty minutes elapsed between the delivery of the body of the child and of the head, notwithstanding all the extractive aid that was given, and pulsation of the cord ceased about five minutes before the head was born. Of the seven non-primiparous cases, the children were born alive in five, and dead in two. Death in one of these two cases was occasioned by hæmorrhage from placental presentation; in the other it was produced by pressure on the cord between the birth of the body of the child and the head. In this latter case the back of the child was turned to the back of the mother,—a position it is well-known which greatly increases the risk of fatal injury to the child. Of the five non-primiparous cases in which the child was born alive, the position of two relatively to the mother was not noted; in the remaining three the back of the child was turned (obliquely) to the belly of the mother. In all of these five cases, with one exception, the children had breathing well established immediately after birth. In the exceptional case, half an hour was spent in varied efforts at exciting this function ere the desired result was attained. In all the cases the practice was followed of allowing the breech to pass before using any extractive efforts to aid delivery.

B. *Footling.*—The number of cases of this presentation at the full time being 4 to the 1000, is in the proportion of 1 to 250. Of the 4 two were primiparous and two non-primiparous. The proportion, therefore, stands thus:—

| | |
|---|---|
| Primiparous, . . . . | 2 to 220 or 1 to 110 |
| Non-primiparous, . . . | 2 to 780 or 1 to 390 |

It will thus be seen that footling presentations at the full time occurred about three and a half times more frequently in first than in subsequent labours.

*Results to Children.*—1st, The Primiparous.—In both primiparous cases the children perished. The fatal issue occurred, as in the breech cases, from prolonged pressure on the funis after the birth of the body of the child, owing to inefficient uterine action protracting the delivery of the child. 2d, Non-primiparous.—In one of the non-primiparous cases the labour was over before my arrival, and the child dead. Those in attendance stated that a long period elapsed before the head was born after expulsion of the body of the child; fatal pressure of the cord had therefore taken place. In the other case, extractive force was exerted by the presenting foot, and so soon as in this way the other foot was brought within reach and got down, delivery was easily accomplished. The child was speedily quite vigorous.

C. *Arm.*—Two arm-presentations to 1000 cases is 1 to 500. Both having occurred in non-primiparous cases gives the proportion of 2 to 780, or 1 to 390 of such labours. Turning was performed in both cases under chloroform by means of one foot, with moderate ease; and although by and by the children were vigorous and lively, they were at first so asphyxiated that it seemed for a time quite as doubtful as in the case of poor Oliver Twist, whether they would be got induced to "take upon themselves the office of respiration."[1]

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[1] Here, in connexion with the subject of "turning," it may not be inappropriate to advert very briefly to an extraordinary discovery, of late launched forth upon the profession with all the gravity of seemingly profound conviction,—a discovery so undeniably original, that we dare no longer embrace in our creed the venerable aphorism, "*there is no new thing under the sun.*" It is in substance this, that nature in bringing it to pass that about 96 out of every 100 children (born at the full time) shall enter the world by the cephalic instead of the pelvic extremity, has committed an egregious mistake; that we therefore, as her tutors and guardians, should interpose our services to correct this blunder,—to reverse this mal-arrangement,—and in every case of cranial presentation to turn the child and deliver by the feet! The earnestness with which this notable discovery has been announced and inculcated, evidently implies a belief that its universal reception would constitute an obstetric millennium, in which both maternal and fœtal life would be placed on an eminence of security they have never hitherto reached. Doubtless, the genesis of such

D. *Funis.*—Five cases of funis presentation to 1000 is 1 to 200. Of the five, two were primiparous and three non-primiparous.

| | |
|---|---|
| Primiparous, . . . . | 2 to 220 or 1 to 110 |
| Non-primiparous, . . . . | 3 to 780 or 1 to 260 |

Funis presentations were thus met with 2·36 times more frequently in the former than in the latter class of labours. In both of the two primiparous cases the children were born dead. In the first the child perished from ante-partum hæmorrhage; in the second, from pressure on the cord during delivery by turning. Of the three non-primiparous children with funis presentation, one perished and two were saved. In No. 1 of these three, version was performed, but pulsation ceased in the cord before delivery was half accomplished. In No. 2, the coil of funis was always pushed up into the uterus during the interval between the pains, and at length it was got so far introduced that it did not again protrude, and labour went favourably forward. In No. 3, re-position of the protruded portion was effected in a similar manner, the head quickly descended, and the child was born alive and vigorous.

E. *Face.*—This presentation occurred only once in the 1000 cases, and in a non-primiparous labour. The chin of the child was directed first to the left acetabulum, and then to the pubis. The uterine action being strong, and the child's head of moderate size, delivery was not long delayed, and the features were not greatly distorted.

F. *Face to Pubis.*—There were 10 such presentations, being 1 to 100. Two were primiparous and eight non-primiparous.

| | |
|---|---|
| Primiparous, . . . . | 2 to 220 or 1 to 110 |
| Non-primiparous, . . . | 8 to 780 or 1 to 97·5 |

This presentation was thus found to be more frequent in subsequent than in first labours. In nearly all of these cases the uterine action was feeble and the labour consequently tedious. In two of them the forceps were required, viz., once in the primiparous and once in the non-primiparous cases.

(*To be continued.*)

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a dogma is referable to exalted views of human nature. For it *will* strike certain speculative minds as very sad and very undignified, that so noble a race as ours, possessing the "vultus ad sidera tendens" (a distinction not shared with us even by our forest friend and next of kin, the gorilla), should come upon life's stage with "the dome of thought" turned upside down! But the sadness and the *safety* of the thing are quite distinct; and if the latter can be satisfactorily established, then surely it is our duty loyally to abstain from all *per*-version[1] of nature's arrangement. Hitherto we have been accustomed to believe that "the principal peril which the fœtus undergoes during labour, is the danger of asphyxia from compression of the cord," and that "in head presentations the risk of this is immeasureably less than in pelvic presentations" (Simpson). I cannot persuade myself that the day will ever come when the profession shall abjure such sound and time-honoured belief, and accept in its stead the absurd vagary to which I have adverted. Verily, in such a case, midwifery would have reached a *turning*-point in its history which the friends of humanity would have good cause to deplore.

[1] As an "oxide" when raised to its highest state becomes a *per*-oxide, so "version," when carried to its utmost limits, becomes *per*-version: and "perversion," our great Lexicographer tells us, means "a change for something worse."

## ARTICLE V.—*Case in which Ascites, Coma, and Diabetes were combined.* By JAMES L. KING, F.R.C.S.E., Prestonpans.

ON the 1st of June last, a peculiar case came under my notice. Mrs A., aged 82, a well-proportioned and apparently healthy woman, complained of breathlessness, weakness, and pain in the chest and right side. On examination, I found that there was tenderness upon pressure over the region of the liver and upper part of the abdomen; but, on percussion and palpation, while there seemed to be no enlargement of the liver, there was obviously a large collection of fluid within the cavity of the abdomen. The heart was of large size, and probably flaccid, for it was weak and irregular in its action. The difficulty of breathing was constrictive, and occurred in marked paroxysms, accompanied by pain in the region of the heart; while the patient seemed fidgety and despondent. The pulse, alternately quick and slow, did not exceed 85 per minute. The tongue was red near the margins, and covered with a white coating down the middle. The bowels were sluggish, and the action of the kidneys was much diminished. The urine was loaded with lithates; but the presence of albumen, though suspected, was not discovered on application of the usual tests.

In this case it seemed clear that the state of the liver and the increasing ascites were the diseased conditions mainly requiring the employment of treatment. Accordingly, I commenced a purgative, alternated with a diuretic plan of treatment, and conducted this method for about three weeks, without any abatement of the symptoms. On the contrary, the ascites had increased, and the legs had also become swollen; while the other symptoms were proportionately aggravated. The action of the kidneys had become greatly diminished, and only a mere dribble of high-coloured urine was voided. The bowels had been regularly and frequently evacuated almost every day; though I was afraid of pushing the purgatives too far on account of the weak state of the heart.

The treatment hitherto pursued was now suspended; after using a host of diuretics and purgatives, mercury was administered in small doses, so as slightly to affect the gums. Another week was spent under this plan, after which the patient became daily worse. Scarcely any food was taken, though previously she had partaken at intervals of small quantities of rice and milk, beef-tea, and gin-negus. The treatment, after the exhibition of the mercury, was limited to the use of the acetate of potash in water. I now found that the bowels had not been moved for two days, and therefore determined, weak though the patient was, to risk the administration of a brisk purgative. Accordingly the patient was ordered a dose of calomel and jalap. Next day the bowels were moved frequently, and during the evening, though their action had ceased, I was rather surprised to find that the patient seemed listless, and weaker than she had been during the day.

Soon afterwards I was informed that she had become suddenly worse, and upon hastening to see her, discovered that she had passed into a state of complete insensibility, with loss of muscular power. There was no convulsive character about the seizure, and she had gradually become insensible. She could not speak, and sensibility appeared to be abolished. Her eyelids were half-closed, and the eye was glassy and turned upwards; the pupils were rather contracted. Reaction set in next day, accompanied with redness and heat of skin. She was restless, and constantly moved her hands, shifting them from one place to another; and the breathing and the pulse, which had been more regular since the occurrence of the apoplectic seizure, again became more irregular in character; and, as a kind of "busy delirium" now began to manifest itself, she would attempt to raise herself in bed to the sitting posture. She was supported in this position by her friends,—the head hanging forward, with the chin leaning upon the sternum. A kind of clonic spasm in the muscles of the limbs was now superadded to restlessness,—the case further undergoing exacerbation through excessive excitement; but, on the beginning of the third day after the apoplectic seizure, and at the height of the paroxysm, all the symptoms underwent remission: the kidneys began to act most freely, the dropsy in corresponding proportion disappeared, and the circulatory and respiratory movements assumed their more normal equilibrium of action. The patient, at this stage, began to recover her consciousness; and, towards the latter part of the day, she passed into a sound sleep; and, after enjoying a slumber of about five hours, she got over the bed, unnoticed by the friends, and voided a large quantity of urine. Her friends, speaking to her in wonder concerning this feat, she only replied in a tone of astonishment,—"Where am I? What has been the matter? for my mind is absent, and my body is very sore." She continued to improve with singular rapidity, and she soon walked about, but occasionally complained of feeling a little tired and giddy. Evidently no lesion of the usual characters followed upon the occurrence of the apoplectic attack; though one could readily detect that there was more than an ordinary difficulty in selecting words for conversation. The ascites and dropsy completely disappeared. The other important fact in the history of this case is that the urine turned out to be diabetic, upon remission of the disorder, during the clearing away of the dropsy. I was at first led to suspect the presence of sugar in the urine from its odour; and, on application of Moore's and Trommer's tests, there was abundant evidence of the saccharine deposit. The sugar, however, gradually disappeared, after the lapse of a week from the remission of the symptoms already described.

*Remarks.*—Though diabetes is known to occur in connexion with several manifestations of disordered function, it is generally regarded as a special disease, having a definite origin and doubtful prognosis. Yet the production of sugar in the urine can only in rare instances be traced to a definite cause. In the case of this woman, however, it seems to me that there was a decided connexion between the diabetic state of the urine and the head symptoms. It is further worthy of note, that the liver at the outset of the illness showed manifestations of disease, and this might suggest that the derangement of that organ was the source of the glucogensis. The history of the case, however, would seem to make this opinion improbable, and would induce me to believe that the production of the diabetes depended upon a neurosis.

It is still undetermined whether diabetes is not in every case primarily due to some morbid affection of the nervous centres. Already we recognise in the medulla oblongata and brain, a power capable, when exerted upon other organs, "of producing or preventing the saccharine metamorphosis."

To approach nearer the theory of the causation of the disease; Dr Pavy, when speaking of the phenomena produced by experiment, says—"The effect was supposed to depend upon a stimulation of the nervous centres. I cannot now coincide with this: I regard it as a loss of the proper nervous influence." But, after another series of experiments performed upon animals by Dr Pavy, he modifies his opinion, and says—"I have strong reason to believe that it does not depend, as I originally started out with the notion of, on a simple interruption of transmission of nervous force between the medulla oblongata and the liver."

First of all, then, it is to be noticed in this woman's case that the diabetes was merely of a symptomatic temporary character. It had likewise a distinct connexion with the progress of the illness, manifesting itself after the occurrence of the head symptoms.

Dr Goolden's interesting published cases may here be cited, where diabetes and head symptoms were known to exist at the same time; and where, by treating the affection of the head with blisters and purgatives, the saccharine urine was speedily "checked or removed." It also seemed evident that in this case the diabetes originated, or was connected with, the apoplectic attack, and was consequent to it, and a concomitant upon the draining away of the dropsical fluid—the hyperurisis and diabetes existing in that relation; so that after the apoplexy had occurred, recovery began to manifest itself; the dropsy disappearing, when the urine became saccharine.

Supposing, therefore, that the apoplectic attack led to the occurrence of the diabetes, would it seem rash to suppose that the rapid cure of the dropsy was due to the same source? From the facts of the case, such a possible "modus operandi" of one lesion following upon another should not, I think, be lost sight of. It may, indeed, be said that effusion within the head (which possibly was the cause of the apoplectic attack) could not have occurred to so large an extent as to have produced the diabetes in question; for if the apoplexy had resulted from this cause, the lesions dependent upon its existence would have been more marked and serious in their nature. Still, we may believe that the brain may have been as quickly relieved of the superabundant fluid as the system generally.

Assuming the apoplexy and diabetes to have occurred from the same cause—viz., from a "depression" or interference with the proper balance of nervous power, from the progress and increasing severity of the patient's disease—does the cure of the dropsy, on the simplest estimation, seem to have originated from the same cause? Or, on the other hand, are we to suppose that the fit existed in the relation of a coincidence in the progress of the illness? By way of elucidation, let us view the occurrence of the coma and diabetes as originating from a more general cause, assignable to the head, but nevertheless due to the nervous system; as, for example, in the case of "shock," which presents an array of symptoms very similar to those which existed latterly in this case.

Under what influence then were the kidneys made to act so freely at this juncture? Was it from the brisk action of the purgative powder, which took effect at the expense of the "shock" imparted to the nervous system—the case having become latterly more asthenic in its nature? This view is supported by the success of the treatment recommended in congestion of the kidneys with suppression of urine. Independently of bleeding, purgatives have been found to give peculiar relief, and often to work a speedy cure; and it must be kept in mind that for the most part they have to be repeated often, in order to effect the desired result. And if, from such a proceeding in such cases, recovery ensue, it must simply arise from a "depression or deprivation" of that morbid power which has promoted the unbalanced action of the nervous and vascular systems, and upon a similar hypothesis of the nature of the causation of diabetes as has been advanced by Dr Pavy and others.

But the recovery did not seem to occur from a like direct result as is herein indicated, and as is due to the employment of medicine; for the apoplexy having manifested itself, probably led to an abnormal excitation of the nervous centres (as was noticeable during the fit), and this state being continued would subsequently induce a more specific "depression" of that nervous power, which very possibly brought the kidneys into increased action, and at the same time led to the production of the saccharine urine.

The fit which occurred in this case, I have designated as being of an apoplectic character, yet it was rather indicative of a form of epileptic seizure. But the slow invasion and protracted nature of the fit afford a presumption that it may have been primarily due to the presence of urea into the blood; and probably, therefore, it was more properly a uraemo-apoplectic attack, uncontrolled by the course of treatment employed.

It would not therefore seem proper to attribute much value to the treatment in effecting the ultimate cure, especially if we view the apoplexy as influencing the production of the diabetes, ending in the recovery. And otherwise, whatever might be the danger incurred in treating similar cases in younger persons, in this aged female's case the practice may have led to a more fortunate result than could have been anticipated in such circumstances.

## ARTICLE VI.—*Experiments on the Influence of Ozonised Air upon Animals.* By W. W. IRELAND, M.D. Edin.

THE first means used to produce ozone was by burning phosphorus in closed vessels. The gas was then passed through water, in order to get rid of the phosphoric acid. This, however, did not separate it completely: the air was still charged with the white vapours of phosphoric acid, which, of course, would tend to vitiate the experiment. Moreover, by producing ozone in this way, the relative proportion of oxygen to the nitrogen in the included air was notably diminished by the large amount of oxygen taken up in the oxidation of the phosphorus. I then tried another way to procure ozone—two parts of powdered hypermanganate of potash with three parts by weight of sulphuric acid were introduced into a glass bottle. The sulphuric acid acts slowly on the hypermanganate, setting free some of its atoms of oxygen in the form of ozone, which is thus added to the enclosed air. The permanganate of potash and sulphuric acid were introduced successively into a glass bottle, the cork of which was fitted with two tubes; one of these communicated with a smaller double-necked bottle containing water; the second neck gave passage to a tube which communicated with the spout of a glass jar. This jar had a short broad neck and a large wide shoulder, which became like a balcony when turned upside down in a plate of water. This jar received the ozonised air when pushed through the water, which was done by a pair of bellows communicating with the other cork tube of the first bottle. If the action was allowed to go on too rapidly, the ozone became mixed with a violet-coloured vapour,[1] which, after some days' use, gave a rose tint to the pipes down nearly to the water, but not to those that received the ozone after passing through the water, which ozonised air, as before said, was collected in a jar turned upside down in a plate with water slightly heated.

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[1] No doubt the same mentioned by Berzelius as produced by the action of sulphuric acid on hypermanganate of potash at a temperature of 130° Centigrade, and found to be a mixture of permanganic and sulphuric acids. Professor Fresenius, to whose kind advice and encouragement I owe much, approves of my apparatus, but remarks that, to purify the produced gas, "tubes filled with powdered pumice-stone are more efficacious than water alone." I imagine, however, that the Professor refers more particularly to the ozone produced by phosphorus.

The animal introduced took refuge from the contact of the thin stratum of water into the shoulder of the jar. In all the experiments, save No. I., hypermanganate of potash was used.

*Experiment I.*—A mouse exposed to ozonised air for one hour—very much excited, leaping and running about—did not appear much diminished in vigour at the close of the experiment, but gradually became weaker, and died in about an hour and a half after being withdrawn.

*Experiment II.*—3½ grammes of hypermanganate of potash.—A mouse kept in one hour—respiration accelerated—much excited—leaped and ran round about the jar; when taken out, strong enough to try to escape—warmed, and offered food—died in about ten hours after, of gradual exhaustion.

*Experiment III.*—3½ grammes of hypermanganate used.—A frog kept in for two hours—respiration accelerated—eyes fixed and starting—no excitement; seemed exhausted, but recovered quickly, and did not appear to be the worse next day.

*Experiment IV.*—3½ grammes.—A sparrow shut in half-an-hour—passed the stream of ozonised air through fresh calves' blood instead of water; it caused the blood to coagulate a little more quickly, but had no other visible effect. The respiration of the sparrow became hurried; it looked pert and bold and much excited, but soon became exhausted—number of respirations increased by somewhat less than one-third. The little animal seemed much fatigued when let out, but quickly recovered. The same sparrow was introduced next day—did not appear so readily affected as before, and was allowed to escape.

*Experiment V.*—3½ grammes.—A sparrow shut in one hour—great excitement—respiration accelerated by one-third—symptoms of sinking, with occasional convulsions; very much exhausted when taken out, but recovered in an hour or two. The sparrows recovered better than the mice, probably owing to the greater extent and activity of their respiratory apparatus.

*Experiment VI.*—3 grammes.—A mouse; at first seemed exhilarated—respirations short and quick, difficult to count. In an hour appeared weak and exhausted, but excitement increased—running and leaping about. In three hours it died in the jar.

I examined the bodies of the three mice that died; the venous blood appeared to be lighter in colour than usual. In No. II. the lungs appeared to be congested, but pieces of them floated in water; the hearts still contained blood. No other changes could be made out, so I determined on an experiment with a larger animal.

*Experiment VII.*—5 grammes.—A rabbit shut in a hermetically sealed box with two little glass windows—respiration accelerated exactly one-third—no excitement observable; temperature before being put in 36° Centigrade, but temperature only 31°. When let out surface felt warm, and it ate the very moment it was out. Kept two days longer under observation; it appeared to be quite well.

*Experiment VIII.*—12 grammes.—A rabbit kept in four hours—respiration much accelerated—no visible excitement, save a very little which appeared towards the end—evidently did not suffer much uneasiness. When let out, dull and exhausted, but able to walk—stunned with a blow on the back of the neck, thorax opened, heart still beating—blood coagulated instantaneously in the pericardium when the aorta was cut—clot very firm and tenacious. A little more blood collected in a shallow dish coagulated at once. Arterial and venous blood retained their distinguishing hues—perhaps venous blood a little redder, but I could not be certain. No other changes noticed—stomach, intestines, and bladder full. Portions of the blood, brain, lungs, liver, kidneys, and contents of stomach, put into closed phials, and tried with the ordinary ozone test-paper as well as that of Houzeau, but no ozone could be detected save in the lungs, and a trace in the contents of the stomach.

These, as might be expected, resemble the results of the exposure of animals to oxygen gas, which, however, is described as bringing on comatose symptoms, and, when the animals are withdrawn, as leaving less deleterious after-effects than in Nos. I. and II.

I never observed any coma, and I failed to produce "bright scarlet blood in the veins"[1] with ozonised air as has been done with oxygen.

These experiments were most carefully performed, and all sources of complication avoided as carefully as possible; and, as I felt satisfied of their correctness, I saw no reason to sacrifice the lives of more animals in repeating them. I submit to the reader the following conclusions:—

I. Ozonised air accelerates the respiration, and, we may infer, the circulation.

II. Ozonised air excites the nervous system.

III. Ozonised air promotes the coagulability of the blood, probably by increasing its fibrin. In the blood, however, ozone loses its peculiar properties, probably entering into combination with some of the constituents of the circulating fluid.

IV. Animals can be subjected to the influence of a considerable proportion of ozone in the air for hours without permanent injury; but in the end ozone produces effects which may continue after its withdrawal and destroy life.

AVIGNON, 18*th December* 1862.

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[1] Carpenter's Physiology, 1853, p. 543.

# Part Second.

## REVIEWS.

*Contributions to Practical Medicine.* By JAMES BEGBIE, M.D., F.R.S.E., Fellow, and late President, of the Royal College of Physicians, Physician in Ordinary to the Queen in Scotland, etc. etc. Edinburgh: Adam and Charles Black: 1862.

THE work, the title of which forms the heading of this article, is one to which we have peculiar pleasure in directing the attention of our readers. To many of them Dr Begbie is personally known; to others his name is familiar as that of a skilful and judicious physician; while by a still larger circle he is appreciated as the contributor of various interesting, though too unfrequent, articles to periodical medical literature.

The present volume consists, with a single exception, of papers read before the Medico-Chirurgical Society, and already published. Increased value is, however, imparted to them by the author having in many instances made considerable additions, which contain the results of his most matured experience. Ten important subjects are treated of in as many chapters; and it is a striking and valuable characteristic of the work, that the sections are not devoted to a consideration of the mere symptomatology and treatment of disease, but that in most cases an attempt is made to determine the peculiar morbid constitutional conditions on which the special manifestation depends. Of course we do not agree with Dr Begbie on every point. Pathology is so vast, and many of the subjects it embraces are still so obscure, that scarcely any two independent minds can arrive at identically the same results. But, when we differ from Dr Begbie, we always feel that we are dealing with conclusions founded on a large experience, and only arrived at after careful study and mature reflection.

The first chapter treats of gout and the gouty diathesis. Dr Begbie does not enter into the consideration of the acute and well-characterized form of the disease, but confines his attention chiefly to those obscure and ill-characterized affections which are generally included under the term irregular gout. With regard to this subject there is much difference of opinion. Some almost entirely deny the existence of such a diathesis, maintaining that the affections referred to it are independent of gout, although they may be allied to it, and may be produced by the same causes; while others believe that there is not an organ which may not be attacked by it, not a function which may not be deranged by it, and not a lesion which may not receive a peculiar stamp from its existence. Dr Begbie is a strong supporter of the latter of these opinions. He agrees with Dr Gairdner, that "the strumous is not more frequent than the gouty habit;" and in the early part of his article writes thus:—"Gout is a blood disease: it may disclose itself in every organ of the body, and complicate and involve every disturbance of the system. It may visit every part and every texture, from the crown of the head to the sole of the foot, and molest and vitiate every function appertaining to life." We are quite willing to believe that gout is more common than many are disposed to admit, and that the gouty diathesis plays a very important part in the causation of special disease, but we require evidence before we can assent to such a proposition as that laid down by Dr Begbie. Many writers on this subject seem to expect that we shall be convinced by mere assertion, or, at most, by probability. In this respect Dr Begbie is not faultless. Many of his cases, doubtless, are examples of gout, but with regard to others there is little more than a probability that the affection was a manifestation of the gouty diathesis. The following case, for instance, is brought forward to prove that gout plays an important part in the production of inflammation of the membranes of the heart:—

"I had for many years an opportunity of watching the development of the gouty diathesis in a young lady whose family history afforded more than one proof of the prevalence of the disorder. Her father had died of heart affection, which there is reason to believe was associated with gout. Her sister immediately senior to herself had for many years been affected with frequently recurring attacks of gouty psoriasis, accompanied with gastric and hepatic derangement, which yielded only to remedies of an anti-arthritic character; and a maternal uncle had been for many years the victim of gout in its purest and most undisguised form, and to such excess, that the fits, in frequency and severity, baffled all attempts to control them. With so much of her predisposition known, Miss T. came under my care about two years previous to her death, at the age of twenty-five, with symptoms of obscure heart affection, alternating with those of gout in the small joints of the fingers, and the usual derangement of the stomach and disorder of the renal and biliary secretions. At this time there existed a slight endocardial murmur with the first sound, which after repeated examination I satisfied myself was not anæmic; and it was persistent through life. She frequently complained of pain in the region of the heart, and of preternatural impulse, and these were always aggravated by exertion in walking, or by ascending a stair. Her general health was otherwise good, and remained undisturbed save by the occasional returns of the gouty pain, and swelling of one or more of the metacarpal or finger-joints, and of cardiac uneasiness and palpitation. The attacks yielded to the alkaline salts, with small doses of colchicum, and, in the interval, benefit appeared to be derived from cod-liver oil. Two years had passed, with frequent recurring fits of illness, when, at the close of January 1849, Miss T. was seized with severe pain in the præcordial region, with hurried breathing, and more than usual pulsation. The pain and swelling of the fingers were at the time of moderate character, and quickly disappeared. When seen on the second day of her illness—for long acquaintance with symptoms of a mitigated kind had made her too regardless of her present state—she was found lying on her back, with her shoulders raised in bed, her countenance pale and anxious, her breathing hurried, and performed with pain, her pulse rapid and small, her skin hot and moist. There was pain over the region of the heart, increased by pressure between the ribs, and on the epigastric region. There was extended præcordial

> dulness, and distinct rubbing sound, in which was lost the ordinary endocardial murmur. The rubbing sound by and by gave way. The mind continued unclouded. Repeated leeching, the rapid introduction of calomel and opium, together with colchicum and other adjuvants, appeared to produce no sensible effect on the still more rapid effusion; and with feeble and uncountable pulse, and tumultuous, trembling action of the heart, she sank on the third day. She died, I doubt not, of pericarditis with extensive effusion, caused by the specific poison of gout in the blood, but no *post-mortem* examination was permitted to reveal the full amount of the damage thus sustained."

Regarding this case, we would remark that the proof that the pericarditis was of gouty origin is to us by no means satisfactory. There is no evidence that the heart affection which proved fatal to the father was not the result of rheumatism; and, judging from the age and sex of the patient, we should be inclined to believe that the pain and swelling of the joints of the fingers was not gouty, but rheumatic. Morbid anatomy affords most important aid in this inquiry. Dr Garrod has satisfactorily established, that true gouty inflammation is invariably accompanied with the deposit of the urate of soda; and the same excellent observer states, with reference to chronic valvular diseases of the heart, atheromatous degeneration of the bloodvessels, and other lesions supposed by many to be gouty, that he has ascertained by post-mortem examination "that even when such changes occur in subjects who have been long afflicted with gout, they have not the same composition or structure as those essentially connected with this disease."[1]

In establishing his position regarding the very wide diffusion of gout, Dr Begbie attaches considerable importance to the circumstance that in many cases the symptoms described yielded to an anti-gouty treatment, and particularly to colchicum. To this source of evidence we do not, however, attach very great weight. No doubt colchicum is, as a rule, more useful in gout than in rheumatism, but our knowledge of the mode of action of that drug is as yet too imperfect to justify us in founding a great deal on its success in one case and its failure in another. Dr Begbie believes that it acts by promoting the elimination of uric acid from the system; but precise experiments have recently shown that this result is not generally, or indeed commonly, observed; and the earlier experiments, which seemed to show that the amount of uric acid was increased under its use, were vitiated by the analyses having been performed on urine discharged at particular times of the day, and not on the whole urine passed in the twenty-four hours.

Dr Begbie's remarks regarding the treatment of gout are very judicious. In addition to the means commonly recommended, he thus speaks regarding what will be to many a new agent in the treatment of the disease. "I cannot close without referring to the singular benefit which may be derived from cod-liver oil, in some of the morbid conditions of the gouty. In the chronic and asthenic forms, generally, and in the more acute, after the severity of the disease is overcome, in almost all the chronic ailments connected with the gouty habit, and in the cachectic condition of the offspring of gouty parents, I have found it to be possessed of wonderful renovating powers. Nor have I been less agreeably surprised to find how potent, after persistent use, it is in the least promising of the gouty sequelæ which can be reached by medicine; I mean the nodosities of the fingers and toes, and the crippling, lameness, and deformity occasioned thereby."

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[1] Garrod on Gout and Rheumatic Gout, p. 497.

The next chapter is entitled, "On the relation of Rheumatism and Chorea." To Dr Begbie belongs the credit of having first clearly pointed out the close connexion of these diseases. Other writers had indeed noticed that they were occasionally associated, but it was generally supposed that the nervous symptoms depended upon a metastasis of the rheumatic affection to the spinal cord, or to an irritation of the phrenic nerve occasioned by inflammation of the pericardium. Dr Begbie, however, came to the conclusion, which has now been generally accepted, that chorea is dependent upon the morbid condition of the blood which exists in rheumatism, and that it is connected with affections of the heart in no other way than that both are produced by the action of the same poison. For the treatment of chorea, Dr Begbie strongly recommends arsenic; he has never known it to fail, or give rise to bad effects.

The third chapter is closely related to the preceding; it treats of the connexion of erythema nodosum with the rheumatic diathesis. The following are the conclusions which Dr Begbie arrives at:—

"*1st*, The skin affection is most prevalent in, if not confined to, the young, and those under thirty—the chief subjects of the rheumatic diathesis and rheumatic fever.

"*2dly*, It occurs very frequently in females suffering from menstrual derangement, confirming the views of Drs Todd, and Rigby, and Sir Charles Locock, as to the intimate connexion of disorder of the uterus with rheumatic affections.

"*3dly*, It occurs in connexion with disorder of the general health, characterized by pallor, cachexia, and defective excretion, and subsequently developed in febrile excitement, pains in the joints and muscles, and the copious deposition of lithates in the urine—a state of matters analogous to what takes place in rheumatism.

"*4thly*, It is associated frequently with rheumatic fever, co-existing or alternating with it.

"*5thly*, It is often complicated with those internal disorders with which rheumatism is allied, particularly with pleurisy and pneumonia. It has also been noticed in connexion with endocarditis, or other cardiac disease.

"*Lastly*, It is successfully treated by colchicum and alkaline salts, and yields also to a plan of treatment—I mean the use of bark—which was long extensively and successfully employed in the treatment of rheumatism, and still has its advocates among the best-informed physicians of the day."

The fourth chapter is headed, "Anæmia and its consequences; Enlargement of the Thyroid Gland and Eyeballs." It is principally devoted to that peculiar protrusion of the eyeballs to which attention was first specially directed by Dr Graves, which is often associated with anæmia, and has been known by various names, among which may be mentioned anæmic exophthalmia, and cachectic exophthalmos. Dr Begbie details several cases of this curious affection, and arrives at the conclusion that it is dependent upon an impoverished condition of the blood. His conclusions, however, we shall give in his own words:—

> "1*st*, That the essence of the disease consists in a vitiated or impoverished condition of the blood—a true anæmia.
>
> "2*d*, That this abnormal condition, this attenuated quality of the blood, acting directly on the cardiac nerves, through the rapidity with which it circulates, excites the heart and vessels to over-action.
>
> "3*d*, That the anæmic palpitation thus produced is, after a longer or shorter time, followed by turgescence of the vessels of the head and neck, and by enlargement of the thyroid gland, and protrusion of the eyeballs.
>
> "4*th*, That these phenomena are subject to remarkable variations as to intensity according to the state of the heart's action.
>
> "5*th*, That the acoustic signs in the heart, in the vessels of the neck, and in the thyroid gland, are those of anæmia; and cannot be referred to any other morbid condition with which we are acquainted.
>
> "6*th*, That the disturbance of the circulation, the turgescence of the thyroid gland, and the protrusion of the eyeballs, are all capable of mitigation and removal, by means calculated to improve the quality of the blood.
>
> "7*th*, That this condition of the vascular system, if not removed, issues in organic change, in hypertrophy and dilatation of the chambers of the heart, in enlargement of the great vessels, in induration and structural degeneration of the thyroid gland, and in disorders of vision, and partial or complete loss of sight."

To these conclusions we are by no means prepared to give an unqualified assent. That anæmia and protrusion of the eyeballs frequently co-exist, is undoubted; but it does not follow that they are related in the connexion of cause and effect. Anæmia is a very common condition; but why is it that cases of protrusion of the eyeballs are comparatively rare? It is very true that the causes of disease do not act with, by any means, the same certainty as the causes which are under the control of the chemist or the natural philosopher. But it is surely evident, that if a morbid condition be the cause of a special symptom, in the majority, or, at least, in a large proportion of the cases where the morbid condition exists, the particular symptom should manifest itself. This, however, is not the case with regard to anæmia and protrusion of the eyeballs; it is only in a small minority of cases of anæmia that exophthalmos occurs. We must therefore allow that the existence of anæmia alone is not sufficient to determine exophthalmos; and we must admit that in the exceptional cases some other cause must have been in operation. Nay, more, it is possible, not to say probable, that the same cause which determined the protrusion may have occasioned the anæmia. Many circumstances lead us to look to the nervous system as closely connected with the phenomenon in question. Experiment has shown that the protrusion may be produced artificially by irritation of certain parts of the nervous system; in many cases nervous symptoms have first shown themselves, and have then been succeeded by anæmia and exophthalmos; in others the protrusion is paroxysmal, and is manifestly under the influence of mental emotion. The subject is too wide to enter into within the limits of this article, but we trust we have said enough to satisfy our readers that the pathology of this condition is far too complicated to be explained by a simple reference to an impoverished condition of the blood.

It is, however, satisfactory to know, that, obscure as the condition is, it is not unamenable to treatment. In many cases the tonic regimen, combined with the persistent employment of chalybeates, has effected a cure, or has at least brought about a marked alleviation of the symptoms.

The fifth chapter of Dr Begbie's work is devoted to a consideration of that form of dyspepsia and nervous disorder, which is associated with the oxalic diathesis. Two opinions have been entertained in regard to this condition: some believe that the existence of oxaluria is the indication of a specific condition; while others, and these, we think, with better reason, consider that the presence in the urine of crystals of oxalate of lime is merely the indication of an abnormal condition of the processes of digestion and assimilation, and may be determined by a great variety of causes. Dr Begbie does not enter particularly into the general question, but records some interesting cases illustrative of the great importance of an early recognition of the morbid state, and of the efficacy of nitro-muriatic acid in effecting a cure. Though this subject has been long before the profession, it has not even yet met with the attention it deserves; and many cases are allowed to go on unrelieved, which might at once be ameliorated by the institution of a treatment directed against the oxaluria.

The sixth chapter treats of fatty degeneration of the heart, and is rendered specially interesting by containing a record of the cases of Dr Chalmers and Dr Abercrombie. The symptoms of this condition are carefully described; but, unfortunately, little can be done in the way of treatment. Prophylactic measures may do something; but, in the majority of cases, fatty degeneration is but one of the indications of the gradual failure of the human machine.

Erysipelas forms the subject of the seventh chapter. Dr Begbie traces its origin in several cases to the influence of impure air, and shows the enormous importance of sanitary measures in warding off the affection. The remarks regarding treatment are interesting. Though fully acknowledging that erysipelas is not a mere local inflammation, but that it is connected with a morbid condition of the blood, Dr Begbie recommends bloodletting in certain cases. "Bloodletting," he says, "is, no doubt, frequently admissible, and often beneficial. It is not, however, I apprehend, so much by diminishing the force and frequency of the circulation, that it produces its beneficial effects, as by relieving the system, and particularly the brain and nervous system, from the full weight of the poison which has contaminated the blood, and thus restoring the powers of life, and enabling the depurating organs and emunctories to act and eliminate the offending matter. It is in this manner, I believe, that bloodletting is found useful in other cases of disease,—as, for instance, in small-pox, in scarlatina, and in rheumatic fever,—in all of which we must have noticed many examples of the remarkable effects of a well-timed and judicious loss of blood, in facilitating and promoting the action of other remedies, and so conducting the disease to a happy issue."—P. 228. Though we doubt not that the treatment recommended is judicious, we question whether its efficacy depends upon the grounds here laid down. The quantity of blood removed is too small to diminish materially the amount of the poison, and we believe that the remedy acts by relieving cerebral congestion, just as bleeding in pneumonia relieves symptoms by diminishing the congestion of the lungs. Dr Begbie strongly recommends the use of the muriated tincture of iron, and narrates cases illustrative of its efficacy. Many have doubted the powers of this remedy, but we believe that in many cases their want of success has arisen from its too sparing employment. Ten drops every four hours will produce little or no effect: to do good we must administer the medicine freely, in doses of twenty or thirty drops every two or three hours. There are, no doubt, cases of erysipelas where no treatment except direct and copious stimulation is of any avail; but there are many others in which the severity of the disease may be materially diminished by a judicious employment of the remedy in question.

The next chapter treats of diphtheria and its sequels; but, as it appeared in this Journal very recently, we shall not attempt to give any analysis of it. We may merely remark that it furnishes an admirable description of the affection, and proves that the disease, though under a different name, was known in Edinburgh upwards of thirty years ago.

The concluding chapters treat of the therapeutic properties of arsenic, and of the datura stramonium. There was an interval of thirty-five years between the original appearance of these papers; but both manifest the same carefulness of observation on the part of the author.

In conclusion, we heartily recommend this work to the attention of our readers: they will find in it much valuable practical information, while it has a peculiar value from inculcating the necessity of looking beyond the mere assemblage of symptoms, and of endeavouring to discover the cause of local morbid conditions. Disease is throughout regarded in a broad and general manner: mere empiricism is reduced to its proper level, and we are taught to treat the constitutional condition, in order that we may relieve the local manifestation. In taking leave of Dr Begbie we must express the hope that he will favour the profession with some further results of his enlightened experience.

*Army Medical Department: Statistical, Sanitary, and Medical Reports for the Years* 1859 *and* 1860. *Presented to both Houses of Parliament by Command of Her Majesty.* London: Harrison and Sons: 1861–62.

THE Statistical, Sanitary, and Medical Reports of the Army Medical Department for the year 1860, are a manifest improvement upon those for the year 1859. In the latter, the statistical portion was necessarily incomplete—partly because of the imperfect state of certain of the old forms of returns, but more especially owing to the total absence of information concerning the health of the troops serving in India. At the same time, in the sanitary and medical portions, amidst some valuable matter there was a great deal which might well have been omitted.

The Statistical Report for 1860 contains separate chapters on the health of the troops serving at home and in the different colonies. In each chapter the sickness and mortality, the extent of invaliding, the mean daily sick, and the influence of age on the mortality, are clearly shown; and in the first chapter, in which the health of the troops serving in the United Kingdom is considered, there is some additional information respecting the results of re-vaccination, the recruiting for the army, etc.

It would be foreign to our present purpose to review the whole of this report, which extends over 188 pages. We have, therefore, made the following summary of what seem to be the most important conclusions to be derived from it; and we have added some further information obtained from the Medical Report and other sources.

| | |
|---|---|
| Total number of troops belonging to the royal army at home and abroad in 1860, | 211,473 |
| Total number of deaths in do., | 4,102 |
| " of invalided in do., | 2,881 |
| Average of daily sick in do., | 11,766 |
| Ratio of deaths per 1000 in do., | 19·9 |
| " per 1000 of died and invalided in do., | 33 |
| " of deaths per 1000 amongst the civil male population of England and Wales of similar age,[1] | 10 |
| Number of medical officers in 1860, | 1,075 |
| Proportion of sick to each daily, | 10·9 |
| Proportion of troops to each, | 244 |
| Deaths of medical officers in 1860, | 32 |
| Ratio per 1000 of deaths of medical officers, same year, | 28·8 |
| " " amongst the civil population of England of similar age (from 25 to 55),[2] | 13·9 |

Thus we see that in the military service in 1860 the average mortality of the rank and file was very nearly twice as great as that of the civil male population (19·9 to 10); but this does not represent the whole amount of waste of life and health which is experienced in the army. In order to ascertain the latter, the number of those invalided for ill-health or disability should be added to the deaths, which would give 33 per thousand as the actual ratio of waste for that year.

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[1] Registrar-General's Reports, 12th vol. [2] Ibid. 16th vol.

It is also seen that there is one medical officer on the average to every 244 men, of whom there would be about 11 on the sick list; but in addition to these, the regimental officers, their wives, children, and servants—the wives and children of the soldiers—the officers, men, women, children, and servants belonging to the commissariat, store, barrack, and other departments, have to be counted. When, therefore, we remember the number of returns, etc., which the medical officers are required to make, and the various other duties, such as attendance at ball-practice, parades, sanitary inspections, etc., which they have to perform, we have no difficulty in believing that, as a rule, they are as fully occupied as medical practitioners in private life.

It is much to be regretted that in the Statistical Reports for 1859 and 1860 there is no mention made of the sickness or mortality amongst the medical officers. It is only from the brief statements made on this subject at the commencement of the Medical Reports that we have been able to obtain information on this head, and we must confess that it astonishes us beyond measure, because whilst we find the average mortality amongst the rank and file for 1860 to amount to 19·9 per 1000, that of the medical officers is seen to exceed 28 per 1000. It is necessary, however, to remember that the medical officers are not compelled to retire from the service until they have reached the age of fifty-five, and that the rate of mortality increases with age in all occupations; but even with this proviso we find their average mortality to be more than double that of the civil male population in England of similar age (28·8 to 13·9). If to this high rate of mortality we add the waste as evidenced by the numbers placed on half-pay on account of ill-health, the results will be seen to be still more startling; thus, in 1860, out of 1075 medical officers, 8 retired on permanent half-pay on account of ill-health, and 17 were placed on temporary half-pay from the same cause,—making a total waste of health and life amongst the medical officers of 51 per 1000 as compared with 33 per 1000 amongst the rank and file.

These considerations naturally lead us to inquire what advantages in position, pay, or prospects the Army Medical Department affords to its members in return for their exertions, and to compensate for their excessive mortality.

The profession of arms is in all countries one of the most highly honoured, but it is seldom highly remunerated by money payment: the soldier looks for honour and glory as his rewards, and he obtains them in this country to their fullest extent. The profession of medicine, on the other hand, receives but few honours from the State: even when allied with the profession of arms, and participating in its dangers, it has never obtained higher honorary distinctions than have been bestowed on physicians or surgeons in private life. We, therefore, take it for granted that military service *per se* adds nothing to the social position of the medical officer. He enters the service as a "*Doctor*," and as a "*Doctor*" he leaves it, even though he should in the meantime have obtained the relative rank of a general. The title of inspector-general, deputy-inspector-general, surgeon-major, or the like, can never be carried into private life: the result, therefore, in the vast majority of instances, is what we have stated,—that the medical officer obtains no advance in social rank by service in the army.

We have seen that in the Army Medical Department there are about 1000 medical officers, and we know that in the United Kingdom there are in round numbers about 16,000 registered medical practitioners in private practice. By way, therefore, of estimating the payment of the former let us contrast their pay in the several ranks with what we may assume to be the incomes derived from private practice—as in the following table:—

| ARMY MEDICAL OFFICERS. | | | | CIVILIAN PRACTITIONERS. | | |
|---|---|---|---|---|---|---|
| Numbers. | Rank. | Exact Pay per Annum. | Estimated Value of Pay, Allowances, Pension, etc. | Supposed Relative Numbers, in Classes. | | Estimated Yearly Incomes. |
| | | £ s. d. | £ s. d. | | | £ s. d. |
| 1 | Director-General.. | 1500 0 0 | 2000 0 0 | 16 | 1st Cl. | 4000 0 0 |
| 7 | Inspectors Gen... | *730 0 0<br>to<br>821 5 0 | 1000 0 0 | 112 | 2d „ | 3000 0 0 |
| 27 | Dep. Inspect.-Gen. | *511 0 0<br>to<br>620 10 0 | 750 0 0 | 432 | 3d „ | 2000 0 0 |
| 100 | Surgeons-Major ... | *401 5 0<br>to<br>456 10 0 | 500 0 0 | 1,600 | 4th „ | 1000 0 0 |
| 210 | Surgeons............ | *273 15 0<br>to<br>328 10 0 | 350 0 0 | 3,360 | 5th „ | 700 0 0 |
| 650 | Assistant Surgeons | *182 10 0<br>to<br>237 5 0 | 250 0 0 | 10,400 | 6th „ | 350 0 0 |

* According to length of service.

It cannot be asserted that the incomes derived from private practice are here overrated, or that the value of the pay and allowances received by the army medical officers is at all under-estimated. The conclusion, therefore, seems inevitable, that the pay of medical officers is below the average remuneration of civil practitioners of corresponding grade.

We shall have no better opportunity than the present for pointing out one or two of the errors that are commonly fallen into on this subject. First, It is often said that the payment of an assistant-surgeon on first entering the service is greater than what he could expect to receive from private practice at the same age; and such a statement contains nothing but the truth. It is not, however, the *whole* truth; for, if the deductions to which his pay is subjected, for contributions to mess and band, expenses for uniform, etc., be calculated, even at their average rate in the least expensive regiments, they will be found to leave him not even enough to live upon, whenever he happens to be serving at home, or in any expensive foreign station. Second, It is alleged that although the pay of the surgeon-major is only about £400 or £450 a-year, yet he can retire, at the age of fifty-five, on £337, 12s. 6d. a-year. This also is true, but it is not the whole truth of the matter. It is fairer and more correct to say that he *must* retire at fifty-five; when, although his pension may be supposed to represent a sum of £4000 sunk in an annuity, it is but a poor pittance for a man having perhaps a family to maintain and educate, especially when one remembers that he is now too old to attempt the rough and hard work of commencing private practice, notwithstanding that he might still be equal to the performance of the duties to which he had grown accustomed. Such is the view we take of the position and pay of the medical officers of the army. Let us now examine their prospects.

If we calculate from the numbers shown in the *Army List* for the present month, we shall find that, as there are 7 inspectors, and 27 deputy inspectors-general, nearly one in every 4 of the latter may expect to be promoted, and as there are 310 surgeons-major and surgeons, nearly one in every 9 of these officers may likewise calculate on promotion. This is all that can be said about the prospects in the service of the officers in these ranks; but with the assistant-surgeons the case is different, because their promotion takes place entirely by seniority, and its rate must depend upon the number of vacancies created by the deaths, retirement, etc., of their seniors. Let us suppose the death-rate to be about 3 per cent., bearing in mind that since October 1858, when the last warrant was issued, the mean rate of promotion of assistant-surgeons has been 14 a-year. As, then, there are, in round numbers, 350 medical officers above the rank of assistant-surgeon, the vacancies occasioned amongst them by death would amount to 10½, and by retirement, at the rate of 2 per cent. to 7—say 17 altogether. Then, in order to ascertain the probable number of years that any assistant-surgeon will have to serve previous to promotion, the number of assistant-surgeons senior to him must be divided by the mean between 17 and this number, plus the death-rate of those assistant-surgeons. For example, the assistant-surgeon who happens to be the 300th from the top of the list will get 17 steps a-year from vacancies occurring amongst the senior ranks, and 3 per cent. from the deaths of the assistant-surgeons his seniors; *i. e.*, $17+9=26$ steps the first year, and 17 the last—the mean between these two being 21·5; now, $300 \div 21{\cdot}5 = 13{\cdot}9$, the number of years he has yet to serve. And when it is considered that the assistant-surgeon of this standing has already served for about six years, it will be seen that his total length of service as assistant-surgeon will not be less, in all probability, than twenty years. We have said that there are about 650 assistant-surgeons on full pay during the present month, —What, then, must be the prospects of the 600th?

It is astonishing how very differently the Warrant of 1858 has been regarded by different individuals. When it was first published it was received with great acclamation by a certain party, but experience has shown that the plaudits which hailed it as the regenerator of the army medical department were rather premature. It certainly was a step in the right direction; but, so far from being a complete measure of reform, it was in fact only a very small instalment. We are sanguine enough to believe that further progress must soon be made, and we therefore venture to submit some of the opinions which we have formed concerning the errors of the existing system, and of the objects to be aimed at in the construction of a new.

When Sir James M'Grigor commenced his service at the age of twenty-three, he was able to purchase the surgeoncy of a regiment without having gone through any preliminary stage. The "hospital-mate" or "assistant-surgeon" in those days usually came from an apothecary's or druggist's shop. He almost never possessed any medical diploma or degree, and it was therefore only natural that he should occupy an inferior rank with little pay. Nor could fault be found with his designation of "assistant-surgeon," because it really expressed his occupation, and, in a great measure, his attainments. But since that time the most important changes have been made in medical education. The so-called assistant-surgeon of the present day is a fully and legally qualified *surgeon*. He has given abundant proof, by competitive and other examinations, of his professional knowledge, and if the passing of such examinations be tests of ability, he can produce more of these than the "surgeon." He is now no more an assistant to the surgeon, in reality, than the latter is to him; and for these reasons we consider the term "*assistant-surgeon*" most objectionable and injurious—objectionable because it implies inferiority of attainments, and injurious because it deprives him of rank and pay. The proportion of assistant-surgeons to surgeons in the army is nearly two to one, and every one conversant with military affairs knows that their duties and responsibilities bear a similar proportion. Why, then, it may be asked, should they not receive the title and pay of "*surgeons?*" At present the surgeon owes his rank, as such, to the mere accident of seniority. There are many instances of surgeons who served only six or seven years as assistants. Those who entered the army in 1849 or 1850, had most of them this good fortune. Is it not manifestly unfair, then, that the assistant-surgeon who entered in 1856 should have to serve perhaps twenty years before promotion?

Our idea is, that the rank and pay of the medical officer should, in all cases, correspond with his length of service, and not be dependent upon the accident of peace or war. All the medical officers not in the administrative ranks ought to be called *surgeons*, and their duties should be so defined by regulations as to prevent those unseemly clashings which are inseparable from the existing system. The competitive examination for admission into the service should be retained, and we would render it compulsory for each candidate to serve for two years at least, in one or more of the large military hospitals. Then, after having shown by another examination his acquaintance with the rules and regulations peculiar to the service, he should receive his commission of surgeon, with rank and pay according to the scale which we shall presently lay down, and he should be eligible for appointment to the charge of a detachment or regiment. The rank of surgeon-major should be abolished, because the distinction is perfectly useless.

At any time after 15 years' service on full pay, the surgeon should be at liberty to present himself for an examination on the following subjects, and he should not be eligible for promotion to the administrative ranks without having passed it. These subjects should be meteorology, physical geography, geology, zoology, botany, chemistry, physiology, pathology, dietetics, hygiene, arithmetic, algebra, logic, and, finally, the rules and regulations for the administration of the establishment both in peace and war. There should be only one grade in the inspecting rank, but no surgeon should be promoted to it until he had served at least twenty years.

The examination, which the assistant-surgeons now have to undergo previous to promotion, would thus be done away with, but for it there would be substituted one of a far higher and more useful kind. The duties of an inspecting medical officer, if rightly understood and properly executed, require the exercise of a different and, in some respects, more extensive knowledge, than that which is necessary for the regimental surgeon; and, therefore, no one should object to the application of means for ascertaining the possession of such knowledge. Very few, we imagine, would voluntarily resign all chance of promotion to the inspecting ranks. We might, therefore, reasonably expect that the majority of surgeons would endeavour to pass this examination, and, with this object in view, to take advantage of every opportunity for improving their professional or general knowledge. It would not be desirable to render this examination absolutely competitive, but the number of marks obtained by each individual should be publicly recorded. The highest number should not alone be sufficient to secure promotion, but, other claims and qualifications being equal, it should then carry the day. The advantages which this kind of examination would possess over that at present in use are so obvious and manifold that we shall not insist on them at greater length; and, as we have already exceeded the limits which we proposed to ourselves at starting, we shall conclude by subjoining the scale of pay and relative rank for the medical officers, which, we think, the circumstances of the service now require.

| | Under two years' service. | Under five years. | After five years. | After ten years. | After fifteen years. | After twenty years. | After twenty-five years. | On promotion. | After five years. | After ten years. |
|---|---|---|---|---|---|---|---|---|---|---|
| | s. d. | s. d. | s. d. | s. d. | s. d. | s. d. | s. d. | s. d. | s. d. | s. d. |
| Candidate ...... | 10 0 | | | | | | | | | |
| Surgeon......... | ... | 12 6 | 15 0 | 17 6 | 20 0 | 25 0 | 30 0 | | | |
| Med. Inspector | ... | ... | ... | ... | ... | ... | ... | 35 0 | 45 0 | 55 0 |
| Probable age... | 24 to 26 | 26 to 29 | 29 to 34 | 34 to 39 | 39 to 44 | 44 to 49 | 49 to 60 | 45 to 50 | 54 to 59 | 59 to 65 |
| Relative rank.. | Lieutenant. | Do. | Captain. | Do. | Major. | Do. | Do. | Colonel. | Brigadier-general. | Major-general. |

*Health in the Tropics; or Sanitary Art applied to Europeans in India.* By W. J. Moore, M.R.C.S., etc. London: Churchill: 1862. Pp. 318.

It is becoming more and more evident every day that the cheapest way to maintain an effective corps is to have a proper regard for the health of the men who constitute it at any given time, rather than to neglect them, simply because others can be had to fill their places when they are gone. One veteran is worth ten recruits, and the only way to obtain veterans is to be careful of the health of the troops, at all times and in every climate. Truisms, perhaps, these may be, but we are even now only on the very threshold of sanitary science in its relation to the military service. Sir James M'Grigor, to whose zeal we owe so much improvement in the hygienic condition of the army, was probably the first to impart that stimulus to medical officers in their duties touching the well-being of the troops, the happy effects of which are now so fully recognised. Sir Ranald Martin has been in a measure the pioneer of hygienic medicine in India, and to him the gratitude of our army resident within the tropics is justly due. Amongst other writers on kindred subjects may be mentioned, Balfour, Bettington, Carter, Chevas, Chevers, Coles, Cornish, Curtis, Day, Ewart, Geddes, Greenhow, Jeffrey, Lowndes, McAsh, Mackay, Morehead, Murray, Payne, Stovell, Walker, Waring, and others, all of whom have contributed to the welfare of troops in foreign, and especially Indian, stations.

Dr W. J. Moore, the author before us, is not unknown to the profession, having already published "A Manual of the Diseases of India,"—a work from which a fund of useful information may be elicited by those who, after a life within the tropics, look forward to a remnant of days in their native land. The present work consists of twenty-nine chapters devoted to the etiology, prophylaxis, and hygienic treatment of tropical diseases.. The first chapter is occupied with a brief review of the subject under consideration; in the second is an account of the causes of zymotic disease, followed by a chapter on *malaria.* The succeeding chapters are devoted to the history of sanitary science, the urgent necessity for its fuller development in tropical climates, and showing, moreover, that preventive means alone can materially lessen tropical mortality. In the ninth chapter Dr Moore speaks of the value of hill sanitaria. This is a question of vital importance, and one which has occupied the attention of most military officers. It is beset, however, with many difficulties; its advancement is not unfrequently checked by the obstinacy of ignorance, while, on the other hand, the zeal of its advocates often leads them to an abuse rather than a judicious use of hill stations. It was the remark of Dr Johnstone that "a blockhead can deny more in a single hour than one hundred doctors have proved in one hundred years,"—an apophthegm which the experience of medical officers will readily support. It is often a matter of deep grief to zealous officers to find their labours not only disregarded but treated with the utmost contempt by those who affect to despise sanitary precautions, and who are ever ready to bestow their censure in circumstances where a conflict of opinion leaves an open door for the entrance of a dangerous, and ofttimes suicidal, parsimony. But sanitary science is making headway in spite of hindrances, and the Indian soldier is gradually gaining a more healthful position, to which nothing can conduce more certainly than a well-conceived plan of operations on the part of medical officers. It is in their failures and conflicts—few though they be—that the strength of the opposition lies; and this has been remarkably observed with respect to hill sanitaria. Concerning the importance and probable increase of these health stations the author has the following remarks:—

> "Amongst all the numerous means which are required to be adopted to preserve the health of Europeans in India, there are none of such vast importance as the establishment of hill stations, or sanitaria. Now that it is evident that a larger European force must be retained in the country, the subject of the location of troops in hill climates is receiving more attention than heretofore. That this is the case is evident from the interest which the present Commander-in-Chief of India and the Commander-in-Chief of the Bombay army evince in the matter. That considerable progress has latterly been made in the establishment of sanitary stations, in various localities, is also shown in the

'Report of the Extent and Nature of the Sanitary Establishments for European Troops in the three Presidencies,' recently published by authority, where a more or less detailed account of some thirty of these stations is given. Much, however, yet remains to be effected; and I venture to prophecy that, as time develops the advantages of a hill climate, combined with a strict sanitary system; as railroads traverse the land; as civilisation advances; as the natives of India, becoming more aware of our resources, cease to imagine the *local* absence of European soldiery results from decline of power—that eventually the great majority of our soldiers will reside on the hills, and that it will come to be the exception, and not the rule, for a European regiment to be stationed on the plains."

Discredit, however, not unfrequently accrues to the hill stations through carelessness in discriminating between those cases in which an elevated locality is likely to prove beneficial, and those in which it will certainly prove prejudicial. There are very many diseases to which a change to the colder atmosphere of the hill sanitaria would be in any and all stages unfavourable; there are others in which, in certain stages, such change would be injudicious; and a third class to which such removal would be beneficial at one season but hurtful at another. Many of the disorders incident to Europeans in India are susceptible of benefit by a change from the interior to the coast, and Dr Moore follows his remarks upon hill climates with an appropriate chapter on marine sanitaria. Next follow some practical observations on the choice of stations in the plains, situations which—however undesirable in a sanitary point of view—can never be wholly abandoned. Chapters thirteen and fourteen are devoted to the consideration of barracks and hospitals, with especial reference to their hygienic points, and are followed by two chapters upon "conservancy" and "water." The seventeenth chapter is devoted to cholera, the eighteenth to "the soldiers' diet," the nineteenth to the effects of intemperance. Then follow two chapters on scurvy and syphilis, with an intervening one on prophylactic medicine. The twenty-third chapter treats of "dress;" the twenty-fourth of the employment of the spare hours of the European soldier in India; the twenty-fifth on camps and marching; the twenty-sixth on the despatch of troops by sea; the twenty-seventh on European colonization in India; the twenty-eighth on the condition of the Anglo-Indian soldier's wife, widow, and children; and, lastly, the twenty-ninth chapter is devoted to remarks on quarantine.

Such is a glance at Dr Moore's *Health in the Tropics*. It is obvious from the discursive character of the work that it would be out of place to detain the reader with critical remarks. The book is an epitome of useful knowledge on sanitary matters, rather than an original treatise. It is written in a good style, easily and agreeably. It contains a digest of the works of many esteemed writers on kindred subjects, and is interwoven with Dr Moore's own remarks and original conceptions, which impart a value to it such as pertains to no mere compilation. It is a work well worth reading, and one calculated to be of essential service to persons, whether in military or civil life, who have the prospect of a sojourn within the tropics.

*Ten Years of Operative Surgery in the Provinces.* By AUGUSTIN PRICHARD, F.R.C.S., Surgeon to the Bristol Royal Infirmary. London: T. Richards: 1862.

THIS work, of which the present volume is the first part, originally appeared as a series of articles in the British Medical Journal, under the head of "Provincial Operative Surgery."

The volume contains brief but complete records of 639 cases, on which Mr Prichard operated during a period of ten years. Most of the cases occurred in the Bristol Royal Infirmary; a small proportion in his private practice.

We might very naturally have expected that a record of 639 cases, contained in 174 pages, each of which contains less material than that upon which these words are printed, would have proved dry and uninteresting, but our author has condensed when it was possible, and expanded when necessary, with so much skill, and has infused through the whole so much personal interest, in reference both to the case and to the surgeon, that it is really an interesting record of difficulties surmounted and benefits conferred, on a scale of no ordinary magnitude. The sentence in which Mr Prichard justifies the publication of his cases is worth quoting for its truth and modesty:—

> "I think that those of us who are employed in a like occupation, will feel sufficient interest in this and the following pages, to justify my publishing them; and I am not without hope that they may be even useful; for every surgeon with a moderate amount of practice, whether among the poor or rich, must be the depository of some truths worked out by his own observations, or some curious results of his own experience, which, if they were published, would be new to most of us; and it cannot be doubted that a vast store of valuable medical knowledge lies concealed, especially in the remoter country districts, and that the greater part of it is lost to the world, and disappears at the same time as its possessors."

Most of the cases are very briefly recorded, but they are grouped with much skill; while each series is summed up and illustrated by remarks, always practical and sagacious, often original; and even the very shortest case conveys sufficient information as regards sex, age, treatment, and result, as to render it available for statistical purposes.

The first section contains 40 cases of operation on the face, tongue, palate, neck, and chest, excluding excisions, tumours, and aneurisms of these regions. The cases of epithelial cancer of the lip show, as usual, the very favourable results of operation, and those of cancer of the tongue the sad tendency to recurrence of the disease. From the experience of several cases of tracheotomy, Mr Prichard has made up his own mind that no instrument, however ingenious or complicated, is so useful as an ordinary bistoury, with a tenaculum or a sharp hook to fix the trachea.

The second section is devoted to operations on the abdomen and lower intestine. Twenty-five cases of operation for strangulated hernia are fully and ably reported; all of which confirm the general rule that every case in which the operation is required possesses some point of special individual interest. It is easy "to be wise afterwards," but we cannot help thinking, that in one or two of the unsuccessful cases the patient's chance would have been a better one had the operator not returned the bowel, but retained it at the external wound, having freed the point of constriction. The appearance of the bowel and the history in cases 53, 55, 56, would, we think, have fully warranted non-reduction.

In the operation for the radical cure of hernia, Mr Prichard prefers Wützer's method to the modifications either of Wood or Redfern Davies, and suggests an important little addition to the operation. "On the withdrawal of the wooden plug, to strip a thin slip of the integuments away from the orifice of the cutaneous canal, and put a pin through the parts, to get them to unite and close the aperture completely."

In the third section, we have a series of operations on the mamma. We notice the usual preponderance of cases of carcinoma, the safety of the operation, but the sad probability, almost certainty, of recurrence.

In the next, which contains the operations on the urinary and generative organs, we would especially notice the results of the various plans of treatment of hydrocele. The injection, after tapping, of the compound tincture of iodine, in the proportion of three parts of the tincture to five of water, effected a cure in every case in which it was tried except one, which eventually was opened and stuffed with lint, but which turned out to be not a true hydrocele, but a mixed case of hydrocele and hæmatocele. In two cases, the plan of introducing, on a probe, a grain of the nitric oxide of mercury into the emptied sac was tried: in one case it effected a cure, but in the other excited a very smart attack of inflammation, resulting in suppuration.

The last sentence in the chapter on hydrocele is, "The little I have seen of the mode of treatment by wires has not been encouraging."

In lithotomy the recorded experience is very small, yet it has been sufficient to impress Mr Prichard very strongly in favour of the median operation (Allarton's).

Without giving his reasons, he thus expresses himself,—

> "It is not likely that I shall ever perform the lateral operation again, except upon the dead subject, as a demonstration to the students, for I cannot imagine any circumstances that could induce me to return to it. My own few lateral operations have been much more speedily performed, and have looked mnch better than the others, and I met with an average share of success in them; but the real question to be considered is the patient's safety, and this is undoubtedly best observed by the median cut."

The fifth and concluding section in the volume includes operations on the eyes, eyelids, and appendages. A long list of operations for cataract by extraction gives the remarkably good proportion of 110 successful to 25 unsuccessful cases.

In the list of operations for artificial pupil, there is one very curious case which deserves notice.. The disease in the eye was of considerable duration: "the pupil was blocked up with lymph, the eye soft, and the iris green when I saw him. I made a corneal section, and some *brown aqueous humour ran down his cheek, and his iris, which had been green, at once became blue.* I succeeded in making a pupil, but his sight was no better. In most of these cases of discoloured iris in diseased eyes, I believe that the green hue depends upon this alteration in the humours, and not in the membrane itself."

Remarkable as this case is, we cannot accept this explanation as applicable in *most* or even in many cases of discoloured iris, but regard the discoloration as rather the result of increased vascularity and effusion of lymph into the substance or on the posterior surface of the iris.

We wish this little work all the success it deserves. If all "provincial surgeons" are like Mr Prichard, there are not many of her metropolitan rivals in whose presence provincial surgery need hide her head.

## *Works on Anatomy by Congelation.*

ANATOMY by congelation has been very appropriately named, by M. Legendre, Homalographic Anatomy. This word does not imply that the parts are prepared in any way before being anatomized; it only implies that the study is pursued, not by dissections, but by sections exhibiting plane surfaces. For securing accuracy M. Legendre congeals the parts before cutting them, this being the best method hitherto discovered.

The advance of medicine and surgery tends to increase the value of regional anatomy, and to demand from it more and more exactness. Hitherto the relations of the various structures in the limbs and in the bony and muscular parts of the trunk have been the chief themes of the regional anatomist. These parts have been abundantly often displayed in dissections and portrayed in elaborate pictures, as in the works of Tiedemann, Arnold, Lizars, Cloquet, Quain, Bourgery, Beau, etc., etc. But the display of relations in a dissection is, for the most part, really the exhibition of what is not in nature. Areolar tissue and fat and skin and membrane have been removed, and the exact relations of parts are no longer seen, they are displayed by art and then painted. Anatomy by congelation aims at getting rid of this source of error, and it undoubtedly offers the profession very valuable results.

The chief acquisitions of homalographic art are, however, in the field of medical anatomy. The works of Sibson and others show how acutely is felt the want of exact knowledge of the true natural position and relations of the viscera. That they have only imperfectly supplied the want, every one will admit who knows what anatomy by congelation has effected, or who reflects on the confusion always necessarily introduced into investigations of this subject, by opening the cavities of the body and admitting air into hermetically sealed sacs, and still more by any dissection of or interference with the contained viscera. We are indeed not sure that, in their dissections of the abdomen, the homalographic anatomists even have taken, in every case, sufficient care to congeal the whole trunk before making sections; it is evident that previous opening of the chest and, still more, of the abdomen, will lead to displacement of the whole viscera, and introduce to some extent the very error to avoid which congelation is practised.

Congelation is not the only plan used for securing accurate sections of the viscera in situ. Some time ago we called attention to the excellent work of Kohlrausch on the anatomy of the pelvis. In his preparations freezing was not resorted to. Kohlrausch hardened the soft parts by immersion in alcohol, and used other ingenious devices to ensure accuracy in his sections, and he was very successful. Jarjavay, in preparing dissections for his work on the male urethra, plunged the parts into diluted nitric acid; he thus hardened the soft parts and softened the hard parts, making them easily cut. But both these plans are easily seen to be much inferior to the method of freezing.

Already, in 1820, Serres used this plan in researches on the brain. MM. Manec, Geraldès, and Estevenet, subsequently applied it to other parts of the body. Henle, in his recent work on systematic anatomy, has availed himself of it in the study of the joints. Luschka has, by means of it, produced a valuable work on the anatomy of the chest. Legendre has published a general atlas of homalographic anatomy of great value, and of moderate size and price. But it has been reserved for the northern metropolis of Russia, to produce the grandest congenial congelated sections. The splendid work of M. Pirogoff is, we believe, now, at length, completed. It is large and expensive, but to the anatomist, the surgeon, the physician, the obstetrician, it is of the highest value. Its name is "Anatomia Topographica, sectionibus per corpus humanum congelatum triplici directione ductis, illustrata."

# Part Third.

# PERISCOPE.

## PRACTICE OF MEDICINE.

### CASE OF PATENCY OF THE FORAMEN OVALE WITH REFERENCE TO DIAGNOSIS.

BY DR REISCH, VIENNA.

It is generally believed that patency of the foramen ovale produces no disturbance of the circulation when the valvular apparatus of the heart is sound, and that no communication of the masses of blood takes place through this opening unless an impediment to the circulation has been occasioned by anomalies of the vascular trunks, or diseases of the valves and ostia. The direction of the current of blood may be either from left to right or from right to left, according to the form of abnormality of the heart. A case which confirms this opinion appears worthy of being communicated, because such cases are rare, and because it seems to explain a mode of origin of the venous pulse, which, so far as I know, has not yet been observed.

Katharina P., forty-nine years of age, was admitted into the Imperial General Hospital on the 11th March 1862. The patient stated, that in the beginning of the winter, 1861 (November), she had suffered from rheumatism; that since then she had suffered from pain in the chest, breathlessness, cough, and palpitations; that these symptoms had become very much aggravated during the last few weeks; and that cyanosis and dropsical swelling of the feet and abdomen had lately occurred. The patient said nothing of any previous illness: she stated, on the contrary, that she had suffered from neither cough, palpitation, nor cyanosis; and that she had been confined several times without any disturbance of her health. The patient was strongly built, pretty well nourished, and had a good muscular development; the countenance was exquisitely cyanotic; the veins of the neck were distended to twice their natural size, and in them a pulsation synchronous with the systole of the heart was perceptible. Examination of the chest afforded evidence of double pleuritic effusion, and of a high degree of bronchial catarrh. The cardiac dulness was extended both perpendicularly and horizontally; the impulse of the heart was clearly perceptible between the sixth and seventh ribs, to the outside of the line of the nipple. Auscultation disclosed a loud systolic bruit, which had its maximum intensity at the apex of the heart, and a weak, indistinct, second sound. Over the lowest part of the sternum no murmur was audible, but two weak sounds were heard. The second pulmonary sound was strongly accentuated. The aortic sounds were clear. The liver extended three finger-breadths below the border of the ribs. The belly was distended and fluctuating; the lower extremities were œdematous. Dyspnœa was intense; pulse above a hundred, very small. The urine contained no albumen.

From the above symptoms it was evident that the dropsical effusion was occasioned by an important disturbance of the circulation, and that the latter was determined by an insufficiency of the mitral valve. The cyanosis of the face was so intense, the swelling of the jugular veins so considerable, and regurgitation in them was so apparent, that there must necessarily be some other cause to account for these symptoms, in addition to the mitral insufficiency. Incompetence of the tricuspid valve, which would have afforded a satisfactory explanation, could not be made out, for, as above remarked, I had heard both cardiac sounds, unaccompanied by bruit, over the right ventricle. Pressure of the pericardial effusion upon the auricle did not appear a sufficient explanation; for, although it generally determines a systolic swelling of the veins, no regurgitation of blood is perceptible. An abnormal contraction of the auricle seemed to me an unjustifiable assumption, as in this case the regurgitation would not correspond with the period of the systole. Finally, the idea of congenital malformation of the heart occurred to me; but, as the patient had not till lately suffered from cyanosis, and as her bodily development was strong, I did not feel warranted in giving weight to the conjecture. Accordingly, the cause of the venous pulsation remained involved in considerable obscurity, until the death of the patient, which took place on the 20th of March.

On the 21st of March the post-mortem examination was performed by Dr Schott, assistant to Professor Rokitansky, and the following were the appearances found: The jugular veins on both sides were much distended, and contained dark fluid blood. The bronchial mucous membrane was reddened, and coated with bloody mucus. In the right pleural sac were six, and in the left, five pounds of clear yellow serum. The lungs had a coarse appearance, and the upper lobes, in particular, were rich in pigment. The lower lobes were dry and anæmic; in the left, the bronchi were dilated, their mucous membrane reddened, and they contained much tough mucus. In the pericardium was more than half-a-pound of clear serum; the visceral layer at various places, over both ventricles, but especially over the left, was covered in part with tendinous patches, in part with thin false membrane; the heart was enlarged, chiefly owing to eccentric hypertrophy of the right ventricle. The muscular substance of the left ventricle was pale brown, soft, and easily lacerable; that of the right firm, and of bacony appearance. The free borders of the mitral valve were thickened; the corresponding chordæ tendineæ were tolerably natural; the other valves and ostia, especially the tricuspid, were unaltered. The valve of the foramen ovale was imperfect, there being a crescentic opening which admitted the first joint of the little finger. The peritoneum contained several pounds of serum.

On the discovery of these lesions, the symptoms observed during life seemed capable of explanation in the following way:—The congenital insufficiency of the valve of the foramen ovale had given rise to no cyanosis previous to the rheumatic attack; accordingly, no communication between the blood in the auricles could have taken place; for, so long as the valves were healthy, the pressure in the two arteries was equal, so that the passage of blood from one side to the other was impossible. But when, as the result of endocarditis in the beginning of winter, incompetency of the mitral valve was established, an increased tension was exerted on the blood in the left auricle, whereby not only a congestion of the pulmonary circulation was occasioned, but simultaneously a quantity of blood was pressed into the right auricle. In this was laid the foundation of a high degree of cyanosis, and the bad effects of mitral insufficiency upon the circulation acquired an enormously increased potency. The passage of the blood from one side to the other must have taken place at the moment of the ventricular systole, for this was manifestly the time when the difference of tension in the auricles had reached its maximum. The blood flowing from the left auricle into the right must have presented an obstacle to the blood entering by the venæ cavæ, and have imparted to it an impulse in the centrifugal direction, and so given rise to the systolic venous impulse. In order to the production of a systolic venous impulse in such a case, a certain size of the abnormal opening is necessary, as well as a certain energy of the contractions of the heart; where the opening is very small, the quantity of blood which would pass through the opening would be too inconsiderable to give rise to this phenomenon.

From the above observation regarding the venous pulse, it follows, that this phenomenon is not alone sufficient to justify the diagnosis of tricuspid insufficiency, for it may appear under other circumstances, as in the case just narrated.—*Wochenblatt der Zeitschrift der K. K. Gesellschaft der Aertze in Wien*, 1862, No. 43.

## ON THE USE OF TONICS. BY PROFESSOR LAYCOCK.

TONICS are drugs which strengthen the body. Now, the body may be strengthened in very various modes. If weak from want of food or of fresh air, or from over-fatigue, then nutritive food, pure air, and rest are the most certain tonics. If the blood be defective in certain ingredients necessary to health, we add these. Thus, in convalescence from fever, cholera, and other diseases, in which the salts of the blood have been wasted, chloride of sodium, the phosphates, and the nitrates are *tonics.* Digestion may be imperfect, from imperfect enervation of the digestive organs, and thus chylification is hindered. Then defective blood results, and this reacts morbifically on the nerve-centres, intensifying the primary cause. Hence, tonics must often be compound in their nature, because intended to be multiform in their effects. When we give bitter ale or beer as a tonic, we give a very compound thing, and, *quâ* the bitters, a "tonic" not to be trifled with, from their action on the nerve-centres.

The cases treated in the Infirmary being, for the most part, asthenic and cachectic, give great scope for the observation of tonic remedies. Bitter drugs, chalybeates, "kitchen physic," and wine, have been freely used in the wards, either singly or in conjunction. A good diet ("kitchen physic") is often all that is needed in many of the cases we admit, and hence a source of fallacy when estimating the effects of drugs of this class. Of the bitters, we have chiefly used quassia, gentian, chamomile, quinine, and strychnine, and all these are, I conceive, nervine tonics,—that is, stimulate digestion, and give strength by primarily acting on the nerve-centres. Quinine, strychnine, hop, and wormwood are acknowledged nervines; and it is probable that other vegetable bitters, even such as aloes and colocynth, belong to the same category. Hence, the tonic value of aloes in non-purgative doses. These were rarely administered singly, as the blood was usually in a morbid state, as well as the nervous system; hence, we combined bitters with chalybeates and cod-liver oil. When this last cannot be taken, glycerine seems to be a useful substitute. A combination of quassia, sesquichloride of iron, and glycerine,[1] was found useful in various cachectic states, as anæmias, cardiac and renal dropsies, and the like. One man, who came in with all the pathognomic symptoms of the anæmia or asthenia known as "Addison's disease," quickly rallied under this combination, with a generous diet as an auxiliary, and went out in six weeks convalescent. At first he added half a stone to his weight in a week. Five years ago a similar case was under my care in the clinical wards, and, after taking good diet, wine, and chalybeates without benefit, recovered as rapidly as this man did under the same remedies. I do not pretend to say that in these cases there was structural disease of the supra-renal capsules; but most certainly there was that discoloration of the skin and mucous membrane of the mouth, that asthenia and those nervous symptoms, which Dr Addison and others have observed to be associated with such structural disease. That the melasma occurs without structural disease of the supra-renal capsules is certain; but this observation does not exclude functional disease of them as a cause, which may, in fact, pass away altogether, leaving the melasma as a permanent thing. A more conclusive objection is, that structural disease of the capsules is found without the accompanying melasma; but, then, in physic there are no single causes at work, and it is probable that a lesion of the blood and nervous system, especially of the skin, must be superadded to the capsular disease. In the absence of a satisfactory pathology, it would, perhaps, be better to take the leading symptoms as a guide, and term the disease a melasmic anæmia—an anæmia with a sort of melanosis, in fact, in which the epithelial cells of the skin and mucous membrane are the chief seats of the melanotic deposit.

*Strychnine* is a valuable nervine tonic of the bitter class, and may be said to be generally available in all cases in which there is defective enervation of the pneumogastric system. I always begin with $\frac{1}{40}$th of a grain as a dose thrice a-day. We have found it useful in cases of bronchitis in which the cough is more or less spasmodic, in phthisis with much bronchorrhœa, and in the convalescent stage of febrile pneumonia. The chief objections to its use are, the danger of an overdose from careless compounding or administering, and the rapidity with which its physiological effects are sometimes manifested. I am inclined to conclude, however, from frequent observation, that it does not stand alone in this respect, and that other "bitters" exercise an analogous and often as dangerous an influence on the nervous system. I have known bitter beer excite twitchings, and urgent head symptoms result from taking the "bitters" of the dram-shop; and I suspect the liqueur or "sirop" termed "absinthe" owes all its mischievous effects to the bitter drug it contains.

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[1] The formula used is as follows:—R. Tinct. ferri sesquichlor. ℥ij., glycyrrhinæ, infusi. quassiæ, āā ℥iv.; two tablespoonfuls thrice a-day.

Strychnine and other nervines were tried in two cases of diabetes. The valuable and sedulous observations of Mr Smart as to the progress of these cases, enable us to judge as to the effects of both drugs and diet on the production and elimination of sugar and water. The administration of strychnine, Mr Smart reports, in doses progressively increased until the physiological effects became incipiently apparent, was followed by progressive and commensurate decrease in the amount of urine and sugar. Chloroform. and the chloric and sulphuric ethers, increased it. I think nervines are most likely to be available in diabetes, for all the most recent researches into the pathology of glycosuria point to the nervous system as the primary seat of disease. In one of our cases the glycosuria was periodic, occurring every fortnight for about ten days, the interval of four days being free. This periodicity is, I suspect, less commonly observed than it occurs.

*Quinine* holds its place as a powerful nervine tonic. Perhaps it is in this way that it is so serviceable in cases of splenic disease, or of blood diseases consecutive thereto. In one case, in which hæmoptysis was associated with splenic disease, in a discharged soldier, returned from India, we found the blood-corpuscles, when examined under the microscope, to be highly crenated. This appearance is very decisive of splenic disease, and is readily altered, in some cases, by quinine. In the case alluded to, the hæmoptysis ceased, and the crenation of the blood-corpuscles disappeared, after the rapid administration of two-grain doses of quinine. Perhaps it is by its action on the nervous system that quinine facilitates the action of purgatives. You have seen how much the activity of the sulphate of magnesia is increased by combination with the drug; a scruple of the salt, combined with a grain of quinine, repeated a few times, at intervals of three or four hours, acts as an efficacious laxative. —*Medical Times and Gazette.*

## ON THE TREATMENT OF PLEURISY AND ENDOCARDITIS BY IODURATED FRICTIONS.

### BY PROFESSOR DELIOUX.

ALL those who have traced the progress and watched the tendencies of pleurisy know with what obstinacy the plastic exudations produced by the inflammatory process often persist. The pleuritic false membranes are not in general liable to a spontaneous and speedy resorption; by their mere presence they interfere with the normal functions of the organ in the interior of which they are formed, and secondarily with the functions of the lung which depend in part on the integrity of the pleura. By multiplying the surfaces of exhalation they are a frequent cause of those secondary effusions which complicate pleurisies which were originally dry; finally, they are converted into adhesions which at the end of a certain time are organized and become irremediable. It is, then, of the greatest importance to disembarrass the patients as soon as possible of intra-pleural false membranes; but their disappearance is not always readily brought about. Amongst the therapeutic means employed with this intention successive blisters present undoubted advantages, but when they fail they must be replaced by other remedies. M. Delioux has then recourse to topical applications of iodine, and they have been attended with a success which he thinks worthy of being recorded.

He employed in the first place tincture of iodine, but in the majority of cases it seemed insufficient. He then tried iodurated pomades, and they proved much more efficacious. In the circumstances under consideration, the "hydriodated pomade" of the pharmacopœia, which contains one-eighth of iodide of potassium, is not active enough; accordingly, in the first place, he orders the addition of fifteen grains to the ounce. If the desired effect is not produced, he increases the quantity both of iodine and of iodide of potassium; and the formula which he in general finds most useful is the following:—Iodine, half a drachm; iodide of potassium, two drachms; axunge, an ounce. This combination of iodine and iodide of potassium is very active; it speedily irritates the skin, and for this reason must be employed with some caution, especially in the case of those whose skin is susceptible and delicate; but it leads to the introduction of appreciable quantities of iodine into the system more certainly than any other topical application of iodine. In order, however, to obtain this desirable result, on which the efficacy of the treatment chiefly depends, two rules must be attended to in practising the frictions: 1st, To clean the skin perfectly by a preceding friction, in order to remove everything which could prevent the absorption of the drug; 2d, To rub strongly, during at least five minutes, in order, as it were, to force the door, somewhat difficult to open, of cutaneous absorption. The frictions ought to be made around all the surface corresponding to the affected portion, indeed exceeding its limits. Two frictions, one in the morning, another in the evening, suffice. Over the layer of the iodurated ointment, left after the friction, a sheet of cotton wadding is applied, and over this a piece of oiled silk; and the whole is kept in position by a bandage round the body. This mode of dressing has the double advantage of preventing the bedclothes and the patient's linen from being marked by the almost indelible stains of the iodine, as well as of favouring the absorption of the remedy. When the frictions sensibly irritate the skin, they are to be suspended during the time necessary for the subsidence of the irritation, and they are resumed as soon as possible, leaving as few intervals as may be in a treatment the success of which is much favoured by its continuousness.

M. Delioux has met with twenty cases where this mode of treatment has triumphed over intra-pleural exudations, some of which succeeded acute pleurisies, the others, in smaller numbers, having existed for a greater or less period of time, and constituting chronic exudative pleurisy. The treatment has lasted from fifteen days to two months; fifteen to twenty days have generally been sufficient to obtain the absorption of false membranes determined by an acute pleurisy. The most tedious case was one where the treatment extended over two months, where the patient had had an inflammation of both pleuræ, and where there was loud and extensive pleuritic friction, but which disappeared completely, leaving the membranes perfectly free. The action of the frictions was followed day by day by auscultating the patients. The diminution, and finally the complete disappearance of the rubbing sound, could be thus distinctly traced.

M. Delioux has applied the same treatment to some cases of pericarditis, but without obtaining any decisive results. He thinks, however, that pericardial false membranes, when they are not extensive, and are situated near the apex of the heart, may be efficaciously treated by the same means. He has, however, had every reason to be satisfied with the results of this treatment in the case of endocarditis following articular rheumatism. In two cases, among others, perseveringly treated by iodurated frictions over the pericardial region, the blowing murmur completely disappeared, and apparently there was resolution of the endocardial lesion which had given rise to it. This result is important when we consider the difficulty which is often found in effacing every trace of the secondary lesions occasioned by rheumatic endocarditis. In this case, no doubt, blisters are very useful, as well as in pleurisy; but when they do not succeed, M. Delioux recommends to have recourse to iodurated frictions, which appear to favour resolution.

M. Delioux endeavoured to determine whether the internal use of iodine in exudative pleurisy and endocarditis would increase the effect of the topical applications. It appeared to him, at least in the first of these conditions, that the internal administration of iodide of potassium was of no use; the resolution of the pleuritic false membranes took place with no greater rapidity than in the case of those submitted simply to the topical applications. It appears, then, that in certain circumstances medicines develop greater activity of action when they arrive directly at the affected organs by the local circulation. The topical employment of iodurated applications in the case of inflammations of the serous membranes situated near the periphery of the body, is a practical consequence of this peculiarity of absorption.—*Bulletin Général de Thérapeutique*, and *Gazette Médicale de Paris*, 17th January 1863.

### ON THE SUCCESSFUL USE OF IODIDE OF POTASSIUM IN THE TREATMENT OF ANEURISM. BY DR ROBERTS, MANCHESTER.

J. P., a collier, aged 39, unmarried, was admitted into the Royal Infirmary, on October 6th, 1862, complaining of pain in the chest, a troublesome cough, and difficulty of breathing at night. He stated that about four months ago, when in a state of intoxication, he was kicked in the chest by a policeman. From that time he has been subject to paroxysms of excessively severe pain, persisting for several hours at a time in the back, neck, and running up to the left side of the head and left shoulder. He compared the pain in the latter situation to having pieces of his flesh torn out. There has not been hæmoptysis at any time. About two months after the above occurrence, he observed an unnatural prominence of the upper part of the sternum.

When admitted, the patient had the appearance of good health; the pulse was 96, and the respirations were 22 per minute. The superficial veins of the upper arms and the external jugulars were somewhat distended; the right radial pulse was slightly feebler than the left. The speaking voice was unaltered, but the cough-voice possessed the peculiar stridulous character distinctive of interference with the function of the recurrent laryngeal nerves.

The pupil of the left eye was motionless from previous accident, the right pupil was natural. There was some dysphagia—when the chest was uncovered, the first bone of the sternum and its immediate vicinities were observed to be bulged out, and to be the seat of a heaving pulsation. On the left side of this prominence, in the second intercostal space, close to the sternum, there was a soft pulsating elevation, standing out about a quarter of an inch above the surrounding level. This elevation was conical in shape, and had a base about the size of a shilling. The heart's apex beat in the fifth interspace within the nipple line.

Percussion revealed a considerable area of diminished resonance over the bulging parts. Transversely, in the level of the second interspaces, the dulness measured four inches and a half, extending more to the left than the right of the middle line; vertically, there was dulness for three inches and a half below the sternal notch. There was no fulness or pulsation in the last-named spot. The cardiac sounds were normal over the precordial region; they were loudly audible over the seat of dulness above described.

The first sound was faintly murmurish over the soft elevation; and a distinct though faint systolic murmur existed in the course of the innominate artery; this was intensified over the right carotid and in the acromial angle. No murmur existed over the left carotid and subclavian. With such an array of symptoms and physical signs, the diagnosis could not remain for a moment doubtful. The case was one of aneurism of the arch of the aorta making its way forward through the parietes of the chest.

On October 10th, the patient was ordered to take five grains of iodide of potassium three times a-day; to keep his bed; and to restrain as much as possible the quantity of liquids taken in the way of drink. Three days later the iodide was increased to seven grains and a half three times a-day. Six days after the commencement of the treatment, the patient reported himself a great deal better; the paroxysms of pain no longer recurred; the cough and difficulty of breathing were less troublesome; and the soft pulsating elevation was slightly less prominent. The physical signs remained otherwise unchanged. The iodide was now increased to ten grains three times a-day.

From this date (October 16th) to October 27th (a period of eleven days) the patient went on favourably. He continued free from pain, difficulty of breathing, and dysphagia; and the soft elevation had almost altogether subsided to the level of the surrounding parts.

He was now allowed to get up, and the restrictions as to fluids were relaxed. The iodide was increased to fifteen grains three times a-day.

On November 4th (twenty-fifth day of treatment), the dulness measured four inches across, and two inches and three-quarters vertically. The dose was now raised to twenty grains.

On November 18th (thirty-ninth day of treatment), the area of dulness was carefully measured again. It had now contracted to three inches and three-quarters transversely, and two inches and a half vertically; so that from the commencement of the treatment, it had diminished three-quarters of an inch in one direction, and one inch in the other. The soft elevation had sunk to the level of the surrounding parts, and had become wholly inappreciable to the eye. Pulsation was still felt over the spot. The patient was free from pain and difficulty of breathing, and felt himself exceedingly well. The cough was still stridulous in character, and the veins of the neck and upper arms were not perceptibly less distended. The inequality of the radial pulses had not disappeared.—*British Medical Journal.*

# Part Fourth.

## MEDICAL NEWS.

## MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION XLII.—MEETING III.

*Wednesday, 7th January* 1863.—JAMES SPENCE, Esq., President of the Society, in the Chair.

### I. CURE OF GRANULAR CONJUNCTIVÆ BY INDUCTION OF GONORRHŒAL OPHTHALMIA.

*Mr Walker* exhibited a patient whom he had treated by a plan which he believed had not previously been followed in Edinburgh. The patient presented himself about the middle of last July, stating that he had been discharged from the army on account of the state of his eyes. He had served for four years, and was standing sentry one night at Natal when the simoom was blowing; the man attributed his subsequent sufferings to some sand having been blown into his eyes at that time. A day or two afterwards he went into hospital on account of an affection of the eyes. He was treated for some time with but little benefit, was then invalided, and sent home to England. On his arrival he was treated in a military hospital, from which, after some time, he was dismissed as incurable. On being examined by Mr Walker last July, the eyelids of the left eye were found highly vascular and granular; the cornea was so vascular that the pupil could only be perceived at one point, and the patient could barely recognise light. The right eye was much the worse of the two; the cornea was opaque nearly all over, and the patient had no perception of light. On everting the eyelid, a large mass of granulations was found. Mr Walker remarked that every practitioner knew how extremely intractable these granulations were; no means for curing them were known; they might be relieved for a time, but, after exposure to a bright light or to cold, they were almost sure to return. Accordingly, no hopes were entertained of effecting a cure by any ordinary means, and there appeared to be but one method which held out any prospect of success. When in London last year, Mr Walker had seen several patients treated by the method in question. It was not always, though often, successful, and either improved the eye or made it worse. This method consisted in inoculating the eye with gonorrhœal matter, thereby giving rise to an attack of gonorrhœal ophthalmia. This mode of treating granular lids had been proposed by Dr H. Walker many years ago; it had dropped out of use, but had been revived by Dr Piringer twelve years ago. It was again abandoned, but was reintroduced about two years ago by Dr Bader of Moorfields Hospital, and had since been employed by Bowman and Critchett. The cases of this kind which Mr Walker had seen had been pretty favourable, and accordingly he thought himself justified in having recourse to it in this instance. The patient was admitted about the middle of July, and soon afterwards a camel-hair pencil dipped in gonorrhœal matter from a female, was introduced between his eyelids. No effect was produced, and, on inquiry, there was reason to believe that the female was suffering, not from gonorrhœa, but from leucorrhœa. Matter was next procured from a man with gonorrhœa, for which he had undergone no treatment. A brush dipped in the discharge was introduced between the eyelids of the patient. The ordinary symptoms of ophthalmia manifested themselves in twelve hours, and the disease ran its course in the usual way. There was pain, swelling of the eyelids, and discharge; but the inflammation kept quite within bounds, and there was never any apprehension of sloughing of the corneæ. No treatment was required during the acute stage: nothing but cleanliness was enforced. Afterwards, some simple astringents and ointments were employed. The patient left the hospital about the middle of September, and since that time has been able to go about alone. At present the cornea of the left eye was comparatively clear, and it was gradually becoming clearer. The right cornea was also clearing at one part; so that, if considered advisable, a good artificial pupil could be made. Probably, however, this would not be necessary, as farther improvement might be anticipated.

### II. SUICIDAL WOUND OF THE THROAT.

*Dr J. D. Gillespie* showed the larynx and trachea of a patient who had died in consequence of a wound she had inflicted on her throat. The subject of the observation was a woman, sixty-eight years of age; she had never shown any symptoms of insanity, was in comfortable circumstances, and did not appear to have been actuated by any assignable cause in perpetrating self-destruction. She had gone direct from a prayer-meeting to her bed-room, and there inflicted the injury with a table-knife. The preparation was an illustration of the fact that would-be suicides rarely succeeded in directly making away with themselves. Though the wound was between four and five inches long, and extended nearly from ear to ear, death was not caused by the injury of any important part. There was not much bleeding, and death resulted from a secondary inflammatory affection, acute pleuro-pneumonia. The wound was inflicted between the hyoid bone and the thyroid cartilage, had shaved off a portion of the epiglottis, and on each side was very close to the external carotid artery. There had been no arterial hæmorrhage; what bleeding there was had proceeded from the veins. The patient did well till the inflammation of the lungs set in, which carried her off on the sixth day. From the position of the wound there was much gaping; the hyoid bone was detached from the thyroid cartilage, so that it followed the movements of the tongue. About half of the food came out by the wound: still the patient got enough of nourishment, and the use of the tube was not necessary.

### III. ORIGIN OF THE RENAL ARTERIES FROM THE MIDDLE SACRAL.

*Dr John Struthers* exhibited a preparation in which a renal artery arose from the arteria sacra media. High renal arteries came off in the usual situation, one on the right and two on the left side; the upper of the two latter small in size. There was a low supplementary renal on each side. The left arose from the commencement of the left common iliac. The right arose from the middle sacral, after a short course of that vessel, and passed forwards and then across the front of the right common iliac artery. Each passed behind the ureter to enter the lower end of the corresponding kidney. Besides the upper renal veins, which had the usual arrangement, there was a low supplementary vein on each side, the right ending in the vena cava, the left terminating in the left common iliac vein after passing behind the left common iliac artery.

Dr Struthers mentioned that he had met with another case, in a subject now in the anatomical rooms, in which a renal artery arose from the middle sacral. On the right side the renal artery and vein were normal. On the left side the kidney lay chiefly in the iliac fossa, and was supplied by two arteries of nearly equal size, one from the aorta, just at the origin of the left common iliac, the other from the middle sacral after a short course. It was twice the diameter of the continuation of the middle sacral, turned over to the left in front of the left common iliac artery, and passed behind the ureter to reach the kidney. The upper of the two arteries of the left kidney divided into two; one entered at the upper part of the hilum, and also gave branches into the kidney above the hilum; the other wound round the posterior border, lay in a fissure across the outer surface, and broke up into branches which pierced the outer surface and anterior border of the kidney. There was only one left renal vein, and it terminated in the left common iliac vein after crossing in front of the left common iliac artery.

Dr Struthers remarked, that although cases had been recorded of supplementary renal arteries arising from the iliacs, he had never before met with or heard of an instance of a renal artery from the middle sacral. The nature of the arteria sacra media, as the true aorta, in contrast with the iliacs, which, notwithstanding their size in man, were of the nature of branches, explained the fact that in both the cases the renal artery furnished by the middle sacral, instead of passing more directly behind, advanced and passed across in front of the common iliac, just as a supplementary or normal renal from the aorta was in front of a lumbar artery. In both of these cases also, the pelvis of the ureter was seen to come out at the hilum behind the upper renal bloodvessels and in front of the lower, as was usually the case when there were two or more renal arteries at considerable distances from each other.

### IV. ON THE PROTRUSION OF THE EYEBALLS TERMED ANÆMIC.

*Dr Laycock* read a paper on this subject, which will be found at page 681 of this number of the Journal.

*Dr Seller* remarked that much of the matter contained in Dr Laycock's paper was still so new, that members were scarcely in a position to enter at once upon the discussion of it: the Society, however, was not the less indebted to the author for having directed attention to so interesting a subject. He (Dr Seller) well remembered the interest which had been excited when Dr Begbie's paper was originally read, and the Society was fortunate in having in Dr Laycock so able a continuator. There could be no doubt as to the importance of the subject; but it was only since the recent investigations into the functions of the nervous system that its consideration could be taken up on any other than speculative grounds: actual experiment must now, however, form the basis for the establishment of any theory regarding it. How far science enabled us to arrive at the diagnosis and discrimination of the numerous singular affections which had been alluded to, Dr Seller was not as yet prepared to say; but there could be no doubt that Dr Laycock, from his knowledge of the history of inquiries into the nervous system, and from his familiarity with the most recent investigations, was thoroughly qualified to give an example of the mode in which such an inquiry should be conducted.

*Mr Benjamin Bell* agreed with Dr Seller in thinking that the subject was too wide for discussion: there were, however, one or two points which Dr Laycock had brought out very clearly, to which he might allude. He (Mr Bell) had always felt that in cases of protrusion of the eyeballs there must be something more than, or distinct from anæmia; for we every now and then met with strongly marked cases of anæmia without exophthalmos, and, on the other hand, with cases of exophthalmos without anæmia. He had made this remark more than once to Dr Begbie, who, he thought, had seemed to admit that there might be some room for doubt. Again, we have cases of goitrous enlargement of the thyroid gland, chronic and acute, without exophthalmos. There must, therefore, be some more general explanation, some principle to direct us to the true nature and the proper treatment of the disease. It was well known that in some cases iron and tonics did good, while in others they had no effect. In a case lately under observation, tonic treatment did very little good, while change of air had a remarkable effect; when, however, the patient came back, the symptoms returned. Dr Laycock, in Mr Bell's opinion, had indicated the real source of the disease as being connected with an abnormal condition of the nervous system. A point worthy of remark was that he (Mr Bell) could not recall a case except one where vision had been affected. If, as many practitioners supposed, there was any real enlargement or distention, leading to thinning of the sclerotic, we should surely find vision more frequently interfered with. In the exceptional case the patient suffered from exophthalmos and enlarged thyroid, but for many months without any affection of sight. He recovered from the exophthalmos, and remained well for a considerable time: the protrusion, however, returned, and then his vision became impaired, so that he could not see his friends in a room. On examination with the ophthalmoscope, the only abnormality which could be detected was some loss of transparency of the retina. Under little treatment vision was restored; but some protrusion of the eyeballs still remained, and every now and then the pulse was affected, becoming rapid, and presenting a peculiar thrill.

*Mr Walker* had not seen a great many of the cases under consideration; in none which had come under his observation had there been any appearance of disease of the eye; the coats were not thinned, there was no effusion into the globe, and the media were transparent; he did not, therefore, believe that there was any change in the form or structure of the eye. He believed that exophthalmos was often caused by an increased quantity of blood in the deep orbital veins, which pushed the eye forwards, and that, when vision was affected, it was due to stretching of the optic nerve; farther, that the venous congestion was due to spasmodic pressure of the muscles of the neck on the jugular veins, which partially prevented the return of blood. In two cases of the kind, where a post-mortem examination had been made, the jugular veins were found increased in size, without any circumstance to account for their enlargement; so that he (Mr Walker) was rather inclined to think with Marshall Hall, that the protrusion of the eyelids was due to pressure on their veins, exerted by the muscles of the neck. Such was the view Mr Walker held at present, though, no doubt, Dr Laycock's paper pointed out some other causes of the affection, an inquiry into which might be followed out with much benefit.

*Mr Benjamin Bell* remarked, with reference to the post-mortem examination in such cases, that after death all trace of protrusion was found to have disappeared. This, of course, gave the quietus to all such causes as an increased quantity of fat in the orbits.

*Dr Alex. Wood* had formerly paid considerable attention to this subject. Since the introduction of the microscope into medical inquiries there had, perhaps, been too great a tendency to look out for material changes to account for disease, and therefore it was important to have our attention called, from time to time, to subjects where this was not so much the case. Dr Wood was scarcely prepared to accept all Dr Laycock's conclusions. He did not think there was much force in the argument used by Dr Laycock when he said that many cases of anæmia occurred when no exophthalmos was present. We knew that many pathological changes manifested themselves, sometimes in one way, sometimes in another. Thus, in chlorosis, anasarca was often present; but though it was often absent, we were not on that account justified in saying that the two conditions were not associated. The result of the paper was, in Dr Wood's opinion, to throw us back upon the question whether the original departure from health was connected with the nervous or with the vascular system. It was not probable that the exact connexion existing between these systems would ever be entirely cleared up. We knew the close relation subsisting between them, and it was exceedingly difficult to say in which the departure from health commenced, and which we should look upon as the point from which we should treat disease. Dr Wood must confess that he was still inclined to accept the demonstration given by Dr Begbie as to the connexion of protrusion of the eyeball with enlargement of the thyroid gland, and an anæmic condition of the system generally.

*Dr Argyll Robertson* observed that the question before the Society was so wide that he could allude to only one or two points. Dr Laycock had expressed the opinion that exophthalmos always occurred symmetrically, but Dr Praël of Brunswick had narrated nine cases of protrusion of the eyeballs connected with an anæmic condition of the system, and in three the affection was solely confined to the right side. The explanation given by Dr Praël was, that in consequence of over-action of the heart, the blood was forced more readily up the right than up the left carotid artery, the former being nearer to the heart. In only one of the cases which came to a post-mortem examination was extensive valvular and interstitial disease of the heart found (and on this case Dr Praël's theory was founded); in all the others no cardiac lesion was met with. It was important, however, to notice that in three out of the nine cases the protrusion was unilateral, although the lesion was apparently due to constitutional causes, and the other ordinary symptoms were present, such as bronchocele and excessive action of the heart. In all cases the motions of the eyes were perfect, and in the majority of cases, under the use of iron and a nutritive diet, the symptoms disappeared. Dr Laycock had directed attention to the possibility of determining whether the cause of the protrusion depended upon a spinal neurosis, or upon disease of the fifth nerve or the trigeminal ganglion, by observing whether there was ulceration of the cornea induced; but in many of the cases dependent upon anæmia, ulceration of the cornea showed itself in the later stages, in consequence, as Von Graefe had shown, of exposure of the eye to air, dust, etc., due to inefficient closure of the eyelids. That this might depend upon anæmia and not necessarily on disease of the fifth pair, was farther shown by the post-mortem examination of such a case, in which no lesion of the fifth pair was found after death, and in which no other symptom of its affection had been present during life. With regard to the cause of the protrusion, Dr Robertson thought Von Graefe's explanation the most probable; that there was congestion of the venous system, and that, as the result of this, exudation took place behind the globe of the eye. That the protrusion was in part due to congestion was also shown by the circumstance that the eyeball could be partly replaced by pressure. If Dr Laycock's theory was correct, the pupil should generally be affected, but in the cases Dr Robertson had seen this had not been the case, its appearance was normal, and it acted naturally under the stimulus of light; and, so far as he had read, no affection of the pupil had been noted.

*Dr Laycock* had but a few remarks to make, by way of explanation. As to the theory that venous congestion caused protrusion of the eyeballs, it was disposed of by one or two considerations. In the first place, when the orbit was examined after death, no traces of venous congestion were found; all the mechanical theories were disposed of in the same way; the supposed thing was not discovered, whether it was venous dilatation, serous effusion, or accumulation of fat. In the second place, when we looked at a well-marked case of nervous protrusion, we observed that on the slightest emotion the eye was suddenly protruded. Three classes of cases were to be distinguished—cases depending on an affection of the spinal cord, on the sympathetic system, and on the fifth nerve. Important conclusions could often be arrived at by carefully observing the condition of the eye. When the protrusion depended upon a simultaneous affection of the spinal cord and the sympathetic nerve no inflammation occurred. Indeed it had been mentioned by Dr Begbie that the protruded eyes were singularly exempt from inflammation. This circumstance was readily accounted for, because when the sympathetic was divided there was an increased flow of blood through the vessels of the part, with increased evolution of heat, and increased resistance to the causes of inflammation; when the trigeminus was affected, without any involvement of the sympathetic, it was different; here there was exposure of the eyeball without any increased power of resistance, and hence inflammation. Dr Laycock, therefore, regarded this as a very sound point of diagnosis. If the spinal cord alone were affected there might be no inflammation; but in old cases of this kind a species of diathetic degeneration was apt to occur in the form of atheroma of the aorta, of the thyroid or ophthalmic artery, or of disease of the heart. In reference to Dr Wood's allusions to chlorosis, Dr Laycock might observe that there was a class of chlorotic cases in which we found the eyes a little enlarged and slightly protuberant. This was probably the early stage of the condition under consideration, for in such cases we often found, on examination, that there was a slight degree of bronchocele and increased vascular action in the carotids. These symptoms were liable to occur when there was already a tendency to dropsy, and after their occurrence the dropsy was apt to become much worse. There was a class of cases in which surgeons had operated for aneurism within the orbit when none existed. These were probably cases of increased vascular activity of the ophthalmic artery of the same nature as in those to which the attention of the Society had been more particularly directed to-night. Dr Laycock would take an early opportunity of bringing before the Society the conclusions at which he had arrived regarding the effects of the nervous system on the bloodvessels and the blood.

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## PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

### SESSION XXI.—MEETING XIV.

*August* 6, 1862.—Dr Graham Weir in the Chair.

### I. CASE OF TURNING.

*Professor Simpson* showed a preparation of a child which he had delivered by turning in the Lying-in Hospital, where fracture of the fourth cervical vertebra had occurred during the operation, leading, as was discovered on post-mortem examination, to extravasation of blood at the side, and pressure on the phrenic nerve.

### II. PREDETERMINED PREMATURE LABOUR.

*Dr Keiller* read a paper "On the New Method of Inducing Premature Labour at a Predetermined Hour." Dr K. explained the reason of his communication appearing in the billet as a quotation,—it being that of the title given to a paper recently published in the Edinburgh Medical Journal by Dr Robert Barnes of London, whose name had of late been prominently brought forward in connexion with the use of the caoutchouc bag in the induction of premature labour.

In commenting on Dr Barnes' paper on "the new method," he (Dr Keiller) reminded the Society of what he had communicated in regard to the various methods of inducing and promoting labour, and referred to the question of priority respecting the application and recommendation of the caoutchouc bag as an obstetric instrument. Dr K. repeated what he had at former meetings stated on this subject, and after again citing the particulars of the first case (Mrs Field), in which he induced labour by means of the caoutchouc dilator, and referring to numerous other cases in which he had found the instrument to be useful, concluded by leaving the matter in the hands of the Society in which he had several years ago first introduced the subject. He (Dr Keiller) had been blamed by many of his obstetric brethren and pupils for being so remiss in noticing the pretensions of others who had published on the subject and claimed a priority to which they were not fairly entitled.

*Dr Graham Weir* remarked that he could corroborate the statement of Dr Keiller with regard to the case of Mrs Field. Dr W. remembered of having had a conversation with Dr Keiller on the case; and, as Dr Keiller had stated to the Society, he (Dr Weir) pointed out the description of an apparatus noticed in Rankine's Abstract, a modification of which might prove useful for dilating the os uteri; which idea was then and has since been successfully carried out by Dr Keiller, who, as is well known to the members, is entitled to the merit of being the first to introduce into obstetric practice an instrument admirably calculated to dilate the os, and in many cases to expedite delivery.

*Dr Simpson* stated that he always thought and always said, in his lectures and otherwise, that the introduction of caoutchouc bags as dilators in obstetric practice belonged to Dr Keiller, who was the first to show him (Dr S.) the instrument used on the occasion referred to by Dr Weir. This occurring several years ago, there was no dubiety in the matter. Indeed, as might be remembered by those present, he (Dr S.) when lecturing at the College of Physicians on the Chief Modern Advances in Practical Midwifery, took occasion to make special mention of the introduction by Dr Keiller of the caoutchouc bag into obstetric practice, and he (Dr S.) considered it only as a simple act of duty on the part of the Society to associate Dr Keiller's name with caoutchouc dilators. He (Dr S.) believed that all the members present were fully convinced, both from previous information as well as from the statement which Dr Keiller had just made, of the priority of Dr Keiller's claims in the matter.

The other members present heartily concurred in the observations made by Professor Simpson.

(*Dr Keiller's paper will appear in the next Number of the Journal.*)

### III. FIBROID TUMOUR OF THE UTERUS, WITH PREPARATION.

The Secretary read notes of a case by *Dr Bramwell*, Perth:—" 7*th March* 1862.—I was called, about four months ago, to see a Mrs F., the mother of a large family, who was threatening to miscarry in her fourth month of pregnancy. She was a delicate woman of about thirty-seven years of age, whose general health had been much impaired by a combination of causes. Since October last, she had suffered from constant vaginal discharge, one week colourless, the next sanguineous. In addition to this, her menstrual discharges had been excessive in quantity, so much so as to compel her on some occasions to keep her bed for several days together. There was also much pain at those periods. At the New Year, she had a violent flooding, which led her to suppose she had parted with an embryo child. Subsequent events show, however, that this could not have been the case, as, the month after, her menstrual catamenia disappeared altogether, and did not return up to the period already specified. During this interval, however, she still continued to suffer from leucorrhœal discharge, and much uneasiness in the pelvic region and down her left thigh.

" Her general health was very indifferent. She was low-spirited, nervous, prone to stagger on the street, and not unfrequently to cling to the house walls for support; in fine, it was clear that there was something sapping her strength, and engendering anæmia with all its painful concomitants.

" I was told she had lost a considerable quantity of blood the day I was called. On proceeding to examine the uterus, I found its os slightly dilated, but could at that time detect nothing abnormal. As nothing could yet be done towards the removal of the ovum, I temporized till night, and again examined her. I now found the os uteri largely dilated, and through it there protruded a tough resisting substance, which, on tracing up to the upper third of the uterus, I found to be a sessile polypus, about the size of a small hen's egg. Around the insertion of this body was wrapped the ovum, which I removed, and immediately afterwards the hæmorrhage ceased. Two days after, having consulted with Dr Frew, we determined to remove the growth without delay. The parts being dilated, and uterine involution not having yet set in, facilities existed for the operation which would have been lost had we procrastinated.

" Chloroform was administered; the apex of the polypus was seized and drawn down with the finger and thumb of the left hand. The index finger of the right hand, armed with a short cutting hook, was pushed up to the root of the polypus; it was divided with the hook and finger, and then removed. The microscope showed the growth to be composed of pure fibrous structure. There was but little hæmorrhage either during or after the operation, but the vagina was plugged with sponge, as a precautionary measure. In fourteen hours this was removed, no hæmorrhage having appeared. Two days subsequent to the operation, she had a shivering fit, followed by slight febrile symptoms and considerable abdominal tenderness. Emollient poultices of bran were applied, Dover's powder administered, and the vagina washed out with tepid milk and water. A speedy amendment followed the use of the means mentioned. After the lochial discharge ceased, there appeared a leucorrhœal discharge in its place, which lasted about a fortnight. It is now, however, all but gone, and her health steadily improving under the use of wine, steel, and generous diet.

"*9th July.*—My patient is now considerably improved in health. She is able to be out every day, has gained flesh, and feels much stronger. She has menstruated three times since the operation; the first period was marked by a tendency to menorrhagia, for which she was confined to bed for a few days, and used secale and gallic acid; the second and third periods were quite natural, there being no unusual pain nor any tendency to excessive uterine discharge. On making a tactile examination of the uterus, I found the os slightly dilated, but nothing abnormal. There is still a tendency to leucorrhœa, which has come and gone. For this I have ordered her the sulphate of iron internally, and cold sitz-bath, with alum, every morning. Any lengthened comment on this case would be superfluous; suffice it to say, that it is one more proof of the superiority of excision to any other mode of removing uterine polypi.

"To Professor Simpson we are under a deep debt of gratitude for establishing the practicability, safety, and superiority of this method of operation; and I doubt not that excision is destined to supersede the use of the ligature, and that at no distant period of time."

### IV. MENSTRUATION IN SIAM.

*Dr Sinclair* read notes "On the Age at which Menstruation begins in Siam, by *James Campbell*, Esq., Surgeon, R.N., of H.B.M. Consulate, Bangkok, Siam," published in this Journal for September 1862.

### V. DISLOCATION OF THE ENSIFORM CARTILAGE DURING PREGNANCY.

*Dr Simpson* read the following letter, by *Dr Sim* of Naples:—"*13th February* 1862.—My dear Sir,—I must apologise to you for allowing so much time to elapse between the receipt of your answer to my last letter and this date. I postponed writing again, till I received from my friend, Dr Alexander E. Mackay, who saw the case with me, his testimony to the correctness of my observation of the eversion of the ensiform cartilage by the gravid womb. When I wrote to you to ask if such an occurrence were rare, I was prepared to hear from you that it was so, but not to learn that my case was, perhaps, unique.

"I shall watch with interest for reports of similar cases; for, notwithstanding even your authority that this case is the first recorded, I cannot easily believe that such cases have not previously occurred. I should not have noticed the present case but for the intelligence of the lady in whose pregnancy it happened. However this may be, I vouch for the perfect correctness of the fact above stated; and I am happy in having my observation corroborated by such a distinguished member of our profession as Dr A. E. Mackay. According to your request, I send you the following brief particulars of the case. In the first place, I may state that the lady in whose case the eversion took place is rather under middle height, of a symmetrical form, and in the prime of life.

"About the end of the sixth month of her recent pregnancy, which was her fifth, she began to complain of great pain in the right side; but, as Dr Mackay observes, it would be difficult to ascribe this pain to the eversion of the ensiform cartilage. There was pain at the place where it was turned over, but there was also great pain lower down, in the right hypochondriac region, the cause of which I must leave to you to explain, as I know of no satisfactory reason given for the existence of these very violent pains, sometimes so distressing, during the last months of pregnancy.

"The patient was delivered of twins in the month of October last, and my attention was drawn by her to a 'lump' under the right mamma, a few days after the confinement. I must tell you that this lady lives in constant dread of cancer of the breast, and that, when she drew my attention to the swelling, she evidently supposed it to be a scirrhous tumour. For two or three days I was afraid that her fears were too justly entertained; but, after observing the tumour for a few days longer, I was gratified to find that it was gradually diminishing in its circumference, and that the hard central part was also changing its form and position. In short, I soon discovered that the hard central part of the swelling, which remained after the disappearance of the surrounding tumefaction, was simply the ensiform cartilage, which had been gradually everted, pressed upwards, and turned over to the right side by the encroachment of the gravid uterus. On the uterine pressure being removed, the cartilage gradually rose from its abnormal position. At first I could hardly insert my forefinger between it and the cartilages of the ribs on which it lay. After some time it was at right angles to the sternum, and thenceforth its progress to its own place was more rapid. At the end of the sixth week from parturition, the cartilage had resumed its proper position.

"There was nothing unusual in the labour. Its duration was only about three hours. The presentations were, first feet, next head. The liquor amnii was not unusually abundant, nor was there anything remarkable about the size of the placentæ. The twins, together, weighed twelve and a half pounds avoirdupois. The former births were all single.

"The length of the sternum is seven inches, that of the ensiform cartilage rather more than two. The distance from the sternum to the pubis I have not ascertained, nor other abdominal measurements.—I am, my dear Sir, very faithfully yours, ROBERT SIM."

### VI. NEW OXYTOCIC!

*Dr Alex. R. Simpson* begged to bring under the notice of the Society a specimen of an oxytocic powder, which he doubted not would be new to most of the Fellows. It was nothing more or less than a small quantity of dust from the tomb of Rachel at Bethlehem, which was held in much esteem by the Jews, and was employed by them for soothing the pains and expediting the process of parturition. The small sample now exhibited had been offered, by an aged Jew, in exchange for a blanket!

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## SESSION XXII.—MEETING I.

*November* 26, 1862.—Dr PATTISON, *President*, in the Chair.

### I. CASES OF FIBROID TUMOURS OF THE UTERUS, WITH PREPARATION.

*Professor Simpson* exhibited a uterus containing a large number of fibroid tumours of various sizes, in different situations, and in various stages of degeneration and development. Some were as small as peas, one was as large as the fœtal head, and there were others of every intervening size. Some lay immediately beneath the peritoneum, others were embedded in the uterine walls, whilst a third set projected into the cavity of the uterus, invested with a more or less complete covering of mucous membrane. The largest of the tumours projected forward in this manner the mucous membrane, and its surface was considerably ulcerated. On being cut into, it was found to be very much softened and friable, and of a pinkish hue, from an increase in its vascularity. It had evidently been the seat of an inflammatory attack, which had led to this alteration in its texture, and which, had the patient survived, would probably have ended in the destruction and separation of the morbid mass. Most of the other nodules and nodular masses presented the ordinary characters of fibroid tumours still vital and progressive; but one of them—a sub-peritoneal one of medium size—was firm and gristly, and at one part of its circumference contained a thin calcareous or osseous layer. The patient had been the subject of many attacks of excessive menorrhagia, which he (Prof. S.) had repeatedly checked by applying perchloride of iron to the mucous lining of the organ. Under the use of bromide of potass, etc., the patient seemed to improve in health, and thought, as he himself believed, that the mass had shrunk somewhat. She had returned to him, however, some months ago, suffering from pain and a recurrence of the menorrhagia. Inflammation set in in the peritoneum, probably spreading from the already inflamed tumour, and the patient sank.

He (Professor Simpson) had had in his ward last session a patient with an enormous fibroid tumour, which he had attempted to remove, partially by means of the écraseur. The chain had given way, but the strangulated part sloughed; the mass contracted considerably; and the patient,—who had been reduced to the last stage of weakness before, so that for a time her life was almost despaired of,—was now going about in the enjoyment of tolerably good health. Where such tumours were not so large he had sometimes removed it by enucleation, but the operation was usually accompanied with much danger, and was never perhaps justified except when the hæmorrhage was so great as to threaten life. Lately he saw a lady from Ireland, who for some years suffered from excessive menorrhagia, and from whom he removed a fibroid tumour of the size of a swan's egg, which was projecting in the form of a sessile polypus from the inner surface of the uterus. The patient was in a low and critical state for a few days after the operation, but she was now the picture of health.

## II. INFLUENCE OF THE IMAGINATION ON PARTURITION—MODERN AGENCY OF WITCHCRAFT.

*Dr R. P. Ritchie* read a paper on the above subject, by *Dr E. Dickson*, physician to the British Embassy at Constantinople:—

Belief in witchcraft is still prevalent among Arabs, and other Eastern nations. The following instance both illustrates the above notion, and, moreover, shows the power the imagination has in controlling or modifying the efforts of nature in cases of pregnancy.

A handsome and wealthy young Moor of Tripoli had been betrothed to a young lady of good family; but having, at the same time, a mistress of whom he was very fond, he kept putting off, *sine die*, his marriage with the young lady.

Made aware of the cause of this delay, and anxious to possess for a husband so desirable a person, the young lady resorted to a practice common in her country, and applied to a sorceress to cause the destruction of the female who stood in the way of her happiness. In the meanwhile the young man's mistress became *enceinte*, and was under perpetual dread of the sorceries practised upon her by her rival. Her mind was continually agitated by all sorts of reports, and she was at last told that the child she bore in her womb would never come into the world.

The more these rumours became current, and the more her rival endeavoured, through the agency of wizards and witches, to destroy her, the more attached and attentive did her lord become to her; at last the awful moment arrived, labour pains came on, and the best midwives of the town were called in to assist. The woman was a fine, strong, well-formed young person, and yet for the space of eight days did these midwives exert every means in their power, and administer all the resources of their art, to no effect. They were called in one after the other, and they all left the woman in the same state in which they had found her. All declared that the child was bewitched, was glued to its mother's womb, and could never be born. At last one of them, more humane and intelligent than the rest, stated that the only chance the poor woman had of being delivered was by sending for me. This was immediately done. I found her much exhausted, but composed in mind; the child's arm protruded; all the soft parts retracted to their normal condition, and more or less rigid. Labour pains had entirely ceased, and the child was dead. I informed the young man that I would bring away the child by instrumental aid; but that I could not answer for the woman's life, as that would depend upon many circumstances, some of which were beyond my control; that before undertaking the case he must promise to see that all my injunctions were strictly carried out; and that every person be sent out of the house excepting one assistant; and that he was to remain himself, to attend, if necessary. With the aid of a warm hip-bath, and of my instruments, I soon brought the child away, without inflicting the slightest injury to its mother. It was a large well-formed boy, with the body and limbs completely covered with dark hair, which was especially thick upon the back parts. The extraction of the child brought on labour pains. The placenta came away as usual, and no hæmorrhage or other untoward symptom manifested itself.

It is the custom at Tripoli, when a woman has given birth to a child, that all her female friends and relations assemble in her room to congratulate her. On these occasions her chamber is literally crammed with women and children, who are perpetually offering the lady sweetmeats and other refreshments. This crowd vitiates the atmosphere of the apartment, and together with the variety and quantity of confectionary the woman has to consume, must affect her health to a more or less injurious extent. It was therefore to obviate this inconvenience that I insisted upon everybody being sent out of the house; and, moreover, declared that if any person were admitted there before I consented to it, I would not attend any longer upon the case.

For the space of five days after her confinement everything went on favourably. No hæmorrhage or any inflammatory reaction of any kind manifested itself, nor had any lacerations occurred in the soft parts. I had therefore reason to congratulate myself that the woman would speedily recover and resume her ordinary occupations. But the wizard's dictum was more powerful than my best efforts. Finding that his mistress was doing so well, and unable any longer to oppose himself to the customs of the country, the young man allowed the congratulatory horde to rush into his house. On my visit upon the fifth day I found his house teeming with females and children of every description, who, no doubt, had been attracted there in more than usual numbers from the success of the delivery in spite of the magician's art. My warning had been disobeyed, and the issue of the case will show that my previsions were not ill-founded. The result was, that puerperal inflammation came on; the woman acquired a jaundiced hue, and died three days after, that is, she expired on the eighth day after her confinement.

The superstitious dread of witchcraft gained strength in the minds of the public. The old women declared that, although European surgical aid succeeded in effecting the birth of the child, yet it failed to save its life, and that of its doomed mother. The young man shed tears of bitter anguish and regret every time that he met me in the streets, while his betrothed spouse was rejoicing at the success of her diabolical scheme!

*Dr Ritchie* remarked, in an answer to a question from the President, that, according to Dr Dickson, Arab children have not, when born, long black hair on the back.

*Dr Simpson* observed that cases similar to the above used to be common in Scotland. Delivery was always easy amongst the Arabs, except in cross births. It was a mistake to suppose that delivery was always easy in primitive nations. Robertson had shown that malpositions of the fœtus were quite as common in the islands of the Atlantic as here. In Scotland, St Margaret's Sark, which was still to be seen in the Castle of Edinburgh, was at one time thought to be of signal efficacy when worn by women during difficult parturition, and it is said that James IV. sent for it when his Queen was confined. In Aberdeen, according to Robertson, it used to be a common practice to hang the husband up by the heels in difficult cases, until delivery was effected. Pliny mentions an old superstition, which, if not believed in, is at least still acted upon by schoolboys in the present day, viz., that in fighting with your fists you will be able to give harder blows by first spitting on the hands.

### III. DIFFICULT PARTURITION.

*Dr Peter Young* described a case of difficult parturition, and showed preparation. I was called a fortnight previously to see A. B., who was suddenly seized with alarming hæmorrhage at the fifth month of pregnancy. On examination, the os was slightly dilated, but the bleeding had ceased. She was ordered to remain in bed for a day or two, at the end of which she was able to go about her usual duties. Yesterday, immediately on returning home, she was again attacked by hæmorrhage, and lost a large quantity of blood. As the patient was much reduced by the loss of blood, and there was every probability that the hæmorrhage would recur, I thought it advisable to bring on premature labour. With this object I introduced a large sponge tent, and allowed it to remain for twenty-four hours. At the end of that time I withdrew the sponge tent, and then endeavoured to dilate the os with my fingers. In a short time a pain came on, when an enormous clot of blood was expelled. I then felt the feet presenting, and seizing hold of them, endeavoured to extract the child. After considerable difficulty I succeeded in extracting the body of the child. When the head became engaged in the os, the progress of the labour was arrested, owing to the rigidity of the latter. Notwithstanding the greatest care, the tissues of the neck began to tear, and at last gave way altogether, leaving the head in the uterus. I then introduced my finger into the uterus and removed the placenta. With the intention of removing the head I pushed one of my fingers into the mouth and endeavoured to pull down the head. In my efforts to do this, however, the lower jaw gave way, and a portion, together with the tongue and larynx, came away in my hand. I then procured a perforator, perforated the skull at the anterior fontanelle, and, introducing my finger into the interior of the skull, succeeded in removing it.

*Dr Keiller* remarked that the extraction of a retained head was one of the most difficult operations in midwifery. He (Dr K.) had met with two such cases. In one of these cases he (Dr K.) applied the forceps, but could not succeed in catching hold of the head, as it always rolled away. He (Dr K.) then perforated the skull, and endeavoured to extract with the crotchet, but again failed. At last, by using two crotchets and applying them like forceps, he succeeded in removing the head. In both of the cases the mother died.

*Dr Myrtle* mentioned a case where almost every part of the body came away piecemeal in his endeavour to effect delivery. The mother made a slow recovery.

### IV. FATAL CASE OF OBSTRUCTION OF THE RECTUM BY AN ENLARGED OVARY.

The Secretary read the following notes by *Dr Parker* of Nova Scotia:—

About the middle of August 1860, I was requested by Dr Tupper to visit, in consultation, Mrs J., the subject of the following interesting case. She was then 25 years of age; had been married six years; was never pregnant. Her immediate relatives were healthy; and she, prior to December 1859, had not known sickness. The bowels had, up to the date just named, always acted with regularity. The urine was voided very frequently, but without difficulty, and was natural both in quantity and appearance. The catamenia had not been interrupted until "the period" just preceding my first visit.

The heart and lungs were both healthy. Pulse 100.

The mammary glands were small, bnt exceedingly sensitive.

She was very much emaciated, and the countenance indicated long and severe suffering.

Further inquiry elicited the fact that, near the end of the year 1859, while carrying an armful of wood across the floor of an old wood house, the boards gave way, and she was forcibly caught by the thighs in the broken floor. She was considerably bruised by the fall, and on entering the house discovered that some vaginal hæmorrhage had resulted from the accident.

From this date, she began to suffer pain in the pelvic and lower abdominal regions, and ever afterwards had difficulty with her bowels. At first, several days would intervene without an evacuation. Soon, there being no fœcal pressure on the sphincter, all desire to empty the bowels was absent, and to produce such desire, she was in the habit of making firm, direct pressure on the abdomen, just above the pubis. At length the intervals between the evacuations became so protracted as to be measured by weeks instead of days. On the occasion of my first visit, there had been no action of the bowels for ten days or more. I found the abdomen tympanitic, and extremely sensitive over its entire surface, with impacted fæces plainly discernible to the touch, in the larger bowel. In consequence of this distention and pain, nothing satisfactory could be ascertained by an external examination above the pubis. The tongue was red, dry, and shining, without papillæ. The stomach was very irritable. She had *always* a good and often a craving appetite, but at times very little food could be retained, and she was sometimes unable for days together to keep down nourishment or liquids even in the smallest quantities. She informed me that she, on one occasion, some time previously, had consulted me at my office, for habitual costiveness, and that I had prescribed without effect; after which she placed herself under the care of Dr Tupper, who, finding purgatives ineffectual for the relief of this persistent symptom, suspected that the difficulty was mechanical, and on making a vaginal examination discovered a large abnormal growth in the pelvic cavity.

My vaginal and rectal exploration demonstrated the existence of a dense tumour occupying the interspace between these two passages, and almost completely filling the superior two-thirds of the pelvis. It was so firmly impacted or adherent to the surrounding parts, that all efforts, made through the vagina and rectum to dislodge it, failed.

There were three nodulated pyriform projections, placed on its posterior and inferior aspect, varying in size from a walnut to a small hen's egg. No portion of the mass was remarkably sensitive. The tactus, aided by the uterine sound, gave the information that no disease or enlargement of the uterus was present. The inferior and most dependant portion of the tumour was broad, and no exploration could be made with the finger beyond its lower circumference, while all attempts to pass an œsophagus tube, a gum-elastic catheter, or even the smallest quantity of fluid beyond the mass, to the promontory of the sacrum, failed.

We were at first in doubt as to its true character; subsequent examinations, however, satisfied me that it was ovarian. About ten days from the date of my first visit, we were enabled to detect fluctuation in the pyriform projections above referred to. These continued to enlarge somewhat until the 28th of August, at which time no fæcal matter had passed for nearly four weeks. The impaction of the colon was general, and discernible alike to touch and sight.

All the stomach and abdominal symptoms were aggravated.

Although we did not hope to gain much space, by withdrawing the small quantity of fluid the projecting cysts contained, we resolved to tap them. In the absence of Dr Tupper I readily performed the operation, per vaginam, with the long curved trocar used for tapping the bladder, per rectum.

Two communicating cysts were thus emptied of three or four ounces—a larger quantity than was anticipated—of a thin serous fluid, inodorous, and of a dark green colour. After which, hard fæces could be detected with the finger, below the promontory of the sacrum. Repeated injections of soap-suds, with ox-gall, aided by active aperients, now effectually emptied the canal.

The tympanitis, with its accompanying pain, partially subsided. The vomiting ceased, and the unfortunate woman was able to retain her food, and had a short interval of comparative ease. The decreased size of the abdomen enabled me now to make a more satisfactory external examination than I had before done. The tumour could readily be felt, hard and sensitive, in the pelvis, below the pubic bones.

1*st September*.—The abdominal symptoms are again becoming troublesome. The stomach is irritable at times. The bowels require active interference before they can be relieved. Great difficulty is experienced in forcing the injection beyond the tumour.

20*th*.—She expresses herself as feeling better; has been taking exercise for several days in succession in the open air. With assistance, the bowels act almost daily; and she says that she now occasionally feels feculent pressure on the sphincter and a natural desire to evacuate the rectum.

25*th*.—Not so well. More pain and abdominal swelling than on the date of the last report. The cysts which were opened on the 28th ult. speedily closed, and are to-day nearly as large as before the operation. I again emptied them of a smaller quantity of serous fluid than on the first occasion, and injected through the canula two drachms of tincture of iodine. The pain in the pelvis was, for a time, most acute, but it soon subsided. Very little was gained by this second tapping.

It would be tedious to give details of this case day after day or week after week. Were I to do so the report would contain but a repetition of enormous tympanitic swelling and abdominal pain; nausea and vomiting (never feculent); of œdematous extremities; of abortive efforts to relieve the bowels by warm baths, fomentations, injections, and purgatives; of the failure of anæsthetics, powerful sedatives, and opiates, to allay almost constant distress, which often could be compared only to the last pangs of parturition. The cysts were several times emptied and injected with the strongest alcoholic solution of iodine, but without affording any but the most temporary relief.

We urged upon her to let us perform the operation for "artificial anus," but she persistently rejected the proposition. Three weeks before the end came, she had her last evacuation, a very partial one, *after an interval of more than nine weeks.*

In March of the present year, I left Nova Scotia for the Southern, now the Confederate States of America, and on my return to Halifax in May, learned that her sufferings were happily ended by death on the 16th of April.

Before she died, she left express directions with her husband that her body was not to be examined. I am consequently unable to give the post-mortem condition of the parts, a thing, considering the nature of the disease, much to be regretted for pathological reasons.

This case presents several points of interest. 1*st*, It is rare.—All modern writers on ovarian disease, pelvic tumours, and constipation, it is true, refer to the occurrence of such unnatural growths, in this position, and the consequences resulting therefrom when impacted or adherent; but very few cases that I am aware of are recorded in detail.

Dr Robert Lee, in the Cyclopedia of Practical Medicine, says, "In a case which lately came under our observation in the Marylebone Infirmary, an ovarian cyst having become firmly impacted between the bladder and the rectum produced all the symptoms of stricture of the rectum. In a lady now under our care the presence of an ovary *or uterine tumour* [the italics are mine] in the pelvis, which pressed upon the neck of the bladder, renders it impossible for the bladder to be emptied without the introduction of the catheter."

2*d*, While the origin or exciting cause of ovarian disease is often uncertain, this may, without doubt, be referred to the accident I have mentioned. Inflammatory action and consequent enlargement of the organ, resulting from the fall and shock.

3*d*, While the case under consideration cannot compare, as regards the continuance of the fæcal retention, with those cases of constipation recorded by Joerdens, Staniland, Crampton, Valentin, and others, in which the duration was five, six, seven, eight, and even nine months, still this feature of it adds to its interest. The longest period, as already stated, was a little over nine weeks. She was more than twelve weeks without having passed, per rectum, so much as four ounces in weight; and, strange to say, although vomiting almost daily, no feculent matter was ejected from the stomach. Dr Tupper informs me that during the last two months of her life, notwithstanding the irritability of stomach with which she was frequently troubled, she had almost constantly a morbid appetite and partook largely of nutritious food.

4*th*, The failure of the repeated injections of iodine to cause obliteration of the cysts is worthy of note, but it is only what has too frequently been the result of a similar procedure in the larger ovarian cysts of the abdominal cavity.

5*th*, In reference to diagnosis, such a case is interesting, and suggestive too, of the necessity of making careful inquiry and examination, when females present themselves complaining of habitual constipation and difficult defæcation. I have stated that I was in doubt at first as to its true character. In the same locality "pelvic cellulitis" might have resulted in an enlargement, to the touch closely resembling this tumour. Enlarged glands, or a malignant growth, might have occupied the same site. Thanks to Simpson's sound and modern teaching, such mistakes, as were not uncommon in days gone by, are not likely now to occur. Abnormal pelvic ovaries are not often written, Retroverted Uteri.

In a case like this attended by so much distress and pain, a very pertinent question constantly suggests itself to the mind of the medical attendant,—viz., could anything further have been done to afford the sufferer relief? Every effort consistent with safety was made to force the tumour into the abdominal cavity. Excision could not be thought of. A false anus was looked upon as worse than death, and declined by the patient.

After a very partial trial, she refused to continue the use of chloroform, in consequence of the increased discomfort and irritability of stomach which followed its inhalation. Tapping the tympanitic bowels was not resorted to: it might perhaps have afforded slight temporary relief; but this was problematical. Another thing, suggested by a professional friend as I passed through New York, was not tried, the patient being dead ere my return. It was to keep up a drain by tents from the emptied cysts. While the suggestion was practical, it is very doubtful whether it would have possessed any advantages over the repeated tappings. The cysts were not so much the cause of the rectal obstruction as the more solid and superior parts of the tumour. I conclude by repeating the question, Could anything further, than was tried or suggested to the patient, have been done with the probability of affording relief?

*Dr Keiller* remarked that he remembered of a case in the hospital of obstruction of the bowels; the cause could not be very accurately made out during life. On post-mortem examination, it was found one of the ovaries and the uterus were the seat of a scirrhous tumour which pressed upon the rectum. The bowels were enormously distended with gas, giving rise to great pain; and on examination, the mucous membrane was seen to be ruptured and fissured in a longitudinal direction. He (Dr K.) regretted afterwards that he had not punctured the bowel, so as to allow the gas to escape, an operation which in some cases has been attended with great relief.

*Dr Myrtle* mentioned a case where, owing to the formation of a band of lymph around the rectum, the patient was subject to weekly attacks of ileus. Various means were had recourse to, such as an injection of a solution of four grains of tartar emetic, etc., but the most benefit was derived by the introduction of an elastic bougie. It would have been a good case for the formation of artificial anus, could it have been ascertained before death that the obstruction was confined to the rectum.

*Dr A. Simpson* said that Dr Parker seemed to think that the accident which befel the patient was the cause of the formation of the ovarian tumour, as the uneasy symptoms were then first observed. It was difficult to conceive how this should have been the case, and it is much more probable that the shock which the patient sustained displaced the already formed tumour, after, and in consequence of which, pain would be experienced.

*Dr Thos. Balfour* suggested that the application of a freezing mixture to the abdomen in cases of tympanitis might be of benefit.

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## REPORT OF THE TRIAL OF THOMAS DONELLY, ACQUITTED ON THE GROUND OF INSANITY.

By Hugh Cowan, Advocate.

In the following case, tried at Glasgow on 23d December 1862, the jury sustained the defence of insanity, and returned a verdict of Not Guilty, under the Lunacy Act, and finding specially that the prisoner was insane at the time of committing the deed. In accordance with the improved practice of the Court the medical witnesses remained in Court, and heard the evidence led in support of the charge.

The prisoner Thomas Donelly was placed at the bar, charged with the crime of assault, especially when committed with intent to ravish, and pled generally Not Guilty, and specially that he was insane. The prisoner was defended by Mr M'Kie, Advocate.

It is unnecessary to state the general evidence in the case, which was similar to what is usually led in such cases, and which clearly established that the act was committed by the prisoner. The crime was committed on Wednesday 12th November 1862. The prisoner was examined before the Sheriff-Substitute of Glasgow in the course of the day following the crime, and appeared then to be calm and collected, and nowise different from other men. The judicial declaration of the prisoner contained the following curious account of the occurrence. "On going into the house I found a woman there. I thought that I was under the influence of the magistrates, and that I would lose my life if I had not connexion with the woman. I had felt my head ringing, and the sound of music I thought I heard; but I was not under one of the fits I am subject to. I asked the woman if she was not going into bed, and she said No. I then ran at her and we had a struggle together, and she fell on the floor," etc. To substantiate the defence various witnesses were called.

*Ann Donelly*, the prisoner's mother, deponed that the prisoner was subject to fits. After he took the fits he had fancies—thought that he was going to be killed, and that the day of judgment had come. He wished always to go away by himself. The prisoner was quite sound in his mind as a child, and till within the last eight years. He was married nine years ago. He was a regular living man. About eight years ago, he began to complain. He was then working in a shipbuilding yard, working high up with great noise in the yard. He sometimes would have fallen, but was prevented. The illnesses did not continue long, and he was able for work next day. The fits did not come on very often. About half a year after they began, on one of those occasions, he took very badly. He was going through the window if he had not been prevented. He cut his hand very badly, fell on the floor, and frothed at the mouth. He always sang hymns and psalms—something that was nice—when he was ill. He would come out of the fits when spoken to, and then relapse into them. On the night before the crime was committed he came to my house about midnight: he said he had come away from his wife and family, and was going to stop with me and prepare for the day of judgment, that the last day was coming, and that after that week he would go away, and we would hear no more of him.

*James Monteath*, foreman to the prisoner's employers—The prisoner has been a porter in our establishment for a number of years. He was subject to fits. I have seen him affected by them. He foamed at the mouth, was in convulsions, and when he came out of the fit was stupid-like. On the Saturday before the 12th, he said he had been unwell all night, and wished away, but he came back very soon. On the Monday and Tuesday, he was somewhat absent. He either did not listen, or could not understand when spoken to. He said to me, some four years ago, that he was accustomed to see Christ and the apostles.

*George Maxwell*, a fellow-workman, deponed that the prisoner was subject to fits. I have often seen him in them. On the Friday before the 12th, in the afternoon, between five and six, he was in one. He always complained of headache after these fits—looked very pale—was of a bluish colour. On Wednesday morning the 12th, at a quarter before nine, I was with prisoner in a necessary. I asked him if he had heard that—meaning something that appeared in the papers. He cried out, "There's a mystery—it's very mysterious. The Lord have mercy on me." This he cried out very loud, so as to be heard in the flat above. He seemed to be out of his mind, and continued to do this for ten minutes, shaking very violently all the time.

The medical evidence was as follows:—

*Dr Joseph Fleming*—Saw prisoner and examined him for a short time on the 12th, and for a considerable time on the 13th November. I found him labouring under strange delusions. He stated he was obliged to do it—he was obliged to get the better of her. I said, How so? *P.*—I had an order to that effect. *Dr F.*—Who gave that order, or what kind of order was it? *P.*—I just got the order—I cannot say from whom? *Dr F.*—What was your object? *P.*—The order was that if I didn't get the better of her I should lose my life. *Dr F.*—Supposing, then, you had got the better of her? *P.*—Then *he* would lose his life. *Dr F.*—What do you mean by *he?* *P.*—The husband. *Dr F.*—Had you ever seen the wife or husband before? *P.*—No; never in my life. *Dr F.*—How could you do such a thing? *P.*—That's just the thing: I wish you could explain it to me. The prisoner then put up his two hands on his head and said, My head's in a very extraordinary state. *Dr F.*—What's the matter. *P.*—There is something ringing in it: I'm quite sure it is a bell. *Dr F.*—That's very curious: how can your head be a bell? *P.*—That's just it: I'm quite sure it is a bell. The prisoner then said there was sometimes a concert in his head, with music and dancing; also that the City Hall was in his head. On surprise being expressed, he said, That's just the thing: I wish you could explain it. This quite satisfied me that he was labouring under insanity. The ringing, etc., is such an effect as very often follows on epileptic fits—insanity often supervenes. The above conversation was on the 13th. *By the Court*—Epilepsy may exist without insanity. My opinion of him from this conversation was that he did not know right from wrong, though he might to a certain extent. Finding him in this state on the 13th November, would you consider that he would be in a sound state of mind on the 12th? He might or might not. I heard what his mother detailed to us to-day, and am quite satisfied that he has been labouring under epileptic fits. His mind must have been weakened by that—at times unable to distinguish right from wrong. The fit of the preceding Friday might have been a short one. It might have lasted in its effects to Tuesday. Continued epilepsy often terminates in madness. This case, in my opinion, is one of incipient madness.

*Dr George H. B. Macleod*—Saw the prisoner and examined him on the 14th and 15th November, and prepared the following report:—

"*Glasgow*, 15*th November* 1862.—Donelly is a man of forty years of age, and when visited, both yesterday and to-day, wore handcuffs, and was under the care of two attendants. Although very violent when admitted, this man was pale, depressed, and weak-looking when visited by the subscriber, and on the last occasion appeared to be very far from well.

"The reporter, from information obtained from Donelly's wife and fellow-workmen, was enabled to test the accuracy of the replies made to his questions, and he is clearly of opinion that this man is at present labouring under several temporary mental delusions which exercise a very powerful influence upon him, and under the control of which he perpetrated the crime with which he is at present charged.

"The reporter has been able to discover that Donelly has been for several years liable to epileptic attacks, after which he was for a time deprived of reason, and that a few days before the committal of the present offence he had several seizures of more than usual violence.—This is truth, on soul and conscience. GEO. H. MACLEOD, M.D."

That is a true report. There was not the least appearance of acting on his part. My opinion is not altered by what I have heard to-day. If he was on the 12th in the condition I have described, I would consider him not to be responsible. I would have had no hesitation on the 15th in signing a certificate for his removal to an asylum. Epilepsy usually ends, when long continued, in insanity. He told me that he was pursued by a butcher, who tried several times to cut his throat, and had lain under his bed. He said that it had been revealed to him that if he had connexion with this woman he would have power to kill the butcher, but that if the butcher got there before him he would then be killed.

*Dr Thomas Watson* prepared the following reports:—

"*Glasgow Prison*, 19*th November* 1862.—To-day, as on several preceding days, I have visited and carefully examined Thomas Donelly, presently confined in the Prison of Glasgow. I find him lying in a state of partial insensibility—dull, stupid, and with difficulty roused. Being roused, he obeys slowly and hesitatingly orders to perform mechanical acts—as to protrude his tongue, extend his hand, etc. His look is wild and startled: his pupil readily contracts under the stimulus of strong light, but immediately expands with an unsteady and vacillating motion. To-day, but not before, he answers simple questions slowly and imperfectly, as if he had only a partial apprehension of their meaning. Sensation is sluggish and inert, responding tardily to painful impressions, such as pricking or pinching. I think him incapable of any intellectual act, and in that sense insane. He wears, in my opinion, the aspect of a man who has sustained a concussion of the brain, or who is recovering from a recent attack of epilepsy. I am informed by his warder, and by the co-occupant of his cell, that on Friday last (14th inst.) he was the subject of a fit which, from their description, I judge to have been epileptic. I am of opinion that his present symptoms might naturally follow such an attack—with the single exception that they are of unusually long duration. They do, however, correspond with a condition not unfrequently consequent on repeated attacks of epilepsy, in which changes have taken place in the organization of the brain, which tend to imbecility or complete idiocy.—All this I certify on soul and conscience.

"THOS. WATSON, M.D."

"*Glasgow Prison*, 23*d November* 1862.—I beg to make this addition to my former certificate with regard to Thomas Donelly—that he has now emerged from the state of partial insensibility in which for several days he lay, and that I have no doubt that he is at present in a state of insanity.—This I certify on soul and conscience. THOS. WATSON, M.D."

True reports. Prisoner was in a state of violent excitement when I first saw him, and wanted to attack me and the warders. He cried out loudly that I was the person that had come commissioned to kill him. He had no other delusions that I saw. I have no doubt that he was in an unsound state of mind. I would not be surprised if he had an epileptic fit on Friday the 7th, that he should be insane on the 12th. My opinion is that he would be insane then, looking to the evidence in the case.

*Lord Cowan* addressed the jury, and after explaining shortly the general evidence in the case, his Lordship observed—"The defence is that when he so acted he was not accountable for his actions—that he was insane. This is a good defence, for it alters the complexion of the act. It is first of all necessary that you as the jury should be satisfied that the man was insane. The truth is that we cannot be too particular or too watchful in regard to a defence like this; for insanity is so easily assumed that we must be careful. The law requires that the defence of insanity shall be proved to the satisfaction of the jury by the prisoner. The law presumes him to be sane, and he must prove the contrary. The medical men are not the judges: it is for the jury to say whether the defence is proved to their satisfaction. The opinions of the medical men are very important for you to carry along with you in making up your own mind. The medical men have told you that this is a case in which they can trace epilepsy proceeding to insanity. With these observations I leave the case in your hands. There is nothing in the commission of the crime to show that it was not the act of a sane man.

The jury unanimously sustained the defence of insanity, and the prisoner was committed to prison during Her Majesty's pleasure.

## MEDICAL OFFICER OF HEALTH AT GLASGOW.

DR W. T. GAIRDNER, Professor of the Practice of Medicine in the University of Glasgow, has been appointed Medical Officer of Health for Glasgow. The election was unanimous, and the appointment was entirely unsolicited on the part of Dr Gairdner.

## THE HOTEL DIEU.

THE Paris correspondent of the *Times* writes:—"The demolition of the Hospital of the Hôtel Dieu and its reconstruction, which the inhabitants of Paris have loudly called for during many years, are at length decided on. The place on which the new Hospital is to be erected is not yet fixed; but a decision must shortly be adopted, inasmuch as the present building is so defective. This is the second time that the Hôtel Dieu will have been displaced. The primitive position of the Hôtel Dieu was to the south of the little church of St Christophe, nearly in the middle of the *parvis* of Notre Dame. The Seine was not then, as it is now, confined with walls, and its waters, after the least flood, washed the wall of the old city. During the excavations made in 1847, the foundation of this wall was discovered. It followed a line parallel to the river, and its position is distinctly indicated by the iron railing of the garden of the Hôtel Dieu in the Rue Neuve Notre Dame. The Hospital did not, in those distant ages, possess the character which it subsequently acquired. It was rather a house of refuge for mendicants and foreigners. The friars, according to the ancient custom, washed the travellers' feet. It was not until the reign of Philippe Auguste that invalids were admitted. This Prince, who may be called the founder of the Hospital, built the Hall St Denis and its additions. Blanche de Castile, his daughter-in-law, founded the Hall St Thomas, and Louis IX. built the large hall on the bank of the river and a chapel beside the Petit Pont. It appears from the bill of the carpenter who made the beds for this hall, that a bench was placed beside each bed, called 'the bench of repose,'—that is, if the bed contained three invalids, it might be made to accommodate six by permitting three to sit on the bench while three took their turn in the bed. During an epidemic which prevailed in Paris, eight persons were placed in a bed. The buildings of the Hôtel Dieu being in a dilapidated state during the reign of Henry IV., they were repaired by that monarch. Between the years 1802 and 1804 the Hôtel Dieu was again repaired, and the building lost its mediæval character. The chapel, with its doorway, was taken down, and the architect, Clavareau, replaced the handsome and delicate work of the thirteenth century with a heavy peristyle."—*Medical Times and Gazette*, 17th January 1863.

## PUBLICATIONS RECEIVED.

Braithwaite's Retrospect of Medicine: July—December. London, 1862.
Clouston,—Minute Anatomy and Physiology of the Nervous System of the Lobster. By T. S. Clouston, M.D. Edinburgh, 1863.
Curling,—Observations on the Diseases of the Rectum. By T. B. Curling, F.R.S. Third Edition. London, 1863.
Hinton,—Life in Nature. By James Hinton. London, 1862.
Mathieu,—Quelques Observations, etc. By L. Mathieu. Paris.
Pécholier,—Recherches Expérimentales sur l'Action physiologique de l'Ipécacuanha. Par G. Pécholier. Paris, 1862.
Ranking and Radcliffe's Half-Yearly Abstract of the Medical Sciences: July—December. London, 1862.
Transactions of the Medical and Physical Society of Bombay. No. VII., New Series for 1861. Bombay, 1862.

## PERIODICALS RECEIVED.

Births, Deaths, and Marriages, Monthly Return of, for Dec. 1862.
Boston Medical and Surgical Journal,—Dec. 11, 18, 25, Jan. 1. 1862–63.
British and Foreign Medico-Chirurgical Review,—January. London, 1863.
British Medical Journal,—Dec. 27, Jan. 3, 10, 17, 24. London, 1862–63.
Bulletin Générale de Thérapeutique,—Dec. 15. Paris, 1862.
Dublin Medical Press,—Dec. 31, Jan. 7, 14, 21. 1862–63.
Gazette Hebdomadaire de Médecine, etc.—Dec. 26, Jan. 2, 9, 16, 23. Paris, 1862–63.
Gazette des Hôpitaux,—Dec. 25 et 27, 30, Jan. 1, 2, 3, 6, 8, 10, 13, 15, 17, 20, 22, 24. Paris, 1862–63.
Gazette Médicale d'Orient,—Nov. and Dec.
Gazette Médicale de Paris,—Dec. 27, Jan. 3, 10, 17, 24. 1862–63.
Glasgow Medical Journal,—No. 40. Jan. 1863.
Henke's Zeitschrift für die Staatarzneikunde,—No 1. Erlangen, 1863.
Journal de Médecine et de Chirurgie,—Jan. Paris, 1863.
Journal für Kinderkrankheiten,—Sept. and Oct. Erlangen, 1862.
Medical Critic and Psychological Journal,—No. 9, Jan. London, 1863.
Medical Times and Gazette,—Dec. 27, Jan. 3, 10, 17, 24. London, 1862–63.
Medizinische Jahrbücher,—Part 4. Vienna, 1862.
Microscopical Science, Quarterly Journal of,—Jan. London, 1863.
Revue de Thérapeutique Medico-Chirurgicale,—Jan. 1, 15. Paris, 1863.
Wochenblatt der Zeitschrift der Aerzte,—Nos. 39 to 48. Vienna, 1862.
Würzburger Medicinsche Zeitschrift,—Parts 4 and 5, Vol. 3. 1862.

# Part First.

## ORIGINAL COMMUNICATIONS.

### Article I.—*An Inquiry into the Action and Uses of Atropia.* By Alex. Fleming, M.D., F.R.C.P. Lond., Physician to the Queen's Hospital, and Consulting Physician to the Midland Eye and Ear Hospital, Birmingham.

*Physiological Action.—Local Action.*—When a solution of atropia, 1 grain of the alkaloid to 10 drachms of the menstruum, of the same strength as that which I employ for internal use (see p. 625), but made without spirit, is well rubbed into the skin, no change results in the sense of touch—the sensibility to common impressions, or to temperature. The colour of the part is not altered, nor is its contractility—as evidenced by the goose-skin produced by the galvano-magnetic current and wire brush of Duchenne—either increased or impaired. I have determined these points by repeated experiments. Further, from several trials which I have made on worms and the small intestines of rabbits, I conclude that atropia is not a local paralyzer, as has been commonly inferred, either from its supposed mode of action on the iris, or from its curative influence in spasmodic diseases. The alkaloid appeared rather to quicken the contraction of the parts to which it was applied.

Painted on the mucous membrane of the mouth and throat, it dries the part, and—chiefly as secondary effects—impairs both its feeling and movement, perverting the taste, and causing much difficulty of swallowing. These symptoms are much relieved by moistening the parts with water.

Applied directly to the smaller arteries, atropia constricts them (Jones); and this effect may continue for many hours.

*On the Eyeball.*—The same solution of atropia, dropped on the conjunctiva, in a few minutes widens the pupil, and after a much longer interval impairs the sight and lustre of the eye to which it is applied. The eye is misty, and cannot read print, easily seen by its healthy fellow. This has been generally referred to diminished sensibility of the retina; but my observations (made in 1856)[1] prove beyond doubt that it is altogether due to the width of the pupil, and the loss of power in the eye to adapt itself to near vision. That it is in part due to the large pupil admitting too many and too divergent rays is shown by the vision being improved in a dusky light, and in looking through a hole—smaller than the dilated pupil—in a black card held close to the eye. But it is especially due to the inability of the eye to adjust itself to near vision. The defective sight is more marked the closer the object, and diminishes with distance. As the influence of the atropia is wearing off, while yet the difference between the two eyes is decided with a near object, scarcely any difference is perceived with a distant one. The type of a book appears smaller to the atropised eye than to its fellow at the same distance. Hence vision with both eyes conjointly is confused, and, for reading, the dilated eye must be covered.

The solution of strength indicated above (1 grain to 10 drachms) serves well for ordinary use to dilate the pupil. To obtain the deeper effects upon the eye a stronger solution of 3 grains to 10 drachms should be employed. This reduces the iris to a mere line, and completely impairs the sight. The blindness continues from one to four days, and subsides some time before the pupil regains its normal size. The eye thus atropised bears the sun's rays better than its fellow, which seems to show that the iris itself is, in part at least, the seat of the incident impressions which contract the pupil and protect the retina. When this stronger solution is applied carefully to the outer side of the eye, the adjoining or outer part of the circle of the iris dilates some time before that of the inner side. This fact points to a strictly local action. Atropia does not redden the conjunctiva. It has no action on the iris of the dead eye.

Among other observations which I have made on the atropised eye, I found that in adapting the telescope to it the instrument requires to be lengthened, as compared with the other eye; but the focus having been adjusted, vision is perfectly clear, although the eye, without the help of the telescope, be absolutely blind. Indeed, several of those who made trials with the telescope maintained that they saw better with the atropised eye—a fact deserving of the astronomer's attention. In using the microscope with the atropised eye, the object has to be removed a little farther from the instrument; but the focus having been adjusted, vision is as good as with the healthy eye.

I was able to make the smaller image of the atropised eye, as compared with the other, very manifest by means of the reflecting stereoscope.

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[1] Medical Times and Gazette, 1857, vol. i. pp. 270 and 345.

The atropised eye is altered as if for distant vision. The pupil dilates, and at the same time I believe that the lens recedes and diminishes the distance between it and the retina. The following is my explanation of the mode in which these results are produced.

The erectile structure of the iris has been of late too much overlooked in discussing its movements — the singular softness of which appears to imply something more than muscular contraction.

The iris may be congested and the pupil contracted by relaxation of its arteries letting more blood into it, or by compression of its veins impeding the flow from it; and it may be emptied of blood and relaxed, either by contraction of its arteries or by relaxation of its veins, or both conditions may co-operate. The ciliary processes have a mixed erectile and muscular structure, not unlike that of the iris, and it is a most feasible supposition that the lens is advanced by their turgescence, conjoined with contraction of the ciliary muscle, and recedes on their relaxation—the former being an active, and the latter the quiescent condition of the parts.

The sympathetic nerve supplies at the same time contractile power to the arteries and to the radiating fibres of the iris; and it seems most probable that in ordinary dilatation of the pupil the same reflex influence stimulates conjointly the contractile arteries entering the iris and the radiating fibres. These being always associated in action, we can understand that an agent like atropia, directly contracting the arteries entering the iris, will draw into movement by sympathy its radiating fibres, the contraction of which is already made easy by the emptiness of the erectile tissue.

For near vision in normal accommodation I believe that the *pupil contracts and the lens advances.* The veins of the iris and ciliary body are, I believe—as formerly suggested by Wallace, Cooper, and Smee—at the same time compressed by the ciliary muscle causing turgescence of their erectile parts. The turgescence of the iris co-operates with the contraction of its circular fibres to close the pupil. The turgescence of the ciliary body must exert pressure on the vitreous humour, which, diffused equally through that fluid, takes effect on the only yielding point—the lens—and pushes it forward, just as a patient is raised by pressure on the side of a water bed. The ciliary and iridial muscles concerned in these associate movements receive their nervous supply from the same source or third pair.

When atropia is applied to the eye I believe that it reaches, *first,* by imbibition, the arteries entering the iris—constricts them—impedes the flow of blood to, and relaxes its tissue. The constriction of the arteries of the iris, with its consequent relaxation, draws into action, *by functional sympathy* (and without the intervention, by reflex action, of the brain or cord), the radiating fibres, and dilates the pupil. Sinking deeper into the eye, the alkaloid reaches, *secondly*, the ciliary processes, and relaxes their erectile structure and causes distant vision. The relaxation of the ciliary body must cause the advance and expansion of the vitreous humour around the lens; while the lens itself recedes, to occupy the place of the displaced fluid. The ciliary muscle is considered by some anatomists to have both circular and radial fibres, like the iris, and animated, in like manner, by the third and sympathetic nerves.

That the lens recedes and approaches the retina in the atropised eye, is proved by the vision being clearer with distant than with near objects, and by its furnishing a smaller image than the healthy eye of an object at the same distance from both.

According to the view here stated, the normal accommodation of the eye to near and distant vision depends essentially on a change in the position of the lens, brought about in the manner described. The experiments of Cramer and Helmholtz, on the other hand, refer this power to a change in the form of the lens. Near vision is effected, according to them, by an increase in the thickness or convexity, and distant vision by a flattening of the lens. This body certainly does not look as if it could be made thick and thin by muscular action; and as it must be most difficult to make the measurements of its antero-posterior diameter in the living eye, on which this opinion is founded, I would desire to see them verified by others. I say this with sincere respect for these most able oculists. Assuming their observations, however, to be absolutely correct, the change in the form of the lens may be only one part of the phenomena of even normal accommodation; and certainly it is quite compatible with my theory of the mode of action of atropia, in widening the pupil and causing distant vision,—for it must not be forgotten that my explanation applies to both of these changes.

In the dead body, Helmholtz found that the lens has the form—thick and convex—of that adjusted for near sight, which is curious, seeing that near vision is the active state in the living eye.

The remarkable precision of the phenomena produced by atropia in the eye, and the comparatively gradual manner in which they are accomplished, are more easily reconciled with my explanation; and without attaching too much importance to them, some facts in practical medicine which harmonize with it, may now be stated. For many years, I have noted that patients suffering from atonic and exhausting disease, and especially from such as enfeebles in a marked manner vital turgescence and erectile power, as enteric fever, diabetes, anæmia, Bright's disease, and diphtheria, are disposed to wide pupil and distant vision. There is also good reason to believe that contracted arteries, wide pupil, and far sight or virtual blindness, are the *early* phenomena in the eye of the epileptic paroxysm, of fainting, of excessive loss of blood, and of pure sedative poisoning. Again, those conditions which check the arterial flow to the eye tend to dilate the pupil, such as aneurism in the chest and neck impeding the passage of blood into and through the carotid, a tumour pressing on, or a plug in the vessel. Worms and disease of the belly in children dilate the pupil and impair the sight, probably by reflex action through the sympathetic nerve—exciting the radial fibres of the iris and contracting the cerebral arteries. Hence, also, in all likelihood, the train of anæmic head-symptoms so characteristic of tape-worm.

On the other hand, those conditions which favour arterial plethora or venous congestion of the head tend to contract the pupil —such as dilatation and loss of contractility of the aorta and carotid—giving passage to a larger stream, and checking its force less, than the normal vessel. Aneurism of the innominata closing the subclavian and leaving the carotid free will increase the force of the blood current in the latter, and this will be further augmented if the carotid be diseased, dilated, inelastic, and non-contractile.

It does not fall within the scope of this paper to allude to the beautiful researches of Budge, Waller, Brown-Séquard, Von Graefe, Bernard, Harley Bell, Hughes, and others, into the physiology of the iris, and of the nerves which preside over its movements, nor to the very useful application of this knowledge to practical medicine by Macdonnell, Gairdner, and Ogle, who have traced contraction and dilatation of the pupil to pressure on, or disease involving the cervical, sympathetic, and other parts of the nervous system connected with the movements of the iris. But, in examining cases of aneurism, and of tumour in the neck and thorax, accompanied by alterations of the pupil, it is well also to bear in mind the causes of change in the size of the pupil to which I have adverted, and to make careful inquiry into the condition of the arterial and venous currents in the neck.

The results related in this paper were obtained chiefly by observations on the eyes of my students in the Queen's University, who most cheerfully gave me every assistance in my inquiries. I have also made numerous experiments on the eye of the cat and rabbit, some of which will be recorded in the appendix.

## ARTICLE II.—*On Inducing Premature Labour with Caoutchouc Bags.* By ALEXANDER KEILLER, M.D., F.R.C.P.; Vice-President of the Edinburgh Obstetrical Society; Physician to the Royal Maternity and Sick Children's Hospitals, etc.; Lecturer on Midwifery and the Diseases of Women and Children in the Edinburgh Medical School.

(*Read before the Edinburgh Obstetrical Society, August* 6, 1862.)

I HAVE been induced to bring again before this Society an old subject under a new title given to it by a distinguished member of the London Obstetrical Society, who has recently published in the Edinburgh Medical Journal an article entitled, "The New Method of inducing Premature Labour at a Predetermined Hour." The author to whom I now refer, Dr Robert Barnes, also read some time ago, at a meeting of the Obstetrical Society of London, an excellent paper on the "Indications and Operations for the Induction of Premature Labour, and for the Acceleration of Labour," which was subsequently published, together with the discussion which took place on that occasion, in the third volume of the Society's transactions.

In now attempting to discharge what I have been led to look upon as an act of duty towards this Society as well as towards myself, I must, I fear, be out-spoken as well as egotistical, as my chief reason for so directly referring to the above-mentioned communications is to prove that the so-called new method, "the primary and all-important point of which" (to use Dr Barnes's own words) "consists in the artificial dilatation of the cervix uteri, by the use of specially constructed caoutchouc bags," cannot be fairly claimed by Dr Barnes, as it can be readily enough shown to have been frequently discussed and successfully practised in Edinburgh before it was adopted by him.

All I now require or desire to do is simply, and very briefly, to refresh the memory of the Society as to what was said and done at some of our former meetings.

The following is our billet for March 1859:—

EDINBURGH OBSTETRICAL SOCIETY.—The Society will meet in the New Town Dispensary, 17 Thistle Street, on Wednesday the 9th inst., at eight o'clock P.M.

BUSINESS.

1. On the Cure of Retroflexion of the Uterus, by Dr Moir.
2. Case of Induction of Premature Labour, with New Mode of dilating the Os Uteri, etc., by Dr Keiller.
3. New Abortion Forceps, by Dr Brink.

And for 11th May 1859:—

Further Remarks on the Use of the Uterine Dilator, with *Cases*, by Dr Keiller.

A published report of our proceedings states:—"Dr Keiller communicated to the Society some observations on a new mode of dilating the os uteri and passages, which he had successfully adopted in several cases. The appliance consisted of an *ordinary* vulcanised india-rubber bag, or dilatable pessary, which could be readily introduced in an uninflated state into the vagina, or into the os uteri, and afterwards inflated to the required extent by means of an attached syringe, or other suitable apparatus for pumping in air. The smooth and highly distensible material of which air-pessaries *are usually made;* the comparative ease with which they can be introduced; their ready and equable dilatability; their slowly, or, if need be, instantaneously, collapsing or allowing the injected air to escape,—were advantages which none of the appliances or modes hitherto adopted for vaginal or uterine dilatation seemed to possess. The comparatively slow and irritating action of sponge, or other substances ordinarily used as tents or uterine dilators, together with the readiness with which they are found to absorb offensive discharges, are objections which cannot be said to apply to the vulcanised caoutchouc inflating bag, or air-pessary, which can be so readily and successfully used, either as a dilator or tampon, in all cases in which dilatation, compression, or plugging are usually had recourse to in obstetric practice. Dr Keiller exhibited various forms of the dilatable pessary he had used, some of which he had specially got made for dilating the os uteri in cases in which it was considered necessary to do so. Dr Keiller cited the particulars of several cases in proof of the practical value of this novel use of the air-pessary. Dr Weir and Dr Pattison had witnessed its use in some of the cases referred to by Dr Keiller, and testified to the efficiency and apparent value of the appliance."

The case in which I, for the first time, used a dilatable india-rubber bag as an obstetric instrument, I shall here recapitulate.

### *Case of Induction of Premature Labour—Use of the Caoutchouc Bag as a Uterine Dilator and Supporting Plug.*

Mrs Field, aged 30, was delivered of her first child on the 24th January 1857, after a very tedious labour, extending over nearly four days (from Tuesday until Friday morning). Professor Simpson was called in to terminate the labour, which was effected by turning and subsequent craniotomy. Catheterising was afterwards necessary for upwards of a fortnight, and a slow recovery took place. I was requested to visit her a few weeks before her next confinement, but the important facts connected with her first labour were studiously concealed from me until it became too evident that parturition could not be naturally accomplished. While stepping into bed on the evening of Tuesday the 9th of February 1858, the membranes suddenly ruptured. I was soon afterwards summoned, and on arriving found her complaining of slight pains, which from time to time recurred, without, however, having much effect on the os uteri until early in the morning of the 12th, when, on being sent for, I found the pains of an expulsive character, and the os sensibly more dilated; but delivery could not be safely accomplished until about 9 o'clock P.M., when I extracted an unusually large male child after craniotomy. During the progress of, and immediately subsequent to, this second parturition, the cause of the delay and difficulty was obviously traceable to the condition of the pelvic brim, which was preternaturally diminished in its antero-posterior diameter.

The obstetric history of Mrs F. clearly indicated the propriety of inducing premature labour in the event of pregnancy again taking place, and with this object in view the usual advice was given, which resulted in my being apprized of the expected third parturient period, and the necessary appointment for the induction was accordingly made and kept; for on the evening of Tuesday the 1st March 1859, which, in this instance, corresponded with the expiry of about seven-months' period of utero-gestation, I commenced to adopt the plan which I was hitherto accustomed to follow in inducing labour prematurely,—namely, that of artificial dilatation of the os, and separation of the membranes from the region of the cervix uteri by digital examination, sponge-tents, and occasional application of the water douche. These means, however, seemed to be unusually inoperative, for although repeatedly had recourse to, very little progress towards useful dilatation was made; and on Friday evening, while assisting the yet imperfect dilatation by means of Kivisch's plan, etc., the membranes suddenly ruptured, and, on examination, the presentation, which was still difficult to reach, was ascertained to be that of the feet, by which delivery might have been accomplished had the condition of the os and cervix been satisfactory; but these parts, notwithstanding what had been done to secure their dilatation, were unusually contracted and firm. I again introduced a full-sized sponge-tent to expedite dilatation and give support to the presenting part of the child, which was, on stethoscopic examination, ascertained to be alive. There being so little progress towards dilatation and no signs of the accession of uterine contraction, there was no great reason to have recourse to anything like the *accouchement forcé;* and it was not until Saturday morning that I deemed it prudent to adopt further measures to expedite the as yet slow process of dilatation. It was at this point of the case that the importance of having some appliance fitted to expand safely the os uteri, and at the same time calculated to support gently the presenting parts, first occurred to me; and knowing the danger to which the child in this case was exposed by the too early rupture of the membranes, more especially as the presentation was a footling, I was naturally led to think over the plans I had in this and other cases followed, and to compare them with nature's simple plan of securing the ends in view. These reflections, aided by the counsel of my friend Dr Graham Weir, resulted in my having recourse to the simple caoutchouc bag, which, when carefully introduced within the os uteri and gently distended, at once becomes an admirable substitute for the unruptured membranes, safely opening the passages, and, like the natural "bag of waters," softly supporting and protecting the contained and containing structures when exposed to premature protrusion or undue and dangerous pressure.

I make special reference here, as I have always done in regard to this matter, to Dr Weir, because it was principally in consequence of his directing my attention to the description of an instrument proposed and used by Mr Spencer Wells, for "dilatation of the female urethra by fluid pressure," that I was induced to think of the advantages to be derived from a caoutchouc bag as a uterine dilator and promoter of labour. I now exhibit the sketch and notice of Mr Wells's instrument, as it appears in Ranking's Retrospect, volume xxviiii., 1858, p. 293, and abstracted from the Medical Times and Gazette for July 1858. While conversing generally and "comparing notes" on our individual experiences of induction cases, and talking over the particular case in hand, we agreed that the instrument described by Mr Spencer Wells might be of some service, and I accordingly resolved to give it, or something similarly constructed, a trial; but the unsuitableness of such an apparatus to effect sufficient dilatation, together with the objectionable character of its hard and pointed extremity, induced me to prefer an uncomplicated caoutchouc bag and tube, which could be readily inserted in a collapsed state, and subsequently distended or inflated to the desired extent. In such a simple apparatus I thought I saw much of the simple machinery by which nature opens up the os uteri, and simultaneously provides that gentle support and elastic pressure which are usually observed in the mechanism and vital movements of parturition. In this I was not disappointed, for, on procuring an ordinary-sized india-rubber inflatable pessary, and using it as I did without unnecessary delay, my impression was at once strengthened, and I soon satisfied myself that the experiment was likely to prove highly successful; for, after folding up the thin and soft expanding bag which I had just selected, then cautiously introducing it in a conical form through the as yet slightly dilated os uteri, and then, in the first place, slowly distending it with water by means of the Higginson syringe (which I had previously used according to the plan of Kivisch with apparently but little effect), and subsequently and more effectually expanding it by inflation (the mode of expansion which I prefer to that by the injection of water, especially since the improved air-inflating appliance has been introduced), I found that the dilatation of the os and the safety of the child were very materially secured.

The novelty of the means used and the effect produced by the simple appliance were on this occasion so striking that I at once sent for Dr Weir, and also for Dr Little of Singapore, to witness the progress of the case. About eight P. M. on Saturday, after one or two inflations, the os became sensibly dilated, and pains of an expulsive character were for the first time established, the presenting foot now descending through the dilating os at the recurrence of each uterine contraction. With the view of preventing the premature descent of the feet, and at the same time with the object of making the artificial bag otherwise serve the purpose of the natural membranes, I from time to time gently pressed up the feet, and in the absence of pain gently insinuated and then dilated the caoutchouc bag, which admirably served the important purpose of supporting the feet until sufficient dilatation of the os and passages was secured, and which could not have been more effectually accomplished by a natural bag of unruptured membranes, which this artificial appliance greatly resembled. When Drs Weir and Little arrived, the case was slowly advancing, and, after examining for themselves, they agreed with me in thinking that nothing could better represent the mechanism, nor answer the important purpose of the amniotic bag, than the extremely simple substitute then used.

The case was allowed to proceed as an ordinary footling, and terminated about ten P. M. in the live birth of a fine female child. This being the first case in which I applied "the new method," I could not but feel satisfied with the result; and as the obstetric advantages derivable from the instrument used, seemed to me to be great, when lecturing to Dr Simpson's class on the following Monday, I took occasion to show the instrument I had used, to complete the history of the case, which I had referred to in Friday's lecture, and specially to mention the various uses to which I then considered the dilatable caoutchouc bag would be applicable.

Believing that the instrument was novel as an obstetric implement, I took occasion not only to show it, but to speak of its capabilities, whenever proper opportunities occurred, which at the time were ample enough, seeing that I was then lecturing in the University for my friend Professor Simpson, doing daily duty in the female wards allotted to me in the Royal Infirmary, and attending as the acting physician to the Maternity Hospital, where the instrument was frequently and successfully applied to facilitate labour.

It is not necessary here to repeat what I then said regarding the various uses of the caoutchouc dilator; suffice it in the meantime for me to aver that, ever since I first applied this simple apparatus, I have felt convinced that it is not only an invaluable inducer and promoter of labour, but that in many other important cases in which not only a dilator but a support or plug is called for, it will be of service, and I feel assured that to the obstetrician it will often prove a valuable aid when timeously and properly had recourse to.

I might produce numerous notes of cases from obstetric friends, who were, apparently at my suggestion, led to try this plan for inducing labour; but such a course is here, I apprehend, quite uncalled for; I have, however, selected one note received from our president, Dr Pattison, who is unavoidably absent to-night.

"Mrs B. was delivered of her fourth child, a boy, on the 11th of April 1859. Labour was here induced about the eighth month, as it had been twice before. Dr Keiller assisted me and used his dilator, which he had described and shown at a meeting of the Obstetrical Society (*vide* Minutes). Mrs B. was not aware when we were to call. The os uteri—the external—was about the size of a shilling, a very small dilator was used and distended with great effect. We got the fingers inserted, when a larger dilator was introduced. In a short time we felt the head floating in the waters at the brim, which could easily be moved about, when a foot and a hand came down. I passed my hand into the uterus and grasped the foot, ruptured the membranes and delivered the child alive. My notes on this case conclude by stating that all was completed in about two hours and a half from the time we first commenced to deliver."

It would appear that, during the discussion which took place on the reading of Dr Barnes's communication at the sister Society in London, my claim to priority in the using and recommending of the caoutchouc bag as an obstetric instrument was specially referred to by Mr I. Baker Brown and others, who were well aware that my first cases were reported in March 1859, whereas Dr Barnes's first case was in April 1860.

In the second and more recent paper of Dr Barnes, which appeared in the Edinburgh Medical Journal for July 1862,—"On *the* New Method of Inducing Premature Labour at a Predetermined Hour" (and it is to this communication that I wish, on the present occasion, to direct more particularly the attention of this Society), no reference is made to the history of the caoutchouc dilator, except in the form of a foot-note, which I may be permitted here to transcribe.

"In a paper on the Induction of Premature Labour, published in the last volume of the 'Obstetrical Transactions,' the author has given an historical survey of the various operations that have been resorted to. He has there endeavoured to assign to the proper sources the merit attaching to each method; and has traced, to the best of his information, the successive modifications and applications of the caoutchouc bag, down to the form and use adapted by himself to the induction and acceleration of labour."

But as my present object is to show that Dr Barnes has been, to say the least of it, unfairly silent as to the claims of those who, prior to him, were engaged in developing and introducing into obstetric practice the method of inducing premature labour by caoutchouc dilators, I forbear entering further at present into the special merits of the latter.

From what I have stated, however, it will be obvious that I consider "the new method" will be found of great value in the induction and promotion of labour; but seeing that Dr Barnes has written his last paper for the purpose of inculcating the propriety of making the uterus expel its contents "at a predetermined hour," I wish, before concluding, to notice his strongly expressed views on the subject.

The following quotation may well surprise even those most experienced in operative midwifery:—"Labour may be induced at will, and terminated, if desired, at an appointed hour, with as much precision as to time as almost any operation which the surgeon performs. By adopting this new method, it is just as feasible to make an appointment at any distance from home, to carry out, at one sitting, the induction of labour, as it is to cut for the stone. The operation is brought within the entire control of the operator. Instead of being the slave of circumstances, waiting anxiously for the response of Nature to his provocations, he is master of the position. He determines beforehand, on a survey of the requirements of the case, the time when the patient ought to be freed from the imperilling pregnancy, and may with confidence announce to her the term of her anxieties." It would indeed be fortunate for the accoucheur as well as the patient, were this by any means generally practicable; but although in some cases labour may be induced within a given period, not only without injury to the patient, but with positive advantage, still it must not be forgotten that an indiscriminate adoption of the practice cannot but occasionally, and perhaps frequently, lead to serious consequences.

The doctrine which Dr Barnes enunciates, that labour can be safely and readily induced at a "*predetermined*" hour, and which, as must be inferred from the extract just quoted, is considered by him to be sound, seems based on very insufficient evidence.

Notwithstanding the facts contained in the cases given in illustration of the practice recommended, Dr Barnes concludes by making the following statement:—"A distinguishing merit of the new operation consists in the substitution of gentleness for violence. The *accouchement forcé*, held necessary at times to avert greater perils, may be altogether discarded from practice."—A statement, the correctness of which I do not dispute as applied to appropriate cases, among which, however, those given in Dr Barnes's paper cannot well be numbered. Their special appositeness does not, at least to me, readily appear, for neither was the mode of delivery confined to that of "the new method," nor was the hour in any one of the cases determined beforehand,—a circumstance which, in spite of Dr Barnes's strongly expressed views on the subject, can be readily enough understood by the majority of practical obstetricians, who, while carefully watching and aiding the vital process of parturition, have not failed to observe the difficulty and danger of forcing it to keep an appointed hour. We have all, doubtless, at times induced labour in an unusually short time,—and I may with truth say that I have repeatedly and successfully brought on labour by the simple and effective method referred to; but, as an obstetric teacher, I for one must object to the title and plea of Dr Barnes's paper, on the ground that it is in many cases impossible to predetermine the exact hour of delivery, and more especially on the more serious ground, that to inculcate any such refinement as to *time* in obstetric practice is by no means free from danger; and, moreover, there is a great danger of inexperienced practitioners being led to attempt "an operation demanding considerable nicety of discrimination and delicacy of execution," which, with all respect for Dr Barnes's well-chosen expressions on the subject, I would humbly submit are qualities which cannot always be looked for "in those who undertake the responsibilities of obstetric practice." It cannot, I think, be too much kept in mind, that the pregnant uterus is not a mere "bottle" admitting of being readily uncorked and emptied at any given hour. To attempt to accomplish within a predetermined time what nature will, with judicious assistance, induce in her own and best way, and at her own and best time, seems to me to savour of "meddlesome midwifery." But I shall defer entering more fully on this important subject until a future occasion.

I exceedingly regret being obliged thus to criticise Dr Barnes's writings, as I greatly respect his ability and zeal as an obstetrician. But I consider it a duty to this Society, and to my many professional friends and pupils, whose knowledge of the facts I have alluded to, has led them to accuse me of undue silence, and whose expostulations have forced me again to bring before you a subject on which I feel entitled and bound to speak. An apology is due to the members present for encroaching so much on their time and goodwill, especially in regard to such an obviously personal matter. The only excuse I can offer, besides that with which I started, is the importance which must necessarily be attached to the corroborative testimony of this Society.

## ARTICLE III.—*On Gunshot Wounds.* By ALEXANDER NEILL, Assistant-Surgeon, 65th Regiment.

(*Read before the Medico-Chirurgical Society, 21st January* 1863.)

DURING the past ten years the subject of military surgery has been brought prominently before us, and has excited much interest in all branches of the medical profession, and of all departments of both services. For many years back we have had military surgery treated as a speciality, and taught as such in most recognised schools. Instruction in this branch of the profession was formerly confined to those who proposed to enter, as medical officers, either the army or the navy; but in the present day, when our whole nation has become essentially a military one, and when every man who can "shoulder arms" carries a rifle, is it not necessary that the peculiar forms of accidental injury and disease incidental to warfare, should be brought prominently before, and taught to all who now study for the medical profession?

We all recollect the horror with which we read accounts of the fearful wounds of our men during the Crimean war, and the numberless capital operations resulting as their consequence. Then the mutilations of our soldiers in the Indian mutiny and Chinese war followed, and are seen in numbers in our large invaliding establishments and general hospitals. Every day they must come under notice in private practice, where years of suffering, terminating only with life itself, are the results of wounds, more especially gunshot wounds. Why surgery, in the general sense of the word, should be divided into surgery proper and military surgery I know not; for in the present day, when weapons of defence are in every man's hands, and often in the hands of those who scarcely understand their mechanism or their use, we are constantly hearing of serious accidents occurring.

The meaning of ball-practice, and the several words of command necessary for loading the rifle are now universally understood, but the great danger to be avoided in the use of them is scarcely appreciated. Certainly, we sometimes meet with individuals who have a most wholesome dread of firearms, and who are not likely to endanger their own lives, but who often suffer from the recklessness of others. Gunshot wounds are frequently seen in private practice, and often of a most severe character. The universal enthusiasm for rifle-practice must occasion many accidents, and surely every person should have a knowledge of the treatment of wounds caused by the rifle and bayonet, as well as those caused by a blow or the knife.

Gunshot wounds have been written about ever since gunpowder was discovered; and wounds caused by a leaden bullet are treated of by Celsus previously to the introduction of gunpowder, the characters of which were very similar to those of the present day, but of course not of so severe a character. Bones and arteries were seldom injured, and the wounds were generally contused and lacerated, owing to the small amount of impetus given to the ball. *Deaths* from these leaden bullets were not frequent, and the imaginary poisonous effects of the introduction of lead into the tissues seems to have occupied more of the attention of the surgeon in an endeavour to counteract its influence, than the more serious effects of the wound itself. Even so far back in the annals of medical and surgical history as the time of Celsus we read of the most approved plan of treatment to be followed in the case of wounds from leaden bullets, the correct mode of discovering them, with the after-treatment of the wound, which in those days must have caused more misery and suffering than the infliction of the injury itself; and I have no hesitation in saying that a patient had a much greater chance of losing his life under the surgical treatment of his wounds than by any direct injury done to his body by the bullet itself. Of course, the first thing the surgeon did was to remove the foreign body, which was generally embedded in the soft tissues; then he proceeded to enlarge the external wound, under the supposition that the poisonous effects of the lead would escape from the system with the loss of blood. Then numerous and varied hot poultices were applied—almost every surgeon having his own favourite form of it—in which lay some subtle virtue for the extraction of the poison from the wound. Plasters of different sorts, and various healing cataplasms were used. The wounds were encased in these filthy messes, and the process of suppuration speedily setting in, the pus not being allowed a vent, and the contact of clean water or fresh air being considered most prejudicial, can we be surprised at the deaths that resulted from these forms of treatment. The crowded state of the hospitals of those days, the want of attention to proper ventilation, the intolerable stench of the putrid discharge from their wounds, not removed more frequently than once a-week at the oftenest, and the inevitable result of this, the formation of maggots in the wounds, induced those forms of low fever by which we have lost so many fine soldiers, and which swept off the unfortunate creatures by scores.

Very little alteration in the treatment of gunshot wounds was made until the commencement of the Peninsular war, and even during the early part of it the same mode of treatment was pursued; but owing to the great losses of men from deaths in hospitals having occasioned some inquiry into the treatment of wounds in general, it was resolved to abandon the system previously in vogue, of invariably making the wound larger than it was by incision, and instead of the hot poultices and dirty plasters, to adopt the more sensible plan of cooling applications. And as it was found that the patient's general health was injured by having his wounds always saturated in the pus discharged from them, instructions were given to endeavour to keep the parts as clean as possible, and to change the dressings "more frequently" than once a-week. Since then great advances have been made in military surgery, and we now appreciate the absolute necessity of avoiding over-crowding in hospital wards, of paying great attention to proper ventilation, of keeping the wounds well washed in cold spring water, and never allowing any offensive smell to arise from the collection of pus, thus conducing to the patient's comfort and general health, which formerly were never studied. The consequence of this mode of treatment is, that we now have cases of men recovering from wounds almost always looked on formerly as fatal. We seldom or never hear of hospital gangrene, or any of those diseases created in the wards of an hospital where a number of cases of wounds discharging pus are under surgical treatment.

In the present day our missiles of defence and offence consist of the rifle and bayonet, smooth-bore gun, pistol, cannon of various calibre and formation, calculated to throw either round shot, or cylindrical shot, or shell percussion, as in the Armstrong gun, or with a fusee, as the old gun. Then we have the mortar and cohorn, for a vertical fire, to use an artillerist's expression, calculated to throw shell. Then for the smooth-bore cannon there is the solid shot, the grape shot, and the canister shot, which in their names express their form of composition. Bar shot are two large shots welded together, with an iron bar between; and chain shot, with a chain between two large shot, which are calculated to mow down like a scythe, making fearful wounds. The two latter are used chiefly against bodies of horse, and on shipboard. The shrapnel shell is a hollow sphere of iron, filled partly with gunpowder, and partly with pewter or iron balls of small size. The common shell is an iron sphere filled wholly with powder. The musket ball is round and solid of lead. The rifle bullet is an elongated, expanding, cylindrical, conoidal projectile, as the musketry instructors tell us, weighing about one ounce, and calculated to travel an immense distance, and at a very great rate. The wounds caused by cannon shot and shell are fearful, and those by the rifle bullet, though not so severe to look at, are often most dangerous. It is a difficult thing to judge which form of weapon is the most destructive of life. Against masses of troops the cannon is no doubt very efficacious; but then, at certain ranges, the rifle is equally so. The characters of the wounds inflicted by these various missiles are equally varied. Shot and shell tear away large portions of the body; and I have seen legs and arms stripped off, one half of the chest quite cut away, and the body cut in halves by cannon balls. Shell generally, by exploding in fragments, either impact themselves in the tissues, making wounds frightful to look at in extent, or merely cut away portions of the body. The wound of the rifle bullet is, of course, very small, and apparently very innocuous compared with the large-shot wounds, but they are just as efficacious in the destruction of life. To give an illustration of the force with which a cannon ball must travel. I may mention that I saw a case where a man had all the ribs, both clavicles, and one arm fractured by attempting to catch and stop a round shot, which was apparently spent, and was rolling along the ground in an innocent manner. He caught at it, and was thrown down, struck on the chest, and the shot rolled over, leaving him in articulo mortis. In this case a portion of fractured rib had passed through the pericardium, and wounded the substance of the heart. What are called spent rifle-balls are capable of inflicting much and severe injury, and always make an extensive bruise. To show the effects of being hit with a spent ball, or one that has lost its greatest impetus, I may mention a case that came under my notice, where a colonial volunteer was standing talking in a group of his comrades during an action, and was struck by a ball in the abdomen, over the region of the bladder. The ball fell on the ground at his feet, without either injuring his clothes or even marking the skin. He did not feel much pain at the time, and walked to the hospital, a distance of two miles, with the ball in his pocket, without feeling much pain, but he died shortly afterwards from peritonitis and extensive inflammation of the bladder. The entire surface of the abdomen presented the appearance of a severe bruise in a few hours after being struck. I could mention several other cases more or less bearing on this point, but they only help to prove the fact, that a spent ball can kill without wounding the integument, and show how small an amount of injury will often take away life; but, on the other hand, it is wonderful how great an amount of injury the human system can endure without succumbing under it. Both legs may be shot away, with great loss of blood, and amputation be necessary afterwards; yet, with such mutilations, death is not always the result.

One of the first peculiarities of gunshot wounds to be mentioned is, the shock to the system, which is generally present in a greater or less degree; but it is not always the case that in very severe and dangerous wounds the amount of shock is very great. I have seen some instances of comparatively trifling wounds where the extreme prostration of the vital powers seemed to indicate much injury, but on examination discovered no dangerous wound, or even great loss of blood. The shock does not always bear a marked relation to the severity of the injury: it seems to depend greatly on the constitution of the patient. To his mental powers and his physical confirmation may be ascribed many of the anomalies we observe in this state of the nervous system. Ballingall has noticed this in his excellent work on Military Surgery, and ascribes it to the same causes. However, it is an undoubted fact that when a person feels himself wounded, let his self-possession and his coolness be what it will, he cannot help feeling an irresistible tremor and nervous agitation, even though it lasts only for a few moments. In some persons the power over the feelings is brought under the domination of the will, and consequently such people speedily regain their self-possession, and will submit to any operation, or endure any amount of pain, without changing even the expression of their countenances. To the suddenness of the injury, and the uncertainty of the amount of danger to life which is sustained on the instant, and when the mind has been engaged in some other manner, and perhaps in a high state of excitement at the time, may be ascribed this peculiarity; and in which instant the reason can see nothing but extreme danger. The amount of the shock to the system is to be observed and studied, and is of immense importance in determining as to the performance of any operation at the time of the injury, or deferring it until some other period when the nervous system shall have recovered its power, and the mind its equilibrium.

It is a matter of serious import to us when we have a patient on the field who has received a wound, which we know requires the performance of some capital operation, such as the removal of a limb [and who is at the time in a state of complete shock, which of itself is sufficient to destroy life], to determine what to do in such a case. The nature of the injury, its situation and extent, must be the first inquiry, and a hasty opinion formed in the mind as to its probable result, so as to determine the question of immediate operation to save life, which may be ebbing fast from loss of blood or shock. It is advisable, if possible, to defer any capital operation till the patient be removed to a more suitable operating theatre than the open field; and it requires no small amount of nerve and decision of character to enable the surgeon to contemplate calmly the necessary steps to be undertaken when in action.

Gunshot wounds of every variety are classed in the Army Medical Reports under the heading of Vulnus sclopitorum, though they might with more propriety be subdivided into simple and compound. The mere *flesh wound*, caused by the passage of a ball through the soft parts without involving injury to bone, artery, or nerve, being one subdivision; and the more *complicated* injury, where fractures of bone and other dangerous lesion has occurred. Gunshot wounds are sometimes classed as mortal, dangerous, severe, and slight,—each of which bears its explanation in its name. The characters of a gunshot wound are those of a contusion and laceration of all the tissues. Sometimes they are so simple as to bear resemblance to a punctured wound, particularly if a rifle ball, revolving on its long axis, has passed through the soft part at a great speed; but within a few hours it resembles a contusion. The wound of entrance, as it is termed, bears no comparison in either size or shape to that of exit when a rifle ball has caused the injury. In the former you see the edges of the wound curving inwards, and its circumference small, with little or no hæmorrhage. In the latter the wound is large, with torn and irregular edges projecting outwards, and perhaps only slight oozing of blood. In a short time, averaging an hour, around the entrance wound slight redness begins, gradually extending to about two inches around its orifice. Again this colour changes to blue or greenish-black, and you see all the appearance of a severe bruise, with a small wound of the skin, its edges still curved inwards. In the exit wound the discoloration of the skin is not apparent. Inflammation quickly sets in throughout the whole course of the wound, and suppuration more or less profuse follows, with sloughing of the injured tissues. During this stage secondary hæmorrhage is to be looked for, if in the course of the missile an artery has been injured or approached too closely; but loss of blood from a wound of this description is not frequent. Pain is great in the injured part, and much increased by motion of the limb; cicatrization throughout the whole course of the wound sets in, and in this manner heals. By care, and with every circumstance favourable to recovering, a patient may soon be going about once more, and have no ill effects from a wound such as I have described. Great loss of substance in the tissues is also well marked, induced by the separation of a slough more or less extensive.

The treatment to be pursued in a case such as I have described is very simple; the wound must be examined and sponged, any foreign body, such as shreds of clothing, etc., removed, and in suitable localities uniform pressure may be applied; in a simple wound, union by the first intention may be induced by closing the orifices and applying slight pressure by bandage, with attention to the position of the limb by giving it complete rest. In one case I saw this result well marked, where the ball passed through the muscular part of the upper arm in its anterior aspect. The wounds were immediately closed from contact of air by isinglass plaster, and a bandage applied. The arm was supported in a flexed position, and cold water abundantly used, to keep down inflammation. The orifices of the wound were quite healed, and the arm in use ten days afterwards. In a similar case, where a different plan of treatment was followed, suppuration set in, and the arm was useless for three months. With water, either cold or tepid, I have seen many almost miraculous cures effected. I consider that in cases where the vitality of the soft parts has been partially destroyed by bruise, tepid water and lint are very necessary as assisting to bring back the vitality, after which cold water may be applied with much benefit. In cases of compound fracture of the long bones with gunshot wound, after the limb has been put up in splints and bandages, cold water will prove the best application, kept constantly dripping over the wound, which must not be closed over by either splint or bandage, but so arranged as to be quite exposed to the water. In seven cases of this description, four were gunshot wounds with fracture of the humerus, in three of which the ball passed out, and in one the ball was lying in the bone and was at once extracted. In these three the arm was put up in the usual manner as for fracture, but leaving the external wounds exposed. The patients were kept in bed with the arm flexed and so arranged, that a constant stream of water was kept trickling over it, carrying off the pus discharged. A pail was made and raised over the bed, with a tube so placed as to carry a small stream from it to the limb, and regulated so as to change the temperature of the water occasionally if needful; one case was fracture of the tibia in its lower third, and one fracture of both bones of the forearm, with two external wounds. In the former case the man was aged twenty-four years, and three months after recovering was killed accidentally. On dissection of the leg, I found that the fracture had extended down to the joint of the ankle, and both pieces of bone were joined perfectly: the mark of union was most distinct. In all these the plan of treatment by cold water was adopted with successful results.

In fifty cases of gunshot wounds, including all descriptions, treated thus by local applications, a successful issue was obtained. Gunshot wounds of the head and face generally terminate fatally; but if no very serious lesion of the brain occurs, much may be done to save life in the treatment. I have noted one case where a man was shot in the head: the ball entered the left eye and passed into the brain from below upwards; the patient was aged twenty-one years, of good general health, and of abstemious habits. He was found apparently in articulo mortis; great hæmorrhage had taken place, his face and hair being covered with blood. I washed away the blood, and gave him weak brandy and water in small quantities. He was unconscious, and his pulse was scarcely perceptible. After getting the stimulant, his pulse gradually got stronger, and he seemed more conscious, though not able to speak. In four hours I looked again at the wound: blood was oozing from it, and a substance resembling that of the brain itself. His pulse was now small and rapid, and he was quite unconscious and unable to swallow. He had croton oil dropped on his tongue; his head was shaved; cold water was kept trickling over; the stimulant was stopped. The eye was quite destroyed, and was removed on the second day; after which time he seemed to improve, but still remained unconscious. Soup was administered by the stomach-pump every day. The only topical application was lint and cold water. It is needless to follow out this case. The patient gradually recovered: the bullet being somewhere in the brain, having no doubt become encysted. The only remarkable thing that occurred in this case was, that paralysis of the right side of the body occurred, the opposite to that on which the ball entered. His general health is good, and he has never had convulsions or epilepsy.

Gunshot wounds of the cavities of the body are always dangerous, but not always fatal. In one case of a soldier the ball passed through the right side of the chest, fracturing one rib, and passing through the lung out at the back near the spinal column. The same man was shot in the left breast, the ball not passing out. This was a serious case. There was great cough, with expectoration of blood in large quantities; bloody froth bubbled out from both wounds; there was also much difficulty of breathing. The right side of the chest was fixed with broad strips of plaster, passed round from over the spinous processes of the vertebræ to the front part of the chest, leaving the wounds exposed: this side of the chest was thus prevented from moving, and gave the lung rest. The opposite side was merely covered with bandages. The patient was supported in a sitting posture. Cold water and lint were applied to the wound. He had muriate of morphia at night to give sleep, and his strength was kept up by nourishing soups. Purgatives were administered when required, and small doses of antimony, in the form of James's powder, were also given along with an expectorant mixture to allay cough. Three weeks after being wounded, and when going on well, he had a violent attack of congestion of the left lung, which was relieved by cupping. This man is now tolerably well, and is invalided from the service. In another case, where the left lung was wounded by the stab of a bayonet, and where the same plan of fixing the chest by plaster along with cold-water applications was adopted, complete recovery followed.

Much attention must necessarily be given to position in the treatment of gunshot wounds. The head must be kept raised in wounds of the thorax, so as to relieve the breathing. Fixing the chest by means of the adhesive plaster gives great relief to the breathing, and, as I know from experience, changes the condition from torture and extreme agony to comparative comfort.

In the limbs, wounds of the arteries and large nerves always require amputation; but when the artery alone is wounded, the only operation necessary is to cut down on and tie it. The treatment of course being,—proper position of limb, complete rest, and cooling applications, such as the water dressing.

When a ball passes through the carpus or tarsus, even though much smashing of the bone takes place without lesion of an artery, the member may be saved. In five instances of such wounds, three in the carpus and two in the tarsus, both hands and feet were saved by adopting the antiphlogistic plan of treatment with water dressing, and the removal of dead bone when it is loosened and acting as a foreign body. Occasional poultices are useful in cases of this sort so as to favour suppuration, being careful to keep them clean and frequently changed. The use of poultices in gunshot wounds is generally to be avoided. They are superseded in their effects by lint and warm water kept constantly trickling over the wound, and only induce putrefaction of the purulent discharge.

When bleeding takes place from a small artery, it may be stopped by the pressure of a bandage, which always becomes necessary when there is œdema of the part. In much smashing of the small bones, bandage and splint will, in a measure, maintain the shape of the foot or hand, which would otherwise become distorted. I have been induced to adopt as my treatment the topical applications of cold water, solely on observations among the aboriginal natives of New Zealand, where from time immemorial they always carry their wounded to the edge of some stream, so that a constant supply of water may be at hand; and in the case of fractured bones, after putting them up, they pour water constantly over the injured part, and many have been the cures that have been thus effected.

Much depends on fresh air as well as fresh water; however, the former is now always supplied in our hospitals, but the latter not so liberally as it should be in surgical cases.

Many and various have been the instruments constructed for extracting bullets. All sorts of forceps and with as many improvements and varieties as weeks in the year nearly; but I am sure all will concur with me, that with the common bullet-forceps any bullet may be extracted, and the only instruments needful for the field are tourniquets, with pad-strap and buckle, scalpel and forceps, with curved needle and silk thread, lint, adhesive plaster, and bandages. By the plan of treatment I have advocated I do not doubt that I have witnessed cures effected where death would have resulted under any other. The plentiful and constant applications of cold water to wounds (either gunshot or by the surgeon's knife) will heal them in a much shorter time and with less discomfort to the patient than any other treatment. Every surgeon has his own favourite plan of treatment, but none will ever supersede the cold water.

It is said that the minie conical ball makes a more severe wound than the round, but not if it revolves in its long axis, as it then pierces the tissues, not tearing them: the least obstruction to its course will, however, alter its motion; and if it then revolves on its short axis, of course, by its greater length, will create a larger wound than the spherical ball.

Erysipelas is a frequent result of gunshot wound, and generally of a severe character, causing much sloughing in and around the soft parts, difficult to subdue and always to be dreaded when in a severe form. It often supervenes in cases where the shock has been great, and where a large amount of stimulants have been administered. In such cases where it might be feared that erysipelas would set in, I have given potassio-tartarate of antimony in small doses, as soon as the shock subsided and reaction set in, thus anticipating the attack, if I may so say, and in no case in which it was administered have I seen erysipelas; but, when it does show itself, the usual means of treatment must be adopted, and changing the form of local applications from cold water to warm. I have never seen a case of gunshot wound in which erysipelas set in, where the shock of the system had not previously been severe, though in two instances the wounds were slight. After amputation, acute bronchitis is to be feared, causing much danger to the patient. In seven cases of secondary and four of primary amputation, I saw acute bronchitis attack the patients, generally commencing about the fifth day after operating, for which no reason could be assigned. The season of the year was in summer with fine weather, and the patients had every care taken of them previously. An attack of acute bronchitis supervening on an amputation or severe gunshot wound is of an aggravated type, and seems less amenable to ordinary treatment than the usual bronchitic affections, and as the purulent discharge from the wound is considerably diminished in quantity, when the first symptoms of the affection show themselves, and is through the whole course of the disease abnormally small in quantity, so, on the return of the pus in increased quantity, the bronchitis gradually disappears, and the wound assumes a more healthy aspect. This occurrence of bronchitis after amputation has been frequently remarked by authors, but I do not know of any cause for it being assigned by them, or any notice taken of the almost total disappearance of the purulent discharge during the attack. The ordinary remedies have been given for the treatment of it; but, till the reappearance of the pus, little benefit seemed to be derived from the usual plan of treatment. Pyæmia is a frequent reuslt of gunshot wounds, but it may often be avoided if proper precautions be taken. Tetanus is more frequently seen now than pyæmia; and for its cure most of the articles in the materia medica have at some time or other been proposed, but none seems of so much use as the continued use of opiates and chloroform.

In the course of treatment in gunshot wounds, diarrhœa and dysentery are very frequently most annoying to the patient, and reduce the strength very rapidly. In these cases I have found nothing so speedily beneficial as powdered charcoal in one-ounce doses, five or six times a-day, and it has invariably checked the diarrhœa. Through the whole course of treatment, I have found that the tincture of the muriate of iron in small doses, twice daily, has been of much service, and has changed the appearance of the wound in a few days after amputation, when of an unhealthy aspect, to a fine healthy look, where the consistence of pus changed materially. I have given it in thirteen cases of gunshot wounds, from the seventh day for a period of about a month or six weeks, with the most satisfactory results. Of styptics in gunshot wounds the best is tincture of matico poured on lint and passed into the wound, when it has acted as a charm in allaying hæmorrhage, and should always be in the pocket of the surgeon when in the field; but styptics are generally to be avoided, as they seem to induce attacks of erysipelas, or at least favour its production.

## ARTICLE IV.—*Statistical Report of One Thousand Midwifery Cases.* By S. LAWRENCE, M.D., Montrose.

(*Concluded from page* 724.)

### IV. LABOUR WITH COMPLICATIONS.

The total number of cases of labour with complications was 21. The nature of such complications, and the proportion in which they occurred among primiparous and non-primiparous cases, is shown as follows:—

| | Primiparous. | Non-Primiparous. | Total. |
|---|---|---|---|
| Puerperal convulsions, . | 3 | 0 | 3 |
| Ante-partum hæmorrhage, . | 2 | 5 | 7 |
| Post-partum do., . . | 0 | 1 | 1 |
| Retained placenta, . . | 3 | 4 | 7 |
| Syncope after delivery, . | 0 | 2 | 2 |
| Monstrosity, . | 0 | 1 | 1 |
| | 8 | 13 | 21 |

Taken in the aggregate, therefore, the relative frequency of complications, first, to the entire series, and, secondly, to primiparous and non-primiparous cases respectively, stands as follows:—

| | |
|---|---|
| Complications, total number, . . | 21 to 1000 or 1 to 47·6 |
| Do. in Primiparous cases, . . | 8 to 220 or 1 to 27·5 |
| Do. in Non-primiparous do., . . | 13 to 780 or 1 to 60·0 |

Judging, then, from these figures, complications of one kind or other are of more than twice the frequency in first than they are in subsequent labours.

Let us now specify such complications *seriatim*, in the order in which they are above set down.

A. *Puerperal Convulsions.*—There were three cases of this complication, which to the 1000 labours gives a proportion of 1 to 333·3—; but as they were all primiparous, the proportion to this division of the series is 3 to 220 or 1 to 73·3. It may not be uninteresting to subjoin particulars of my three cases which fortunately all terminated successfully.

CASE 1.—At 4.30 A.M., I was called to Mrs ——, in labour with her first child. The pains had commenced about an hour before, and a few minutes before I was sent for, she was seized while in the sitting posture with a severe fit of convulsion. Between the time of my arrival and 7 o'clock A.M., she had six or seven fits of a severe character. During the intervals she was in a state of constant jactitation, with consciousness imperfect. The os uteri at this time was dilated to about the size of a half-crown piece. I bled her on my arrival, and administered two drops of croton oil. Left her at 8 A.M., and was called again at 11. She had had another paroxysm, which was over when I arrived. Gave two grains of tart. ant., and other two within the next hour. No vomiting ensued, nor any perceptible nausea. At 11·30, another severe fit occurred, followed by deep coma. Os uteri still but very partially opened. Between 12 and 1 there were two or three fits, followed by occasional loud stertor in breathing. Between 1 and 2 o'clock, there were other two severe paroxysms. (On testing her urine albumen was found present in considerable quantity). A turpentine enema was ordered to be administered. Dr Johnston had seen her twice with me up to this period, but from the rigid and undilated state of the os uteri, delivery was found to be impracticable. I left her at 2 o'clock, and was again called at 3, when I found the pains regular and the head descending. I was now in hopes that labour would be terminated by the natural powers, but another severe fit occurring, I at once applied the forceps and extracted a *living* male child. Having ascertained the presence of another in the uterus, I ruptured the membranes, and as soon as the head descended I again applied the forceps and extracted. The face was turned to pubis. The pulse after delivery was 130, and small. In a few minutes after the delivery of the placenta, the patient had another violent convulsion. Breathing then became stertorous, and now and then very hurried. Pulse rose to 140, and was very weak. I ordered head to be shaved, and cold to be assiduously applied over its surface. 8 P.M.—Up to this hour there had been only one more fit, and there had been fewer accessions of hurried breathing. She lies tranquil, but quite unconscious. Pulse 140, and small. 2d Day, 7 A.M.—Passed a quiet night. No return of fits nor hurried breathing. Had a very copious dejection during the night. Is still unconscious, but lies as if in a tranquil and natural sleep. Pulse is down to 110, and is of better strength. A little light nourishment and wine had been given from time to time and swallowed. 9 P.M.—Still unconscious, otherwise as in the morning. 3d Day.—At four this morning consciousness partially returned, *i.e.*, 36 hours after the birth of the children. There is still (11 A.M.) a degree of confusion about her. Pulse 100. Evening.—Has kept conscious all day. Complains of a little pain in the head and belly. 4th Day.—Much pain in head and belly. Ordered turpentine stupe to the latter, and doses of calomel and opium. 5th Day.—Still pain in head and belly. Bled her from the arm. 6th.—Symptoms as yesterday. Pulse 130. Continue calomel and opium with mixed digitalis and tart. ant. emp. lyttæ nuchæ. After this she began gradually to amend, and at the end of four weeks from her confinement she was again in the enjoyment of almost her usual health. It may be questioned which of the two events, the birth of living children, or the ultimate recovery of the patient, was the more remarkable in this peculiarly severe case.

CASE 2.—This is briefly referred to under the head of forceps cases, as one of those in which the child was born dead. The patient had been seized with convulsions about an hour before I was sent for, and they were recurring every twenty minutes. The head was in the natural position, and descending. Having administered chloroform freely, I applied the forceps and delivered. She remained comatose an hour or two after, but recovery advanced favourably and speedily. Venesection with stimulating enemata were the principal measures employed for the removal of the cerebral oppression remaining after the convulsions.

CASE 3.—In this case the convulsions occurred some hours after delivery, the labour having been natural, and, for a first case, remarkably easy. I had been visiting this patient for some weeks previous to her confinement, and found her having œdematous feet and ankles with highly albuminous urine. By the usual remedies these symptoms were much reduced, but not entirely removed. There was no œdema of the face and hands, which by some is regarded as more decidedly premonitory of eclampsia than that of the lower extremities; neither was there vertigo, tinnitus aurium, or any other symptoms referable to the head. She was delivered at 1.15 A.M., and I left her in a comfortable state an hour after, and in hopes that notwithstanding her previous symptoms, her labour having been got so favourably over, her recovery would progress satisfactorily. At 6.45, I was hastily recalled, being informed by the messenger that my patient was speechless and insensible. The nurse had been the first to observe a twitching motion of the muscles of the face, with a slight jerking of the head. On my arrival she had regained consciousness, and could answer questions. She complained of a little fulness and pain in the head, and by-and-by she began to be occasionally incoherent. I had her placed in the sitting posture in bed, and immediately bled from the arm to the extent of from 12 to 15 ounces, when a decided impression was produced on the pulse. In 15 or 20 minutes after, and while I was still in the house, a very violent paroxysm occurred. Shortly after there was another of equal violence; and after each of these, perfect consciousness returned in about 10 minutes. The pulse before V. S. was 76, and oppressed; after the paroxysm it rose to 130, and remained so for fully two hours. I left her at 8.45, having first administered a pretty strong purge. At 11.30 visited her: there had been no return of convulsions. She was calm and conscious; pulse 84. In the evening the pulse was 76. She was quite calm and collected, and free from headache. There had been no return of paroxysms. Already the œdema of the feet and legs had greatly disappeared. From this time forward recovery advanced in the most satisfactory manner.—Moreau's experience leads him to believe that *advanced age*, the patients being primiparous, predisposes strongly to eclampsia. The cases now recorded, as already stated, were all primiparous, but the patients were all young. The first was about 25, and neither of the other two exceeding 20 years of age. There was nothing in the *physique* of either fitted to suggest the likelihood of such a complication. Indeed one of them, the third, was pale, thin, and peculiarly delicate-looking. Various authors have remarked on the proclivity to peritoneal inflammation after eclampsia; and this connexion seems to have been especially met with in the Dublin Hospital. In the first of the cases now recorded, the tendency to such a sequence existed very strongly; but in neither of the remaining two was it in the least degree manifested.[1]

B. *Ante-partum Hæmorrhage.*—There were seven cases of this complication, five occurring at the full time and two before it. Of the latter one was in the fifth month, the other in the eighth. As has been already stated, two of the cases were primiparous and five non-primiparous. The proportions, therefore, are as follow:—

| | | | |
|---|---|---|---|
| Ante-partum hæmorrhage, . | 7 cases | to 1000, | or 1 to 142·85 |
| Primiparous cases of do., . | 2 „ | to 220, | or 1 to 110 |
| Non-primiparous cases of do., . | 5 „ | to 780, | or 1 to 156 |

The relative frequency of this complication, therefore, in first and subsequent cases, has been as 1·4 to 1.

In these seven cases, all the mothers, although some of them were placed in much jeopardy, recovered. In only one of the cases was the child born alive, and in that case the flooding occurred not prior to, but during labour, at the commencement of the second stage. As the labour advanced it ceased; and although it produced for a time a considerable impression on the mother, the child was born alive and vigorous. In every one of the six cases in which the hæmorrhage took place prior to labour, the child was born dead. In four of these the placenta was found either partially over or near to the os uteri; in two of them its position could not be felt. In two of these six cases, delivery was accomplished by manual efforts,—one by turning, the case being further complicated by protrusion of the funis, the other by extraction by the feet, the presentation being breech. In the other four, as labour was proceeding quickly, no interference was employed beyond rupturing the membranes.—Ante-partum hæmorrhage is justly dwelt upon by all writers on midwifery as one of the most alarming and formidable complications to be met with in obstetric practice. Happily, timely and appropriate aid by means now well-understood throughout the profession will, in a large proportion of cases, save the more valuable life of the mother; but my experience leads decidedly to the conclusion, that no complication will more certainly effect the death of the child, no matter with what promptitude our best-directed efforts may be employed. Indeed, I believe it will usually be found that this event has happened either before labour has commenced, or before it has reached that stage at which we can interfere to expedite delivery.

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[1] The cases above given, possessing as they do distinctive features, may be regarded as illustrative of some of the varied forms which this formidable and dangerous disease presents. My limited space has compelled me to restrict myself to the practical part of the subject. Those who wish to see its etiology, viewed in the light of modern science, and its general and special relations fully discussed, would do well to study "Braun on Puerperal Convulsions," with the valuable foot-notes appended by Dr Matthews Duncan, to whom the profession in this country are under much obligation for rendering this important treatise into the English tongue.

C. *Post-partum Hæmorrhage and Retained Placenta.*—Take the latter first. Of retained placenta there were seven cases, three being primiparous, and four non-primiparous.

| | |
|---|---|
| Total, . . . . . | 7 to 1000, or 1 to 142·85 |
| Primiparous, . . . | 3 to 220, or 1 to 73·3 |
| Non-primiparous, . . | 4 to 780, or 1 to 195 |

The relative frequency of this complication in primiparous and non-primiparous cases being thus as 2·67 to 1. In six of the seven cases the retention was owing to the inertia of the uterus; in the seventh to hour-glass contraction. Pressure and friction over the uterus having failed to induce the requisite expulsive action, the hand was introduced and the placenta removed in the usual way. In one of these cases, the retention was after abortion in the 5th month, and the removal of the placenta was effected with considerable difficulty by two fingers introduced through the os uteri. In the case of retention from hour-glass contraction, the placenta was extremely friable, and broke down in the act of removing it. Some small portions were thus left behind, which it was hoped would be afterward expelled by the natural efforts of the uterus. The patient was much exhausted, and any further fatigue or annoyance by additional attempts at extraction seemed to be inexpedient. In the light shed upon it by the after-history of the case, I am now satisfied that this practice was wrong. Irritative fever and other symptoms, having all the characters of uterine phlebitis, were soon established, and the patient succumbed on the eleventh day. I have known of two very similar cases in the practice of my professional brethren, each having a like fatal result. Whether in cases of abortion, such partial placental retention be as innocuous as was represented by certain speakers at a late meeting of the Edinburgh Obstetrical Society (*See* Med. Journal for Nov. 1862), I will not presume to say. It may be so, and that the fact finds its explanation in the mouths of the uterine sinuses (from being so much smaller at that early period) being entirely occluded before the putrefactive process in the retained placenta has made any progress. Absorption being thus prevented, the decomposing mass would necessarily pass harmlessly off by the natural passages. But for any one to apply such a doctrine to partial retention of the placenta, *after labour at the full time*, would be, I feel assured, to commit a very grave mistake. Of the imminent danger to the patient from such retention, I for one have a profound impression; and consequently of the urgent necessity of adopting measures to remove it. What seems to me the best method of procedure in such cases is this:—In the first place, if possible, let every portion of the broken placenta be removed at once, using, of course, due precaution to guard against injury to the intra-uterine surface in the manipulations requisite for the purpose. If there is much difficulty or delay in effecting the removal of all the broken portions, it is not unlikely that both the patient and her friends will protest against the continued attempt; and there is more or less risk that the practitioner will, on this account, even against his better judgment, be induced to act on the *laissez-faire* principle, much to his after regret. But it ought ever to be remembered, that the present comfort and feelings of the patient must be subordinated to her future safety. Secondly, If the exhaustion is so great as to render continued attempts at the removal of the remaining pieces imprudent, and if no hæmorrhage is present, then let such attempts be suspended in the meantime, and stimulants administered to effect a rallying of the vital powers. If no extrusion of the retained portions follows, a dose of the ergot of rye may be administered; and supposing some hours now to have elapsed, and the desired result still unaccomplished, then I would recommend that the patient should be fully chloroformed, and the hand re-introduced into the uterus, and the leisurely and complete removal of every portion of placenta lying in its cavity effected. Nothing less than this, I feel assured, will meet the emergency. I have had an opportunity of seeing this method once followed, in the practice of one of my brethren, in a case in which I was called in to render assistance. The removal of the retained portions of placenta was in this way satisfactorily accomplished twelve hours after delivery, and the recovery of the patient afterward was all that could be desired.

*Post-partum Hæmorrhage.*—Of this complication there occurred but *one* case (in a non-primiparous labour). It took place after expulsion of the placenta, and seemed to arise from atony of the uterus. The presentation had been face to pubis, and partly from this cause, and partly from inefficient pains, the labour had been somewhat tedious. The hæmorrhage, which was considerable, was arrested by pressure and cold. A word or two with reference to retained placenta and post-partum hæmorrhage taken conjointly (for they are very closely allied), may not be inappropriate. In our systematic works on midwifery we have ample and admirable directions how to *treat* such complications, but very little instruction as to how to *prevent* them. Now, considering the danger they occasion to the patient, and the corresponding anxiety to the attendant, any method of procedure, by which their frequency may be diminished, surely claims our serious notice. From a pretty considerable experience, my belief is that the risk of their occurrence would be immensely lessened by a very simple expedient which I invariably adopt. After the birth and separation of the child, let the funis be laid hold of with the right hand, and maintained tense (without, however, any *dragging*), and let the left hand at the same time grasp, through the abdominal parietes, the fundus and body of the uterus, and thus keep up moderate but steady pressure until the placenta is expelled. In nine cases out of ten such expulsion will happen in less than fifteen minutes if this plan is followed. One of the rules given by the late Dr Burns for the management of the placenta is, that "the practitioner should never leave the bedroom until it is expelled." I go farther than this, and would say he should never leave the *bedside* until its extrusion is accomplished, nor allow his attention to be diverted for a moment by aught else until this essential condition in his patient's safety is attained. Any rude and painful efforts to bring about the removal of the after-birth are, of course, to be strongly deprecated. But such are quite uncalled for: the simple measures above commended will almost invariably secure the desired result in a short space of time. Viewing the two complications we are now considering in the light of my own experience, and the recorded experience of others, I would be inclined to regard it as a law—*That the risk both of post-partum hæmorrhage and of retained placenta, whether from inertia or irregular contraction, is directly as the time which elapses between the birth of the child and the expulsion of the placenta; and that any line of practice, otherwise unobjectionable, tending to secure the extrusion of the after-birth within ten or fifteen minutes from the time of the woman's delivery, will reduce to a minimum the chance of such contingencies.* It may serve to illustrate and confirm this proposition if I place side by side my own numerical experience of those complications, and that recorded by Drs Hardy and M'Clintock as the experience of the Dublin Hospital. By their report we find that they had 56 cases of post-partum hæmorrhage to 6634 deliveries, and 28 cases of retained placenta. If from the latter we deduct 11 cases of retention due to morbid adhesion (which, of course, no treatment could prevent), we shall then have 73 as the total number of times they met with these two complications. This shows that they met with one or other of these two complications once in every 90·8 cases. Of retained placenta, as above stated, I had 7 cases; but, in instituting the present comparison, I am entitled to deduct two of these as having happened in the fifth and sixth month respectively, seeing that Drs Hardy and M'Clintock do not record such early cases. Five cases, then, of retained placenta, and one of post-partum hæmorrhage, give a total of six as the number of times I encountered these two contingencies at the full period of utero-gestation,—that is to say, I met with one or other of them once in 166·6 cases, as against their 90·8. Very probably some of the cases which I have entered in my list as "retained placenta" might not have found their way into that category in the practice of others, inasmuch as they might possibly have been terminated by the natural powers had I chosen to wait the usually specified time. Dr Burns and Dr Lee recommend waiting an hour before any aid at extraction is given; and Drs Hardy and M'Clintock appear to have frequently delayed twice this time before using any means for its removal. For my own part, fully impressed with the truth of Dr Burns' aphorism, that "so long as the placenta is retained the patient is never free from the risk of flooding," I have on no occasion delayed beyond forty minutes in using gentle manual interference, if this organ was not already expelled by the natural powers. As to the bad results alleged to accrue, at least in hospital practice, from the introduction of the hand for the removal of the placenta, I can only say I have never seen them, nor do I think they are at all likely to occur where due care and gentleness are exercised. On the contrary, that, when cautiously performed, such removal is greatly to be preferred to the delay enjoined by many high authorities, I do not entertain the slightest doubt. In the practice of the Dublin Hospital, post-partum hæmorrhage occurred, as above stated, 56 times, or once in every 118·46 cases; and in ten of these cases the reporters tell us "the flooding was so severe as to excite apprehensions for the patient's safety." In my own practice this casualty happened only once in a thousand cases, and by no means to such an extent as to threaten danger to the patient. Among the measures specially recommended by Drs Hardy and M'Clintock for the prevention of post-partum hæmorrhage in those cases where a predisposition to it is known to exist, while they speak of pressure over the uterus *before* the birth of the child—viz., in "the second stage of labour"—the employment of such pressure after the birth is not once mentioned. How far the adoption of such a mode of practice, and an earlier removal of the placenta, would have reduced their high average of cases of post-partum hæmorrhage may be fairly judged of after what has been stated above.

D. *Syncope following Delivery.*—Very alarming cases of fainting, sometimes even ending fatally, are reported by various authors as having occurred during the progress of labour. I have never met with this complication, but have twice had to combat a similar symptom subsequent to the birth of the child. In both cases the state of extreme faintness was out of all proportion to the amount of discharge, which did not exceed what is usually borne after delivery, without any marked general impression. Two such cases to the entire series is 1 to 500; and as they were both non-primiparous, we have thus 1 to 390 of such labours. An extreme aptitude to be affected by even a very moderate sanguineous loss, following the nervous exhaustion of labour, was in each case the only mode of accounting for the phenomenon. Cold air, stimulants so soon as they could be swallowed, and due pressure over the uterus, were the means employed, and successfully, to effect restoration.

E. *Anencephalous Monster.*—Whether a case of this kind be regarded as a variety of natural labour, as a preternatural presentation, or as labour with complication, is probably of small moment. I have placed it in the latter category, for this reason, that such a condition in the head is not only at first very perplexing to the attendant in his endeavour to make out the presentation, but also—

where the child is full-grown—that it greatly increases the difficulty of delivery. We can easily understand how it should have this effect, when we consider that in such an abnormal state the oblate-ovoid form of the fœtal head, on which the easy dilatation of the maternal passages is so dependant, is lost. In my case the mother had previously born several well-formed and healthy children. On my first examination I felt it impossible to assure myself of the presentation; and it was only after feeling first the one ear and then the other, that I could make out distinctly that the child was anencephalous. The parturient pains were powerful, but the progress of delivery, until the birth of the head, was slow, doubtless for the reason above indicated. Both parietal bones, the greater part of the frontal and part of the occipital, were awanting, and the vertex was constituted by a firm fleshy mass, with a deep sulcus in its centre. The body of the child was well-developed. It was alive at the commencement of labour, but perished during delivery, —from what cause was not apparent. One thing is *certain*—its death was *not* due to cerebral compression!

## V. Twin Cases.

Of such cases there occurred seventeen. Three were primiparous, and fourteen non-primiparous. We thus obtain the following proportions:—

| | |
|---|---|
| 17 Twin cases . . . . . . | to 1000, or 1 to 58·8 |
| 3 do., primiparous, . . . . | to 220, or 1 to 73·3 |
| 14 do., non-primiparous, . . . | to 780, or 1 to 55·7 |

The relative frequency, therefore, of primiparous twin cases to non-primiparous was as 1 to 1·3.

Two of the 3 primiparous cases were premature, and one of the 14 non-primiparous cases. The principal facts of the 17 cases, as elicited by an examination of them in detail, are the following:—1*st*, The presentations. In 7 cases both children presented naturally; in 10, one of the children had a preternatural presentation. In these 10 cases, with but one exception, it was the *second* child that had the preternatural presentation; in the exceptional case, the first child had the face to pubis. Analyzing the presentations of the 34 children born in these 17 cases, we have the following results:—24 presenting naturally; 3 footling; 3 breech; and 4 face to pubis. 2*d*, Interval between the birth of the co-twins. The shortest interval between the birth of the children was 5 minutes; the longest, 3½ hours. In 14 out of the 17 cases, the interval was less than half an hour. 3*d*, Placenta, one or two? In 11 cases there was one double placenta; in 6 cases there were two separate placentæ. 4*th*, Sex of children. Of the 34 children 20 were males and 14 females. In 11 of the cases the co-twins were of the same sex; in 6 of opposite sexes. In each of the 3 primiparous cases the co-twins were males. 5*th*, Size of children. In the majority of the cases there was a marked difference of size between the two children. In some of these cases the larger child was born first, in others the smaller took the lead,—prophetic probably of their relative position in after-life! 6*th*, Results (*a*) to mothers. All the mothers recovered quite as favourably as after ordinary labour. (*b*) To children. All the children were born alive, with the exception of one, in one of the two premature primiparous cases. It had been dead some time previous to birth.

*Table showing the Principal Facts in the Twin Cases.*

| CASES. | Primiparous, or Non-primiparous. | Period of Pregnancy. | Sex of Children. | | Presentations. | | Interval between Births. | Placentæ, single or double. |
|---|---|---|---|---|---|---|---|---|
| | | | 1st. | 2d. | 1st. | 2d. | | |
| No. 1. | Non-primip. | 9th mo. | F. | M. | Nat. | Feet. | 10 mins. | Double. |
| „ 2. | Non-primip. | 9th mo. | F. | M. | Nat. | Nat. | 3½ hours. | Two sing. |
| „ 3. | Non-primip. | 9th mo. | F. | F. | Face to P. | Nat. | 5 mins. | Double. |
| „ 4. | Non-primip. | 9th mo. | M. | M. | Nat. | Nat. | 15 mins. | Double. |
| „ 5. | Non-primip. | 9th mo. | F. | F. | Nat. | Nat. | 8 mins. | Double. |
| „ 6. | Non-primip. | 9th mo. | M. | F. | Nat. | Breech. | 10 mins. | Double. |
| „ 7. | Non-primip. | 9th mo. | M. | M. | Nat. | Face to P. | 20 mins. | Double. |
| „ 8. | Non-primip. | 9th mo. | F. | F. | Nat. | Nat. | 5 mins. | Two sing. |
| „ 9. | Primipar. | 9th mo. | M. | M. | Nat. | Face to P. | 20 mins. | Double. |
| „ 10. | Non-primip. | 9th mo. | F. | F. | Nat. | Nat. | 15 mins. | Double. |
| „ 11. | Primipar. | 7th mo. | M. | M. | Nat. | Nat. | 5 mins. | Two sing. |
| „ 12. | Non-primip. | 9th mo. | M. | M. | Nat. | Breech. | 35 mins. | Double. |
| „ 13. | Non-primip. | 8th mo. | M. | M. | Nat. | Feet. | 15 mins. | Two sing. |
| „ 14. | Non-primip. | 9th mo. | M. | F. | Nat. | Face to P. | 1 h. & 10 m. | Two sing. |
| „ 15. | Non-primip. | 9th mo. | F. | M. | Nat. | Nat. | 20 mins. | Double. |
| „ 16. | Non-primip. | 9th mo. | M. | F. | Nat. | Breech. | 7 mins. | Two sing. |
| „ 17. | Primipar. | 8th mo. | M. | M. | Nat. | Feet. | 5 mins. | Double. |

## VI. MORTALITY TO MOTHERS.

Five mothers perished after childbirth—one being a primiparous case, four non-primiparous.

| | | |
|---|---|---|
| | 5 to 1000 | is 1 to 200 |
| Primiparous, . . . . . . | 1 | is 1 to 220 |
| Non-primiparous, . . . . . . | 4 to 780 | is 1 to 195 |

The maternal mortality, therefore, in these two classes respectively was as 1·12 to 1. As I had no opportunity of making a post-mortem examination in any of these fatal cases, I need not minutely detail their history. To two of them I have already adverted (*see ante*, Forceps cases and Retained Placenta). The other three were cases of puerperal peritonitis,—two of which occurred in previously unhealthy subjects, the one after a premature confinement in the seventh month, the other after a natural, though rather tedious labour. The third was in the case of a very healthy woman after her second confinement, which was natural and easy. Owing to a decided imprudence on the part of a friend in attendance, she had a violent rigor on the evening of the second day. Peritonitis of a very severe character was speedily developed, and, in spite of every means employed to arrest it, it proved fatal on the sixth day.

## VII. CHILDREN BORN DEAD.

The total number of dead-born children was 45. Of these 20 were primiparous, and 25 non-primiparous.

| | |
|---|---|
| Total, . . . . . . . . | 45 to 1000 is 1 to 22·2 |
| Primiparous, . . . . . | 20 to 220 is 1 to 11 |
| Non-primiparous, . . . . | 25 to 780 is 1 to 31·2 |

I have included in the above list not only children which had reached the completion of the ninth month, but all those born dead from the fifth month onward. This, no doubt, augments the number of such cases, and increases the ratio which they bear to the total number of children born; but any fair and satisfactory discussion of the subject seemed to me to demand the course I have adopted. In our systematic treatises on midwifery, in our obstetric reports, and, for the most part, throughout our periodical medical literature, the subject of fœtal death-rates appears to me to receive but very cursory notice, which is all the more remarkable, seeing that the question of death-rates generally, as affecting the wellbeing of society, now attracts so large a share, not only of professional, but of public attention. And yet the number of children which in any large community die *in utero*, or perish during delivery, even in the course of a single year, is by no means small; and the cause or causes of such mortality, and the means of its prevention or abridgment, is surely as worthy the consideration of the pathologist and obstetrician, as the diminution of the general death-rate in any locality is worthy the labours of the sanitary reformer. Human life is valuable at any and every period of its course; and on this ground alone it behoves us, as philanthropists, to discover, and, if possible, remove the perils which menace it during the intra-uterine stage of its career, as well as those which endanger its existence and wellbeing subsequently. But there are other and not unimportant reasons why such an inquiry should occupy our attention. No medical man can have been long engaged in the duties of his calling without observing, and, I may add, sharing, the disappointment and sorrow which blighted pregnancies, and especially dead births at the full time, occasion to families; nor can he witness such unhappy events without feeling the obligation imposed on him, by the duty which he owes both to his patients and his profession, to endeavour, if possible, to prevent their recurrence. On him seems to rest the responsibility, that what has proved an occasion of grief in the past, shall in the future prove, what it legitimately ought, an occasion of gladness. Moreover, it is almost superfluous to add that not only the comfort and happiness of families generally, but in an especial manner the health and future wellbeing of the mother, are most intimately bound up with the fate of her pregnancy. In a variety of ways, which it is needless to specify, premature labours, or otherwise unfortunate pregnancies, operate most injuriously on the maternal health; and this alone would constitute a sufficient reason why we should strive to prevent them. I think it will not be unprofitable, therefore, to examine this important question of fœtal mortality in the light of the 45 cases which have occurred in my own experience. Let me first enumerate the facts brought out by a careful examination of the list. 1*st* (as already stated), 20 were primiparous, and 25 non-primiparous. 2*d*, In 18 of the cases the children were premature; of which 18, 5 were primiparous, and 13 non-primiparous. Of the remaining 27 born at the full time, 15 were primiparous, and 12 non-primiparous. 3*d*, Of the 15 primiparous children which had reached maturity, 7 were dead before labour commenced, and 8 perished during labour; and of the 12 non-primiparous children which had reached maturity, and were born dead, 6 died before labour commenced, and 6 died during delivery. If we tabulate the facts now stated, we shall, perhaps, be better prepared for estimating their value.

*Table showing the proportion of Dead Births in Primiparous and Non-primiparous cases respectively.*

| PRIMIPAROUS. | | NON-PRIMIPAROUS. | |
|---|---|---|---|
| Total No. born dead | 20 to 220, or 1 to 11 | Total No. born dead | 25 to 780, or 1 to 31·2 |
| Premature, born dead | 5 to 220, or 1 to 44 | Premature, born dead | 13 to 780, or 1 to 60 |
| At full time, born dead, and dead before labour | 7 to 220, or 1 to 31·4 | At full time, born dead, and dead before labour | 6 to 780, or 1 to 130 |
| Died therefore during pregnancy | 12 to 220, or 1 to 18·3 | Died therefore during pregnancy | 19 to 780, or 1 to 41 |
| Died from accidents during labour | 8 to 220, or 1 to 27·5 | Died from accidents during labour | 6 to 780, or 1 to 130 |

The above table, we think, is instructive in several ways. 1*st*, It shows how very frequently dead births are associated with abortive and premature labours. This occurred eighteen times out of the 45 cases, or in two-fifths of the whole. In such cases it is often difficult, if not impossible, to say whether the death of the fœtus precedes, and therefore causes the cessation of pregnancy; or, *vice versa*, whether some influence, having acted directly on the mother to interrupt the process of utero-gestation, operates at the same time in extinguishing the life of the child. The former of these is doubtless by far the more frequent mode of causation; but the latter I believe to be by no means of rare occurrence. 2*d*, We have presented to us the important fact of the much greater frequency of dead births in primiparous than in non-primiparous cases.

In the former class they are as 1 to 11; in the latter, as 1 to 31·2. The risk of casualty to the child is thus seen to be nearly three times as much (2·8 to 1) in a first pregnancy and labour, as what it is in subsequent ones. 3*d*, The table also teaches us in what proportion the greater risk attaching to primiparous children pertains to pregnancy and to labour respectively. The entire number of primiparous children dead during pregnancy was 12, or in the proportion of 1 to 18·3; in non-primiparous cases the number of children dead during pregnancy was 19, or in the proportion of 1 to 41. The number of primiparous children dead during labour was 8, or 1 to 27·5; the number of non-primiparous children dead during labour was 6, or as 1 to 130. That is to say, judging from this table, the risk to children in first pregnancies is more than double what it is in succeeding pregnancies; and the risk to children in first labours is nearly fivefold what it is in subsequent labours.

If such, then, is the fact, we have to ask ourselves, first, how is it to be explained? and, secondly, can we do anything to prevent it? Let us consider this twofold question in regard (A) to first pregnancies, and (B) first labours, respectively.

A. *First Pregnancies.*—I am not aware of any *physiological* reason why first pregnancies should be more fatal to children than subsequent ones; or that there exists anything whatever in the circumstances of the case why such result should necessarily happen. *A priori*, one would rather expect that the fact should be otherwise. A woman during her first pregnancy, before she has been subjected to the debilitating influences incident to repeated confinements, nursings, and the management of infancy, might naturally be regarded as more likely to carry a living child to the full time than after such influences have come into play. But in my own experience quite the reverse has been seen to be the case; for the fœtal mortality in first pregnancies was more than double what it was in subsequent ones. This interesting fact must be referred, I think, to two causes, one accidental or mechanical, the other pathological. First, to the occurrence of accidents and misadventures (from over-exertion and otherwise) fitted to arrest the process of utero-gestation, and so extinguish the life of the fœtus—accidents and misadventures against which the mother, taught by experience, more effectually guards in her succeeding pregnancies. Secondly, and chiefly, to the operation on the fœtus through the maternal blood of pathological conditions, whose existence is only demonstrated after the birth of an immature and dead child. If such explanation is accepted as a correct reply to the question propounded, it will at once be apparent that it is comparatively little we can do to control the high infantile mortality pertaining to first pregnancies. And for this reason, that since no cause *apparently* exists in the condition or circumstances of the woman for resorting to medical advice during a first pregnancy, such advice is seldom or ever sought. In the majority of cases it is not until after the patient has had a premature labour and a dead-born child, that the discovery is made of circumstances in her health or habits, or in the health or habits of her husband, which have issued in such a result, and which, while unremoved, will tell with equally disastrous influence upon every succeeding pregnancy. In short, it is only in the light of the misfortune that is past, that we discern what contingencies lie in the future, and take means, accordingly, to avert them. No one can have perused with the care and attention to which its merits entitle it, the original work of "Diday on Infantile Syphilis," without being convinced of the vast extent to which fœtal life is poisoned at its source by the subtle influence of the venereal virus, even when the health of both parents is to all appearance quite unexceptionable. How often has it happened in the history of families that child after child has perished in the womb, and come forth immature and blighted, as if under some mysterious and fatal spell, and that all hopes of living progeny have thus been extinguished, until the proof or suspicion of a venereal taint, in either parent or both, has led to the adoption of the appropriate means of cure. Such successive unfortunate conceptions are now, it is believed, of much less frequent occurrence than aforetime, inasmuch as both their cause and cure are better understood. A first pregnancy, however, derives no advantage (or but rarely does so) from our modern and enlightened views of fœtal pathology, just because, being outwardly all prosperous and promising, it seems to need no aid from therapeutics, and therefore receives none. But when the *produce* of such pregnancy furnishes proof, or awakens suspicion, of the subtle and deadly influence which, all unseen, has been at work during its continuance, the requisite treatment is forthwith resorted to, and a similar unfortunate result from any succeeding pregnancy is happily prevented. While, therefore, we consider the *cause* of the comparative high rate of fœtal mortality pertaining to first pregnancies as by no means obscure, the removal or diminution of such excess of mortality, for the reasons now adduced, is, we fear, of far from easy attainment.

B. *First Labours.*—We have seen that first labours, as compared with subsequent ones, are peculiarly disastrous to children. How comes this to pass? In the first place, there is the very remarkable fact (which I do not profess to account for), that preternatural presentations, which always involve enhanced danger to the life of the child, are seen to be of a fifth more frequent occurrence in first than in subsequent labours. Secondly, Not only are such presentations proportionally more frequent in first cases, but they are also proportionally more fatal to the infant. For we find that in those presentations, viz., arm, footling, breech, and funis, which involve risk to the child from pressure on the cord, while in primiparous cases, five children out of six perished during labour, in the same class of presentations, in non-primiparous cases, only four children perished during labour out of fourteen. The rate of infantile mortality from this cause was therefore nearly three times greater in first than in subsequent labours. This striking disparity of result admits, we think, of only one explanation, namely, the greater rigidity of the maternal textures concerned in parturition in the one class of labours than what obtains in the other. Greater rigidity, of course, implies greater pressure; and greater pressure, greater danger to the child. We have thus, we conceive, an adequate explanation of the high ratio of infantile mortality from preternatural presentations in primiparous labours, as compared with what results from such presentations in non-primiparous labours. That there is such extra rigidity in first labours, with all the attendant difficulty in delivery, and consequent danger to the child which it involves, is, we think, indubitably proved, if we look to the frequency with which the forceps required to be employed in primiparous and non-primiparous cases respectively (*see* Forceps cases, *ante*); this instrument having been required upwards of eleven times more frequently in the former than in the latter. Thirdly, Not only is there an increased infantile mortality in primiparous cases, for the reason now assigned, where the presentation is preternatural, but even in *perfectly natural presentations* we find, in this class of labours, a similar result. For instance, while in my experience three primiparous children, presenting naturally, were born dead, having died from cerebral pressure in the course of delivery, there was only one non-primiparous child, having a natural presentation, that perished from this cause. In other words, the ratio of infantile mortality from pressure on the head of the child during labour was, in primiparous cases, 3 to 220, or 1 to 73·3, while in non-primiparous cases, it is 1 to 780, being thus between 10 and 11 times greater in the one class of labours than in the other Thus much for the etiology of the high infantile death-rate pertaining to first labours. Next comes the question, Is there any practical lesson deducible from the facts which have now been stated? Can anything be done to prevent or diminish this excess of infant deaths in primiparous labours? The answer we think is self-evident, following as a corollary from what has been advanced. It may be stated thus: *Pressure from the maternal structures on the head of the child being greater in first labours than in succeeding ones, and its continuance for a given period being necessarily attended with greater peril to the child,—moreover, the cause on which such greater pressure is dependant being, so far as we know, beyond our control; it follows that we ought to counteract this increased peril by abridging the time during which such pressure is allowed to be exerted.* In other words (and I have a profound impression of the truth and importance of this as a rule of practice), in all difficult or tedious cases we ought to employ the forceps much sooner in primiparous labours than in non-primiparous ones, if we would give our patient an equal chance of having a living child.

MONTROSE, *December* 1862.

## ARTICLE V.—*On the Calabar Bean as a new Agent in Ophthalmic Medicine.*

By D. ARGYLL ROBERTSON, M.D., F.R.C.S.E., Lecturer on Diseases of the Eye, Edinburgh.

(*The substance of this paper was read as a communication at a meeting of the Edinburgh Medico-Chirurgical Society, on 4th February* 1863.)

FOR more than a year past I have recognised the numerous advantages that would flow from the discovery of a substance which, when applied to the conjunctiva, should produce effects exactly opposite to those well known to result from belladonna or atropine; which should stimulate the muscle of accommodation and the sphincter pupillæ as the above-named remedies paralyze them. With the view of discovering such an agent, I endeavoured to ascertain from experiments of my own, and from the writings of previous observers, whether any of the common vegetable principles possessed this property. These investigations were, however, productive of no satisfactory results, until my friend Dr Fraser informed me that he had seen contraction of the pupil result from the local application of an extract of the ordeal bean of Calabar. I resolved to investigate the action of this substance, and, above all, to ascertain whether it exerted any influence on the accommodation of the eye. With some difficulty I got a few Calabar beans, and, with the view of obtaining their active principles in a convenient form, prepared from them three extracts of varying strengths in the following manner:—The weakest of the three was made by thoroughly digesting gr. xxx. of the powdered Calabar bean in alcohol, carefully evaporating the strained fluid to dryness, and then adding to the residue ʒj. of water. This formed a muddy solution of a light reddish brown tint. In strength, one minim of this solution corresponded to about half a grain of the bean. The second extract was formed by evaporating a portion of the first to about a quarter of its volume; so that one minim of this extract corresponded to about two grains of the bean. The third extract was the strongest, and was prepared like the first, except that the proportions differed; so that one minim of it in strength corresponded to four grains of the bean.

With these solutions, I then proceeded to perform the following experiments,—in which I had the able assistance of Dr Grainger Stewart,—with the intention of elucidating the exact effects the Calabar bean is capable of producing on the eye:—

*Experiment* 1.—On the 17th of January, I carefully examined the condition of my eyes, and found that with both my sight was normal. I could see distant objects perfectly distinctly, and could read the finest print (No. 1 of Jaeger), at five inches distance, with either eye. Each pupil measured two lines in diameter, and acted readily under the influence of light. These points having been determined, I introduced a drop of the weakest extract of the Calabar bean into my left eye, at 10 minutes past 12 o'clock. Its introduction occasioned no further irritation than a drop of simple water would produce.

No change was observed in the condition of the eyes until ten minutes thereafter, when, upon looking with the left eye, all objects beyond the distance of about a foot appeared dim and indistinct, but within that distance clear and well defined; while objects at all distances appeared nearer and larger than natural. Vision in the right eye remained unaltered. No difference was visible in the size of either of the pupils. A feeling of straining and heaviness was felt in the left eye similar to that experienced after a prolonged inspection of fine objects near the eye.

At 12.30, or 20 minutes after the introduction of the extract, a marked alteration in the size of the pupils was observable; the left pupil being only 1 line in diameter, while the right measured fully two lines. Looking with the left eye, all objects beyond nine inches from the eye appeared very dim, and at all distances everything seemed about a half larger than natural, and nearer to the eye. Vision in the right eye was unaffected.

At 12.40, the left pupil measured only ⅔ of a line, while that of the right eye had dilated to 2½ lines. The furthest point of distinct vision with the left eye was 8 inches from the eye. In other respects no alteration was observable since last report.

At 1.20, the pupils measured on the left side ½ a line on the right 2 lines. With the left eye objects could with some effort be seen distinctly to the distance of about 10 yards, but, as before, larger and darker than with the other eye. When reading with both eyes, a sensation of heaviness and fatigue was soon felt in the left. Vision in the right eye continued unaltered.

At 6 o'clock, the left pupil had dilated to 1 line, while the right had contracted to 1¾ line. Vision with the left was somewhat improved, and with an effort distant objects could be distinctly seen. Both pupils contracted under the influence of light, both singly and sympathetically. At 12 o'clock, the left pupil measured 1½ line, the right 2½ lines, and the same symptoms were present as at last report, only to a less marked degree. A dull aching and heaviness was experienced in the left eye consequent upon its exercise in reading and writing.

On the following morning, there was still an appreciable difference in the size of the pupils, and vision with the left eye was still slightly affected; but the symptoms gradually subsided, and in the afternoon completely disappeared.

The results obtained from this experiment were, that the Calabar bean acted first on the accommodation of the eye, causing indistinct vision of distant objects to such an extent that all objects beyond 8 inches from the eye appeared dim and indistinct. The next marked effect produced was contraction of the pupil, its diameter being reduced from 2 lines to ½ a line. As a natural consequence of less light passing to the retina of the eye experimented on, the pupil of the other eye became sympathetically somewhat dilated, while with the affected eye all objects appeared darker than natural. It is interesting to observe, that while the affection of the accommodation of the eye is the symptom first developed, it also is the first to subside, for we find that while the contraction of the pupil is at its maximum at 1.20, the affection of vision had already decidedly diminished. From the very small size of the pupil, it was very difficult to ascertain whether it acted under the influence of light or not. On the occasion of one of the reports (where it is mentioned), it was distinctly observed to do so.

*Experiment* 2.—Having ascertained that my eyes had returned to their normal condition, and that each pupil, as before, measured 2 lines, and acted under the influence of light, I again, on January 20th, introduced a drop of the weakest extract of the Calabar bean into my left eye at 8.30 A.M.

At 8.40 the pupils were unaffected. With the left eye objects beyond 9 inches appeared dim, enlarged, and nearer than with the other eye, and a concave glass, of 10 inches negative focus, was required to enable me to see distant objects distinctly. With this concave glass the nearest point at which I could see objects distinctly was 9 inches from the eye.

At 9.5 the left pupil measured ¾ of a line, the right 2 lines. To see distant objects clearly with the left eye, a concave glass of 8 inches negative focus was now required, and with this glass the "near-point" of distinct vision was 5 inches from the eye. Both pupils acted under the influence of light. Vision in the right eye was unaffected.

A drop of a solution of atropine (gr.ij to ℥i) was now introduced into the left eye.

At 9.30, the left pupil had sensibly increased in size, and measured 1 line, while the right measured 1¾ line. A concave glass of 12 inches negative focus was now sufficient for distinct vision of distant objects with the left eye.

At 10, the left pupil had become dilated to 3 lines, the right pupil remaining unaffected. With the left eye objects beyond 3 feet could alone be distinctly viewed, and all objects appeared smaller and more distant than natural. The left pupil was insensible to the influence of light. A drop of the extract of Calabar bean, of medium strength, was introduced into the left eye.

At 10.30, all objects beyond 8 inches were clearly seen with the left eye, and the pupils measured 3 lines on the right side and 1¾ line on the left.

A quarter of an hour later, the "near-point" of distinct vision had receded to 12 inches from the eye, but the pupils had undergone no alteration in size.

Another drop of the same extract of the Calabar bean, as was last employed, was again applied to the left eye.

At 11.30, vision with the left eye was perfectly distinct beyond 9 inches from the eye. There was no alteration in the size of the pupils.

At 1.30, all objects within 12 inches appeared dim and indistinct with the left eye, while everything beyond that distance was clear and well-defined. The left pupil was 2¾ lines in diameter, the right 1½ line.

Another drop of the extract of the Calabar bean was introduced, which, as before, improved the condition of the accommodation of the eye, the effects lasting about four hours. The application of the Calabar bean was repeated other three times, the effects at each successive application being more marked, and enduring longer. In two or three days, the effects of the atropine and Calabar bean disappeared.

This experiment led me to the conclusion that the Calabar bean and belladonna are exactly antagonistic in their action on the eye; and I ascribed the temporary nature of the improvement effected by the Calabar bean on the eye under the influence of atropine, to the fact that the atropine solution was much stronger than the extract of the Calabar bean I had employed. To put this to the proof I performed the following experiment:—

*Experiment* 3.—On February 2d, at 2.10 P.M., I introduced into each of my eyes a drop of a solution of atropine of the strength of gr.ss to ʒi. By examination, prior to the introduction of the drops, I had ascertained that vision in both was normal—all objects beyond 5 inches from the eye being distinctly perceived—and that the left pupil measured 2 lines, and the right fully 1¾ line in diameter.

At 3.20, the physiological effects of the atropine had manifested themselves pretty decidedly. The right pupil was 3¾ lines, the left 3½ lines in diameter; the nearest point of distinct vision was—left eye, 8 inches; right eye, 12 inches.

A drop of the strongest extract of the Calabar bean was now introduced into my right eye.

At 3.40, the pupils measured 3½ lines each. In vision there was a marked difference between the eyes, for with the right I could read only very large type (No. 18 of Jaeger, "Canon" type) at 2 feet distance; while with the left, at that distance I could read moderate-sized print (No. 8 of Jaeger, "Small Pica" type).

At 3.55, the state of vision in my right eye was very peculiar. I could only see objects distinctly between 6½ inches and 9 inches from the eye; anything nearer to the eye than 6½ inches, or further from it than 9 inches, appearing misty and dim. With the left eye everything appeared distinct beyond 12 inches. The right pupil measured 3 lines, the left 3½ lines in diameter.

At 4.15, the extent of clear vision with the right eye had very much increased—being in fact almost normal—all objects beyond 6 inches being seen with perfect distinctness. The nearest point of distinct vision in the left eye was 15 inches. There was no alteration in the size of the pupils from last report. Looking with the right eye objects appeared much larger than when viewed with the left. With the right eye I was able to read small print without the slightest inconvenience, while with the left I was unable to read any but large-sized print, and that only when held at a considerable distance from the eye.

At 6.55, the effects of the Calabar bean were passing off the nearest point of clear vision in the right eye, being about 18 inches from the eye. Another drop of the strongest extract of the Calabar bean applied to right eye.

At 7.5, distinct vision with the right eye ranged from 6 to 9 inches from the eye; at 7.15 it ranged between 5½ inches and 8 inches; at 7.55 between 5 inches and 15 inches; and at 8.15, objects at all distances beyond 5 inches could be most clearly defined. Very little alteration was observed in the size of the pupils or the vision of the left eye. No further application of the Calabar bean was made to the right eye, as the effects of the last application lasted until the effects of the atropine had almost passed off, and exhibited a marked contrast to the left eye, in which the atropine acted uncontrolled.

I have narrated these experiments somewhat in detail, so as to elucidate, as far as possible, the method of action of this new agent and its energy. These experiments prove that the local application of the Calabar bean to the eye induces,—*first*, A condition of short-sightedness. That this is present, and the cause of the indistinctness of distant vision cannot be doubted, as it is relieved by the use of concave glasses. The fact that objects appear larger and nearer than natural may be attributed to the induced myopia. And, *second*, It occasions contraction of the pupil, and sympathetically dilatation of the pupil of the other eye. We further observe that atropine possesses the power of counteracting its effects, and, *vice versa*, that it is capable of overcoming the effects produced by atropine. The first symptom noticed is dimness of distant vision, and shortly after the pupil becomes contracted; the symptoms also subside in the same order, first the derangement of accommodation, and then the affection of the pupil.

Let me now say a few words as to the method of action of the Calabar bean. In respect to its effects on the pupil they might be produced either by causing contraction of the circular fibres of the iris, or by paralyzing its radiating fibres. I am inclined to believe that the contraction of the pupil is due to increased action of the sphincter pupillæ, and this chiefly on the ground that the other effects produced by the Calabar bean can only be explained by an induced contraction of the ciliary muscle—the muscle of accommodation; and as the sphincter pupillæ and ciliary muscle are both supplied by the ciliary nerves, I think the most feasible explanation of the action of the Calabar bean on the eye is to regard it as a stimulant to the ciliary nerves. In favour of this view we have the feeling of straining in the eye shortly after the physiological effects are produced. The alteration, too, in the accommodation of the eye exhibits much of the character of a spasmodic action; thus we find in experiment *third*, after the second application of the Calabar bean, that the extent of distinct vision is limited to 3 inches, viz., from 6 to 9 inches from the eye, but an hour after distinct vision extends to any distance beyond 5 inches. It has also been observed that the accommodation of the eye is not usually affected in cases where contraction of the pupil is due to lesion of the sympathetic (exemplified in a case narrated by Dr von Willebrand in the Archiv. für Ophthalmologie, vol. i., where contraction of the pupil depended on the pressure of enlarged glands on the cervical sympathetic, and where no affection of the accommodation was present).

As regards the cases in which this substance may be applied in practice, it is applicable in all instances where atropine is used to render the examination of the eye more perfect or more simple. This includes two classes of cases, those in which dilatation of the pupil is either necessary or desirable to aid ophthalmoscopic examination, and those in which paralysis of the ciliary muscle is necessary, in order to ascertain the state of the accommodation of the eye.

In cases of retinitis, with photophobia, I think it might be advantageously employed to diminish by contraction of the pupil the access of light to the retina, and this more especially in those cases of this disease where the pupil has been dilated for the purpose of ophthalmic examination.

The cases, however, in which I should expect this remedy to produce the most beneficial effects are those in which paralysis of the ciliary muscle occurs as a consequence of long-continued debilitating disease. Cases of this kind are occasionally reported as following attacks of typhus or other fevers. The dimness of vision that forms a frequent sequela of diphtheria appears also to be due to this cause, judging from the symptoms detailed by Dr Begbie in an admirable paper on diphtheria, recently published in this Journal; therefore, in these cases, good effects may be expected from the use of the Calabar bean.

In cases of ulceration at the margin of the cornea, leading to perforation, or even when prolapsus of the iris has just occurred, as well as in cases where the iris has a tendency to protrude through a corneal wound, the contraction of the pupil induced by this agent might prove serviceable by drawing the iris away from the circumference.

I have shortly pointed out the cases in which I consider this remedy may prove useful, but have as yet had but little opportunity to test it practically; I think, however, there can be little doubt that in the Calabar bean we possess an agent that will soon rank as one of the most valuable in the ophthalmic pharmacopœia.

## ARTICLE VI. — *Medullary Disease of an Undescended Testicle; Removal.* By J. FAYRER, M.D., F.R.C.S.E., First Surgeon and Professor of Surgery in the Bengal Medical College.

A HINDOO, about forty-five years of age, of spare but muscular frame, of rather sallow complexion, but otherwise apparently in good health, was admitted into the Medical College Hospital, Calcutta, on the morning of the 29th October 1862, with a tumour in the right inguinal region, lying between the external and internal rings; it was painful on pressure, hard, nodulated, and about the size of an orange. The scrotum was somewhat thickened, and there was hydrocele. He had a testicle on the left side—none on the right—in the scrotum; but he said he had always had a small swelling in the situation of the present tumour. About four months ago this swelling began to increase in size and to cause pain: it became rapidly larger and more painful in paroxysms of a lancinating character. There was nothing peculiar in his appearance; and he said that, but for the tumour, he was well enough: its rapid increase and the great pain have made him anxious to have something done. He was married, but had no children; sexual power had been perfect until lately, but was now impaired.

His father died of sloughing, caused by the application of irritants to a scrotal tumour. His mother died of cholera. None of his relatives were affected with any tumour or disease. There was no glandular enlargements in the vicinity,—the mischief was confined entirely, so far as could be detected, to the tumour.

I came to the conclusion that this was one of those rare cases—a testicle arrested in its descent through the inguinal canal, and become, in middle life, the seat of medullary disease. Having this view of the case, I determined to attempt its removal.

The operation was immediately performed under chloroform. An incision was made in the long axis of the tumour, as though for strangulated hernia; the fasciæ and external oblique aponeurosis were divided on a director until the surface of the growth was exposed. It was of a dark greyish-pink colour, rather nodulated on the surface. Dissecting round it, I passed my finger easily behind the cord, which appeared quite healthy; and, having enclosed it in a strong ligature, I divided it, and reflected the tumour, dissecting downwards until it was detached. The edges of the wound were brought together with wires, and a piece of lint, with a bandage, applied.

The tumour was about the size of an orange; in making a section it presented a greyish-pink appearance, interspersed with patches of yellow deposit. The cut surfaces became convex, as though they bulged on being released from tension by the incision.

On scraping the surface of the tumour a milky juice exuded in abundance. This, under the microscope, presented abundant cell-forms, single and double nucleated, caudate, granular cells, and free nuclei. It was evidently a good example of cancer,—rather denser than one generally sees in the medullary,—not quite so dense as in the hard cancers; it more nearly resembled the encephaloid than the scirrhus. No remains of the proper structure of the testicle were visible, unless what appeared as part of the capsule might have been such, altered and degenerated by pressure. The cord, I should add, seemed unaffected. There was no bleeding during the operation. The ligature round the cord was the only one applied. The wound inflamed the day after the operation, and it soon began to discharge freely; but the patient experienced immediate relief, and, beyond the local inflammation, there was no untoward symptom. A strong solution (ʒi to ℥i of water) of nitrate of silver was applied to the edges of the wound and the surrounding skin, where there was an erysipelatous blush. Fomentations and an aperient, good plain diet, with a moderate quantity of port wine, and a little quinine as a tonic, were ordered.

Under the above treatment he is progressing favourably. The wound is rapidly healing, and as yet there is no appearance of any return of the disease. I repeat I am not sanguine as to the permanent success of the operation. There can be no doubt, however, that his condition is much improved.

On the 8th November, the wound has nearly cicatrized.

Cases of undescended testicle are interesting from their rarity; the more so when they are the seat of malignant disease. In this instance the testicle appears to have been arrested in its descent in infancy, and to have remained quiescent, though probably developed, in the inguinal canal until the middle period of life, when it became the seat of carcinoma, causing so much pain and anxiety by its rapid growth as to send him to the hospital to seek for relief. The disease appears to be an example of the firm kind of medullary cancer, and, so far as I could ascertain, it was local. The general health was but slightly affected; no enlargement could be detected in the neighbouring glands, and no tenderness on pressure to indicate any irritation in the lumbar glands. The spermatic cord seemed unaffected, and the tissues surrounding the testicle were not involved. There is reason, therefore, to hope that the relief afforded by the operation may be lasting, though all experience of the results of removal of medullary cancer are unfavourable to the prospect of permanent immunity from the disease.

As the case is one of rarity, and but few have been recorded, I am glad to have the opportunity of contributing this example of medullary disease in an undescended testicle to the records of surgery.

CALCUTTA, *November* 1862.

# Part Second.

## REVIEWS.

*Lectures on Surgery, delivered in St Bartholomew's Hospital.* By WILLIAM LAWRENCE, F.R.S., Sergeant-surgeon to the Queen, and Surgeon to the Royal Hospitals of St Bartholomew and Bethlem. John Churchill and Sons: London: 1862.

THIS work, written by a Nestor of the profession, calls for an early notice in the pages of every medical journal. When the weekly literature of medicine of the present day teems with the efforts of enterprising aspirants after fame and position, who desire to attain to something of the honour in which we hold such names as Brodie, Abercrombie, Liston, and Alison, it is something unusual for one who long since linked his name with two important works on special subjects in surgery, and who has attained to the highest honours of the profession, to enter again upon the arduous task of publication, and to choose for his subject the wide one of the whole circle of surgery. That he should have done so argues much for his undiminished love of his profession, and of his desire to communicate sound practical precepts to those younger than himself, by leaving behind him in a permanent form those views which he has matured by the life-long study of surgery in one of the largest metropolitan hospitals. It could hardly have been expected of one whose study of physiology and pathology dates back to the commencement of the century, that he should be able to shape his observations of diseased conditions to the modern theories of the Vienna or the Berlin schools; still we find in the early chapters, that while the "exudation" and "connective tissue" doctrines receive their full practical due, they are carefully excluded from the authoritative imprimatur of personal confirmation which marks the practical portions of the work.

The present volume is only, we trust, the first of several others which are to follow; and, though no promise is made for the future, we take the preliminary subjects of which it treats as a sufficient and substantial guarantee for the more practical parts which are still in safe keeping in the same rich storehouse of experience from which this one has emanated. The initial chapter consists of an introductory lecture upon the scope and study of physic and surgery. The other twenty-two chapters into which the volume is divided, treat of disease in general, inflammation, fever, suppuration, ulceration, mortification, wounds, burns and scalds, delirium tremens, tetanus, scrofula, rheumatism, syphilis, gonorrhœa, and cancer. The work is not made up of the mere details of practical observation, but comprehends large generalizations, careful reasoning, sagacious deductions, brief historical notices, and references to what has passed under the author's own observation during a lifetime, spent not only in studying disease for himself, but with a watchful eye on the labours of others in the same field. It is, besides, characterized throughout by that critical acumen and trained experience which are so greatly required in all who would be really good teachers of surgery, or of any other practical department of the healing art.

As an illustration of the general character of the volume, and its interpretation of controverted points in practice, we quote the following passage from the chapter on the treatment of inflammation:—

> "Should your own reflection satisfy you that the foregoing remarks on the loss of blood as a therapeutic measure are sound, you will not, I trust, be deterred from acting on them by the outcry and alarm raised against the practice of late years, which, originating on insufficient grounds, has been taken up and repeated more as a matter of fashion and obedience to supposed authority than from the result of experience and observation. The plan once generally prevalent in country districts, of healthy persons being bled twice a-year, in spring and fall, as a precautionary measure, would alone be a sufficient proof that blood has often been taken away incautiously and needlessly, while it may justify the belief that the practice has been less injurious than might have been expected. That the practice of taking away blood occasionally to a greater or less extent, in the treatment of inflammatory disorders, which has prevailed universally wherever the science and art of medicine have been most successfully cultivated, may have been altogether a mistake is possible. Stronger evidence and arguments than have yet been produced will be necessary to prove the point, and still more powerful proofs will be required to satisfy us that the opposite mode of combating such affections, by means of stimulants, including the free use of brandy, is either advantageous or safe. It would be a public misfortune if the sanction of professional authority should be given to a treatment calculated to encourage the pernicious habit of spirit-drinking."—Pp. 89, 90.

We do not pretend either to analyze or criticise a work which systematically considers so wide a subject; but, in giving the volume a hearty welcome, we beg to recommend it to the attention of the senior student and of practitioners, as replete with matter well calculated to be both suggestive and instructive.

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*The Climate of South Devon, and its Influence upon Health: with short Accounts of Exeter, Torquay, Babbicombe, Teignmouth, Dawlish, Exmouth, Budleigh-Salterton, Sidmouth, etc.* By THOMAS SHAPTER, M.D., F.R.C.P., etc. Second Edition. London: John Churchill and Sons: Pp. 382.

AFTER a lapse of twelve years a second edition of this work has made its appearance; but so much more elaborate in detail is the volume now before us than was its predecessor, that, although it wears the dignity of age, and commands the welcome of an established acquaintance, it bears also an aspect of freshness and multiplied worth which would not have characterized a mere *réchauffé*. The general plan of the previous volume is retained in most of the chapters; but that descriptive of the climate of the district under review is enriched by numerous tables, indicating the local phenomena of temperature, atmospheric pressure, rain-fall, etc. Experience has taught the author to discard from the present edition,—if not as altogether valueless, at least as coming far short of rigid accuracy,—the statistics which the former volume contained as representing the distribution of disease throughout the locality. They were derived from the books of the Exeter Dispensary, a sphere of observation too limited for general application to the South of Devon. The observations now made in reference to the subject are drawn from the returns of the Registrar-general. To these returns of the mortality of the district, Dr Shapter adds an interesting account of his experience of the etiology, symptoms, and treatment of the more prominent diseases; and this portion of the volume is perhaps the one to which most readers will attach the highest value, proceeding as it does from the matured judgment of an accurate observer, a close reasoner, and withal an active, practical, and accomplished physician. Several chapters are devoted to the natural productions and the civil and economic history of the district, matters interesting to the sojourner, whose love for the beauties of nature is not incompatible with a little sober insight into the commercial wealth, the local government, and the homely customs of South Devon.

Dr Shapter's experience of the climate will be most readily understood from a summary given at the close of his first chapter. It follows a minute description of the ordinary meteorological phenomena:—

> "It will be seen, in accordance with the general impression, that its chief characteristics are those of being soft, warm, mild, calm, equable, and free from storms; moreover, it is essentially oceanic, as was to be expected from its latitude and position as regards the Atlantic. Warm winds reach it from many points of the compass, either directly from the sea, or only after passing over short distances overland. Though it may be subject to rather a large average amount of rain, it seldom occurs that a whole day is so unceasingly wet as not to afford some few hours, either early or late, sufficiently fine for out-door exercise. During the winter season, the temperature is rarely maintained for any length of time at a degree so low as to render the climate particularly inclement,—frost only occasionally occurring, and then not for any long continuance. The air, though very often humid, from the general prevalence of warm westerly winds, is neither cold nor raw. It is during the prevalence of these winds there occurs that condition of the atmosphere which is understood by the expressive word 'muggy;' the air being warm, moist, and still, with a grey sky. Doubtless this weather is relaxing, and, according to the popular notion, unhealthy. Such does not, however, appear to be the case, as will be seen in the sequel,—in fact, though the air is saturated with vapour, the accompanying temperature takes from it the usual injuriousness of a moist cold climate. During the winter months the air coming from the sea is generally warmer

than that which reaches this district from the land side, and as these sea winds then chiefly prevail, they may be considered one of the chief sources of the mildness of the local climate, while much of the vapour brought by them being condensed on coming in contact with the land, particularly the higher parts, cloud and rain are the result. Clouds formed in this way are often to be seen over the land when none are to be seen seaward. These, if occurring in large amount, tend to prevent terrestrial radiation, so that at this season, when but little heat is derived from the sun, the cold is diminished; and as they are only dissipated in the formation of a light misty rain, they are the cause of many 'wet' hours, perhaps more than is usual for the amount of rain in some inland places. Though this mild grey weather may be the general character of the Devonshire winter, yet it is sometimes subject to a temperature so sufficiently low as to be severe. These occasional depressions of temperature are especially to be noted, and their effects weighed and appreciated. They are often the cause of much acute and fatal disease which, there can be no doubt, might, to a great extent, were only due caution observed, be avoided. . . . The general mildness of the winter weather often induces the casual visiter, on the recurrence of these occasional periods of cold, to consider the climate has been falsely represented; not so, however, mildness is its essential character, inclement cold the exception; and this exception not only occurs, but is to be expected, for it is the common lot of European climates, even the mildest of them, to have their seasons of severity.

"The character of the early spring does not materially differ from the winter, excepting that the air is less damp and the days less rainy. Towards the middle of this season north-easterly winds prevail, and these are often piercing and cold, while the sun shining joyously, gives the general impression of a summer's warmth. As the spring advances, these winds are tempered by warm showers, and summer with its genial heat is ushered in. The summer is rarely very hot. The occasional clouds of this season, by obstructing its [?] rays, serve to moderate its heats, so that vegetation is often green and luxuriant when parched elsewhere. Though showers are frequent, the air is dry. The winds, which for the most part blow from the north-west, are cool and refreshing. Take it altogether, it is a fine and agreeable season. The evenings and nights, however, are sometimes cold and damp, so that exposure at these times, with only light summer clothing, is to be avoided. July and August are, perhaps, the only safe months in which out-door lounging during the evening can be safely indulged in. The autumn is warm and inclined to be damp and rainy; it is peculiarly the season of a light drizzling rain, which is associated with a uniformly grey, clouded sky. The winds during this season are chiefly from the west."

Passing over the intervening chapters, we close with an analysis of that which treats of the climate of the South of Devon, considered with reference to its general effects in health and disease. The primary effect upon persons proceeding thither from a colder climate, is to induce lassitude of the physical and general laziness of the mental apparatus; the skin and liver increase, whilst the lungs and kidneys diminish in activity; the circulation of the blood becomes softer and more unifom, and nervous irritability is allayed. The climate is commendable, says our author, in cases of disorder of the chest generally, but more especially in that irritable state of the lungs which precedes the development of consumption; in inflammatory bronchitic affections; in scrofula generally; as also in some forms of dyspepsia. Persons returning from a life in the tropics are said to find the climate of South Devon suitable to their condition, as being less likely to promote the general and local congestion to which they are prone. On the other hand, persons suffering from "fulness of the head, any tendency to derangements of the great bloodvessels, a relaxed state of the uterine system," etc., are to be dissuaded from resorting to that district. Climacteric diseases are often alleviated by a sojourn in South Devon. Scrofula, consumption in its incipient stages, some of the diseases peculiar to females, are amongst others quoted by Dr Shapter as those calculated to find alleviation in the South of Devon. We recommend a perusal of Dr Shapter's work to all who are personally or relatively interested in the climate of the south-west of England. It contains a vast amount of information both statistical and general. The author evidently jots rather than writes; and the book has the character of a well-stocked note-book, especially on those subjects of greatest practical importance. It is carefully illustrated with maps and diagrams, which enhance its value as a work descriptive of the physical characteristics of the district. As a whole, Dr Shapter's book affords most interesting and instructive reading.

*Observations on the Diseases of the Rectum.* By T. B. CURLING, F.R.S., etc. 3d Edition. John Churchill and Sons: London: 1863.

THE previous editions of this work have made Mr Curling's name well known to the profession as an authority on the subject of this important class of diseases, and this third edition will add still further to his well-earned reputation. It is no mere reprint of the former editions, but, as the author tells us in the preface, a great part of it has been re-written or altered, and several new chapters added, some of which are very important and interesting.

The first of these new chapters is devoted to the obscure subject of nervous affections of the rectum, including morbid sensibility and neuralgia of the gut, and also a condition which Mr Curling calls "irritable rectum." The latter consists in an inclination, more or less urgent, to empty the bowel at inconvenient times, though both mucous membrane and fæces are healthy, and often there is little or nothing to expel. This condition seems rather to be a mental than a bodily ailment,—less an irritable state of the rectum than a result of mental emotion, an expectant attention to the condition of the lower bowel, combined with a vivid appreciation of the unpleasant consequences, should an evacuation be required. As the recorded cases show, it exists generally in clergymen, public speakers, and travellers. The purely mental nature of such cases is well shown in one recorded by Dr Carpenter, in which a public lecturer having once suffered great uneasiness from such an impulse during a lecture, had a return of the same at every subsequent lecture at the same place, though he was never annoyed anywhere else, even under like circumstances. The increase in the number of such cases since the introduction of long journeys by express trains without a stoppage, may be attributed as much to the state of apprehension and expectant attention, as to any speciality in the mode or the rate of travelling.

In the chapter on hæmorrhoids, when speaking of the bleeding from internal piles, we think Mr Curling insists too much on the possible good effects in some cases of these insidious hæmorrhages. With reference to their treatment, he strongly condemns the method by écrasement lineaire, on account of its rudeness, tediousness, and difficulty. As regards its results, the following sentence from the pen of M. Nelaton is worth quoting:—"During about a twelvemonth I have seen a great number of patients who have come to me in order to undergo an operation to remedy anal stricture, the unfortunate consequence of removal of hæmorrhoidal tumours, the stricture sometimes scarcely admitting the passage of a quill. It has arisen, because not only the mucous projection, which alone constitutes the disease, has been removed, but also a more or less considerable portion of the skin of the orifice of the anus."

In the chapter upon the treatment of fistula in ano, Mr Curling states that in some cases he has found the ordinary incision through the structures intervening between the inner and outer openings prove insufficient, and proposes a method of laying open the sinus, which in such cases is often found burrowing for some distance upwards along the side of the rectum. This is to pass a straight director up to the extremity of the sinus, and to carry along its groove one of the blunt-pointed blades of a pair of knife-cutting scissors, the other blade being passed up the rectum; then the blades being closed, to divide the intervening membrane, and expose the sinus. From the depth of the parts, and the little control it is possible to have over the instrument, this must in most cases be a very doubtful if not dangerous procedure; indeed, Mr Curling himself limits its use to those cases in which the sinus is evidently close to the bowel, it being quite inadmissible in those in which the sinus penetrates the areolar tissue of the pelvis, or lies deep in the buttock.

The chapter on obstructions of the rectum, and the operations required for their relief, is of great interest and value, from the large experience of these rare operations which Mr Curling has had. In cases of complete obstruction of the lower bowel from malignant disease, or in cases where, from a similar cause, openings have been formed between the rectum and the bladder or urethra, and extreme misery results, the formation of an artificial anus at some part of the colon has given occasionally great relief. Two portions of the intestine have claimed the preference as suitable for such operations,—the left inguinal region, in which the peritoneum is opened, and the new anus formed in the sigmoid flexure of the colon; or the left lumbar region, in which an anus is established in the loin by an opening made in the descending colon external to the peritoneum.

The inguinal position has the one advantage, that the artificial anus is better under the control of the patient from its position in front; but Mr Curling unhesitatingly gives the preference to the left loin for several reasons. In it the peritoneum is not necessarily wounded as it is in the other, and, what is even more in its favour, the colon is opened at some distance from the diseased part, and is thus in a more suitable state for operation. "The spot where the intestine should be sought for and opened is about two fingers-breadth above the crest of the ilium, and midway between the anterior and posterior spinous processes." Remembering this spot, an incision is to be made across it, from the outer margin of the erector spinæ for three or four inches outwards. After cautiously dividing the muscles and transversalis fascia, the colon is to be drawn towards the outer wound, and a longitudinal incision of not less than an inch in length made into it. The edges are then to be secured to the lips of the wound in the skin by metallic sutures. The great difficulty in the operation is in cases where the colon lies undistended and flat on the side of the wall. In cases where it is possible, the colon should, previous to the operation, be distended with tepid water, as was done in the case operated on by Mr N. Ward. Of seven operations at which Mr Curling was present, four of which he himself performed, four of the patients recovered, and three died. The deaths were in two cases attributable rather to the length of time during which the obstruction had lasted *before* the operation, than to the operation itself. In three of the four successful cases, in which the obstruction was carcinomatous, life was prolonged two, five, and eight months respectively; while in the fourth the stricture was non-malignant, and the patient was able to attend to his business, when last heard of, a year after the operation.

The last two chapters of the book also consist of new matter, and contain a succinct but very exhaustive account of congenital imperfections of the anus and rectum, the treatment required, and the merits of colotomy in such cases, founded in great measure on a paper by the author in the forty-third volume of the Transactions of the Medico-Chirurgical Society. These very various, distressing, and fatal malformations have hitherto hardly met with due attention from the profession, nor have the results of operative procedure been satisfactory; and the cases of operations in the living, and experiments on the bodies of still-born infants, recorded here, deserve the most careful consideration, tending to prove that in many cases inguinal colotomy, at an early stage, is a far safer and more successful plan of treatment than is diving for a rectum in the perinæum, except where guided by the distention caused by the meconium.

For further details on this most interesting subject we must refer the reader to the book itself, which is concise, practical, and interesting,—in every way worthy of its author.

## NOTES, MEDICAL AND SURGICAL, FROM MADAGASCAR.

By Dr Andrew Davidson, Medical Missionary, Antananarivo.

Since I wrote last, His Majesty Radama has granted me an hospital, which I have already opened for the sick. It will require £40 or £50 to make it in every way suitable for its object, and when finished will hold from thirty-five to forty patients. At present only one or two of the rooms are ready and occupied, and I am anxiously awaiting the completion of the rest of the building, in order to accommodate the numerous patients who daily apply for admission. The hospital is situated in the suburbs of the city, and is surrounded by a mud wall, which encloses a considerable piece of ground. I have appointed Kakotavo (whose Christian name is Daniel) house-surgeon. He resides in the hospital, and is now so far useful as to be able to make up the medicines according to prescription. We still retain the dispensary in town, which, as I formerly mentioned, is conducted on the same principle as the Cowgate Dispensary. It is still crowded to excess, hundreds going away every week unattended to, and many of these are persons of the highest rank. To give you some idea of the cases operated on, I may mention that I have had above thirty cases of hare-lip, all of which, with one exception, have been successful, both as regards the healing of the wound and the removal of deformity. Not having any hare-lip pins, I have adopted the interrupted suture, with iron wire; and this has been so successful as to leave me no room to desire any other means.

I have operated on two cases of cataract; in one the operation was successful, but in the other the inflammation was so severe that I had to perform the operation of excision of the eyeball. I attribute my want of success entirely to the fact that I had broken my cataract knife,[1] and performed the operation very clumsily with a pair of scissors and a director. I had one case of cancer of the mamma, which had existed for two years, and involved a large part of the skin, and required an extensive wound to remove the disease. This operation was performed in the presence of Radama, who expressed great astonishment at it. I operated the other day for fistula *in ano;* there were five fistulous apertures; the patient is doing well. I will not attempt to say how many tumours, fibrous, fatty, and others, I have removed; one kind only deserves notice as being rare in Europe, viz. elephantiasis of the labia. I have removed two growths of this kind with entire success, and comparatively little difficulty. One very large one of the labium, and another of the scrotum, the former weighing about thirty pounds, and the latter much more, are awaiting removal, and, if spared, I shall operate next week. I may say that the former of these came under my knife before. I attempted to dissect carefully, tieing every artery as it was cut. The first incision was not followed by much blood, as it was merely through the skin, but the second opened about twelve arteries that required ligature. I found it necessary to delay the operation for a time, and I now intend to do it with rapid incision, so as to remove the whole tumour in a few seconds, and then tie the arteries as quickly as possible. One rather interesting case is worthy of special mention. It was a large cyst in the neck, stretching from near the clavicle into the mouth, and pushing the tongue to one side. It was deeply situated, and felt hard to the touch. On opening the cyst, I found it containing a thick fluid or semi-solid matter towards the lower part, and a fluid about the consistence of white of an unboiled egg at the top. I opened the cyst by two incisions, one in the neck about two inches in length, and another in the mouth. Instead of dissecting out the cyst, as is generally recommended,—which in this case would have been difficult both on account of the situation and extent of the disease,—I procured inflammation and suppuration of the sac by injecting tincture of iodine into the wound by the opening in the mouth, and keeping the lower opening patent by inserting lint. The sac adhered from above downwards, and now that the wound has healed, scarcely any mark remains. I mention this case particularly, because the patient, originally a strong robust man, had in the space of three months been almost brought to his grave by the pain which the tumour occasioned, as well as its interference with mastication.

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[1] The Directors of the Edinburgh Medical Missionary Society have resolved to send Dr Davidson an immediate supply of eye instruments.

The medical cases, although probably not so interesting to me as the surgical, have been very instructive. Diseases have here been alike unmitigated by art, and unaltered in their character by foreign intercourse. The strict exclusiveness of the old queen saved the island from the ravages of the cholera, which has proved very destructive in the neighbouring islands, Mauritius and Bourbon, tending to prove what can scarcely be denied, that cholera is an infectious disease. I will say nothing at present about the Madagascar fever, as I have only treated two cases; but as the fever season is now at hand, I will (D. V.) have further opportunities of studying this disease. Consumption and chest diseases generally are nearly as common as in England, and much more rapid in their course. I account for this by the great changes in temperature; the thermometer in the shade during the day often being as high as 90° to 100°, and at night has even sunk as low as 45°. It is frequently about 60°. The people, also, are insufficiently clad; and the houses, being without glass, are colder than European houses. During the dry season, the air is full of a fine penetrating dust, which produces a disease of the nature of asthma, and often passes into consumption. The vital energy of the Malagasey is below that of the Englishman. He does not take the same length of time in dying that a European does, but rapidly sinks under disease.

Syphilitic diseases are here common; nay, I might say, almost universal. I have read of no form of venereal disorder that I have not seen here; but I think I have seen some forms different from any described. I never knew a native thirty years of age, male or female, who did not candidly confess to having had gonorrhœa, syphilis, or the yaws. Constitutional syphilis is exceedingly common amongst children; very many of the children formerly died of it. So deadly do the natives regard it, that a child who shows signs of it is "given up," as we would say. It begins, as at home, about five weeks after birth, and exhibits the same character, but is decidedly more dangerous. At home, in the practice of the dispensary, I have seen good results from minute doses of the bichloride of mercury; but such a medicine is particularly unsuitable to infants of such a tender age, and has not by any means proved useful here. Hydrarg. c̄ creta I have also fairly tried, but with little success; new pustules forming, and old ones ending in suppuration and loss of substance under its use. The remedy which, after many trials, I have found beneficial is mercurial *inunction*, as recommended by Sir Benjamin Brodie. This has never failed,—no, not in a single instance. It produces its effect much more speedily than any other with which I am acquainted, and leaves none of those disagreeable results behind which too often follow the use of mercurials both in children and adults. I had a case in which one of the fingers and several of the toes were ulcerating, so as to place hands and feet in great danger. In two days after the inunction with mercurial ointment, the ulcers changed in character, the inflammation decreased, and in a week the parts were healed. I will afterwards give a much more particular account of my observations on syphilis; let me only add at present, that not only do the primary, secondary, and constitutional forms of this disorder prevail to an extent quite unknown in other countries, but that there is here, so to speak, a syphilitic diathesis, so marked as to force itself upon our notice, modifying in a most important way every skin disease, and many other maladies, and requiring that its existence be recognised in order that the treatment of disease may be successful. Abortion in the fourth and sixth months is so common that, until I get regular statistics upon the subject, I would not ask any man to believe it upon my word.

A very great number of cases of leprosy have come under my observation, and I have devoted particular attention to this disease, in order if possible to ascertain its cause, and obtain some clue to its treatment. I have kept a detailed account of many of the cases under my care. I will copy these, and forward them, with a few inductions from the facts which I have gathered. I will at present briefly state the results at which I have arrived. *First*, Leprosy, by which I mean the tubercular, is uniform in its character and manifestations; its commencement, progress, and termination being as distinct and uniform as in small-pox or scarlet fever. *Second*, Its cause is ONE, *not many*, as has generally been represented by those who ascribe its origin "to the want or disuse of salt, to the use of unripe, spoiled, or mouldy grain, to inattention to personal cleanliness," etc. *Third*, This cause is in all probability a poison generated by the use of dried semi-decomposed fish as an article of food. *Fourth*, It is contagious only by inoculation, and not by simple contact. *Fifth*, No leper is capable of propagating his species.

Some of my students are good fellows, and would do credit to a better instructor. They delight to style themselves, *Boksters of the Sicol Medisen.* I have to make all my instructions practical. They do not understand theories. I get each of them to give me a paper every fortnight on one of the classes of disease treated in the dispensary or hospital. The writer has to give the symptoms and treatment, and to note carefully the results. I make them examine the patients, diagnose the disease, and suggest the treatment. After a time two or three of them will, I have no doubt, be useful here as medical missionaries.

# Part Third.

## PERISCOPE.

## REPORT ON PHYSIOLOGY.

BY W. GILCHRIST, M.D., TORQUAY.

### DIGESTION.

#### ON THE PROPERTIES OF THE SALIVA OF THE PAROTID AND SUBMAXILLARY GLAND IN MAN. BY L. S. BEALE.

The author gives, as the result of his inquiries, the following conclusions:—"1. The saliva of the submaxillary gland is always alkaline, that of the parotid in most cases. Sometimes, especially in the morning, the first few drops of the latter are found of an acid reaction; soon, however, this disappears, and an alkaline one takes its place. 2. Immediately after secretion each kind of saliva is perfectly transparent and fluid, and does not contain any cells. Very seldom, however, a few epithelial cells of the ducts are met with; but the corpuscles, so peculiar to the mixed saliva of the mouth, are never found. After a while the fluid of the submaxillary gland gets gradually viscous, and a kind of flocculent sediment is found; whereas the parotid saliva does not change at all, or at most gets a little opaque. 3. Both kinds become opaque when heated, and even small quantities of coagulated matter are formed. By adding mineral acids, solution of the precipitate does not take place. But the quantity of this substance is too small for determining its amount or ascertaining its properties. We shall see that this principle takes part in the physiological action of the saliva. 4. The specific gravity of the parotid fluid is higher than that of the submaxillary gland; and as to the aqueous residue, we find that the same holds. 5. In man each kind of saliva converts starch into sugar. As to this point there is a remarkable difference between the saliva of man and dog. In the latter, according to the investigations made by Messrs Schmidt and Bidder, it is only the mixture of the different fluids of the glands with the fluid of the mucous membrane of the mouth which is able to produce the change just mentioned. In man there is no doubt that the saliva of the submaxillary glands, as well as that of the parotid, acts upon the starch within a very short time. I have often tried it with the same result. 6. Both fluids have this property when heated for a short time, whereas refrigeration to some degrees beneath zero of the thermometer does not injure them in any way. Remembering the fact 3. we may submit that the conversions of amyloid substances into sugar might be effected by the small quantity of albuminous matter we find in each kind of saliva. 7. Sulphocyanide of potassium I found only in the saliva of the parotid gland. In that of the submaxillary gland I failed absolutely, although I tried it at different times and on different individuals."—*Beale's Archives of Medicine.*

#### CONTRIBUTIONS TO OUR KNOWLEDGE OF DIGESTION. BY E. BRUECKE.[1] PART II.

These researches relate to pepsin.

Bruecke observed that if the extract of the mucous membrane of the stomach (prepared by digestion with diluted tribasic phosphoric acid) is nearly neutralized with lime water, almost all the pepsin is precipitated with the phosphate of lime. This is a mechanical precipitate of pepsin, and other deposits accompany it. If the digestive fluid is agitated with animal charcoal, it is found that the filtrate has lost its digesting property. Finely pulverized sulphur has the same effect. In order to obtain a clear view of its nature, Bruecke precipitated pepsin with phosphate of lime, and repeated the precipitation after solution of the deposit in hydrochloric acid. To the acid solution he then added an alcoholic-etherial solution of cholestearin, and agitated. The precipitated cholestearin was collected and extracted from the adhering pepsin by ether. There remained over a clear neutral watery solution. This solution of pepsin did not show all the reactions which are given by pepsin. Strong nitric acid, tincture of iodine, and tannin produced no cloudiness, nor did chloride of mercury. But chloride of platinum, basic, and neutral oxide of lead produced distinct cloudiness. Bruecke is therefore of opinion that pepsin does not belong to the albuminoid group.

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[1] For an account of the first series of Bruecke's Researches, see *Edinburgh Medical Journal, August* 1861.

The precipitate which results, when bile or the solution of bile-acid salts is mixed with the solution of an albuminous body which has been digested with artificial gastric juice, consists, according to Bruecke, partly of bile-constituents, partly of the albuminous matters which can be precipitated from the digested mixture by neutralizing it. B. means the parapeptone, which he regards as albuminous material not yet digested. Bruecke believes that the reason why bile suspends the action of pepsin is partly that the bile precipitates albumen, and causes it to shrink, so that the digestive effect of the gastric juice is prevented, partly that the precipitate carries the pepsine down with it. That the appearance of a precipitate on the admixture of the stomach-contents with the bile does not, as Bernard supposes, abrogate the use of the stomachal digestion, Bruecke shows that each deposit, when alkaline reaction is produced by the pancreatic and intestinal secretions, is again dissolved.

As Bruecke observed that with an equal amount of pepsin a large amount of pepsin was as speedily dissolved as a small amount, if only the conditions for the swelling out (quellung) of fibrin, water, and acids, were proportioned to the fibrin quantities; and as he further succeeded with a given quantity of pepsin in dissolving new additions of fibrin, if only every time a little phosphoric acid was added (phosphoric acid is more convenient than muriatic), Bruecke concluded that pepsin is not destroyed in digestion, and he proposed the question,—Where does it go after it has passed with the food into the intestine? He sought to precipitate pepsin from the urine by means of basic phosphate of lime, and obtained a substance which, when acidulated, slowly dissolved fibrin and coagulated albumen. He also obtained from the watery extract of flesh a substance the acidulated solution of which displayed digestive properties. Bruecke does not doubt that the same pepsin which is secreted by the gastric follicles is found likewise in the urine and in flesh juice. - *Henle u. Meissner's Bericht* for 1861, p. 241.

### RESEARCHES ON THE DIGESTION OF ALBUMINOUS BODIES. BY G. MEISSNER AND C. BÜTTNER.

These researches relate principally to the fibrin of blood. The fibrin was obtained from pigs' and cows' blood, was cut into fine portions, and washed with water containing a little ammonia.

When fibrin has been completely digested with artificial gastric juice, a body remains insoluble in the acid of the juice which appears as a fine gelatinous precipitate suspended in the fluid, and is the analogue of the dyspeptone which results from the digestion of casein. It appears that this dyspeptone is nothing else than a part of the parapeptone — for the parapeptones of the various albuminous bodies (including those from plants) have the property, by the increased and long-continued action of gastric juice, of becoming gradually less soluble, and at last insoluble, in a certain strength of muriatic acid. The parapeptones are distinguished by their different tendencies in this way to lose their solubility; thus the parapeptone of casein becomes most insoluble, next to it that of fibrin; hence, in experiments on the digestion of these bodies, a so-called dyspeptone, *i. e.* a part of the parapeptone which has become insoluble, is separated from the rest.

On neutralizing the acid solution, filtered from the dyspeptone of blood fibrin, the parapeptone separates itself. If the neutral filtrate is again very slightly acidulated with acetic acid, metapeptone is precipitated, and by heating the filtrate a second body results. These are not definite decomposition products, but merely transition states of peptones.

After the separation of both these bodies there remains a clear pure solution of peptone. In reference to the peptone of fibrin, it must be remarked that this solution does not contain one but three peptones, which may be termed *a* peptone, *b* peptone, and *c* peptone. All the three are easily soluble in water and diluted acids; but they present certain distinctive chemical reactions.

Metapeptone is by the continued action of gastric juice converted into *b* and *c* peptone. Meissner therefore thinks it very probable that the metapeptone of syntonin is not a definite decomposition product. The second body above referred to, obtainable by heat, may likewise be converted into *b* and *c* peptone.

Like other albuminous bodies, fibrin, by continued boiling with water, undergoes the same decomposition as with digestion with pepsohydrochloric acid. The greater part of the mass of fibrin goes into watery solution; another part remains quite insoluble in water.

Fibrin, by continual boiling, undergoes the same change as by digestion with pepsohydrochloric acid. The greater part of the fibrin is dissolved, but a small part remains insoluble in water. In the decoction, metapeptone is found, and the two peptones *b* and *c* are found, but not a trace of peptone *a*. The metapeptone cannot be farther changed by boiling water, and cannot be converted into peptone, as it may be by gastric digestion. In fact it would appear that by boiling, fibrin is not so completely decomposed as by digestion with gastric juice; but the effect of both processes is essentially similar.—*Henle und Meissner's Bericht*, 1861.

### ON THE FUNCTION OF THE LIVER. BY F. KÜTHE.

(*Studien des Physiologischen Instituts zu Amsterdam.*)

Küthe found that, by touching the orifice of the ductus-choledochus with acid, continued flow of bile resulted. But this did not occur when an alkaline fluid was used. The author concludes that the acid contents of the stomach operate in this way, and that during fasting, as the acid reaction diminishes but little, bile flows into the intestine. (The above observation was originally made by Bernard.—W. G.)—*Henle und Meissner's Bericht*, 1861.

### ON THE CHEMICAL PROPERTIES OF THE PANCREATIC JUICE IN MAN.

BY W. TURNER, M.B.

Turner made observations upon a fluid obtained from a pancreas, in the extremity of which was embedded a cancerous tumour, which, by its pressure, compressed the biliary and pancreatic ducts; so that "the principal pancreatic canal, as well as several of its small tributaries, and some of the lobules, were dilated in a series of vesicular cavities, so that the body and the splenic extremity of the gland had the appearance of having been attacked by a cystic affection."

The liquid presented the following properties: it was of a citron yellow colour, and very viscous; sp. grav. 1·0105. It contained small white flocculi, which consisted of groups of the gland-cells. It presented a slight, but distinct, *acid* reaction. Heat produced a precipitate of a yellowish white colour, which was much increased on adding nitric acid. Alcohol produced a flocculent precipitate of a yellowish white colour, which appeared insoluble in an excess of distilled water. Corrosive sublimate and the bichloride of platinum produced an abundant flocculent precipitate. Oxide of copper was not reduced. The perchloride of iron produced no red tint. On mixing a small part of the secretion in a mortar with two or three drops of olive oil, a partial emulsion was obtained. A negative result with the iodine-test proved the conversion of starch into dextrin. On boiling white of egg with a few drops of the liquid, no modification of the albumen was produced; but this experiment was unsatisfactory, on account of the small quantity of secretion used.

Turner next compares the above results with those obtained by other observers, and the most striking difference consists in the *acidity* of the secretion in the case examined by him.—*Journal de Physiologie.* (With a cancerous tumour in one part of the gland, and with cystic-like formations in the other, it is questionable how far the characters of the fluid examined may be regarded as normal, and capable of comparison with the pancreatic juice taken from a healthy animal.—W. G.)

### ON PANCREATIC DIGESTION. BY M. SCHIFF.

(*Archiv der Heilkunde*, ii., p. 321.)

Schiff, in former experiments by himself and Corvisart, had discovered that the pancreas, in order to effect digestion of albuminous bodies, must be charged ("geladen") by digestion-products re-absorbed from the stomach. Corvisart regards these essential digestion-products as peptones. Schiff has more recently found that the mucus-membrane of the stomach also must be charged with digestion-products, but that these may be introduced into the blood through other channels than the stomach. Schiff made it a question whether the presence of certain matters in the blood was not necessary for the function of the pancreas. Then dogs were richly fed, so that the pancreas was perfectly emptied within ten to twenty-four hours. Dextrin or digested flesh was then injected under the skin, and the animals were killed after some hours. An infusion of the mucus-membrane of the stomach was always found effective, proving that its mucus-membrane was charged; but an infusion of the pancreas was always found, under these circumstances, to be ineffective. Schiff consequently concludes that the matters requisite for the pancreas must be absorbed from the stomach; and he gives the following explanation:—For the charging ("ladung") of the pancreas certain matters must be present in the blood which can be separated from it as a ferment, but that this ferment be separated in the gland a second agent is required, viz., excitement of the bloodvessels of the pancreas to secretion; this excitement leading to distention of the vessels is probably reflected from the stomach, which must be in absorbing activity. This re-absorption he supposes to be effected by the lymphatics. He tried oil, but found no effect on the pancreas as regards its digestive efficiency. But when emulsionized by gum, oil rendered the pancreas effective. Other substances, such as dextrin, the peptones, etc., introduced through the skin or rectum into the blood, by calling into activity the lymphatics of the stomach, produced a charging ("ladung") of the pancreas. The connexion between absorption from the stomach and separation of the pancreatic ferment is effected by the nervous system, for when the cœliac plexus was extirpated no charging of the pancreas occurs. Schiff considers the spinal cord to be the reflector which establishes the connexion between the lymphatics of the stomach and the bloodvessels of the pancreas. More recent experiments have convinced Schiff that the spleen also must co-operate with the pancreas in the digestion of albuminous bodies. He fed a cat with flesh, seventeen hours after a full meal tied the splenic vessels and the pylorus, and six hours afterwards killed the animal. An infusion of the gastric mucus-membrane proved powerfully digestive, an infusion of the pancreas did not. Thence he concludes that in the spleen the peptogenes taken up from the stomach are so changed that they are rendered capable of forming that material in the pancreatic secretion which digests albumen. After the extirpation of the spleen the pancreatic secretion can no longer digest albuminous bodies; hence the reason why animals from which the spleen has been extracted require a larger amount of food than in the normal condition.—*Henle und Meissner's Bericht*, 1861.

### ON THE EPITHELIUM OF THE INTESTINAL VILLI. BY COL. BALOGH.

(*Moleschott's Untersuchungen.*)

The author is of opinion that the fine drops of fat, as they are absorbed, are surrounded by the so-called haptogen membrane, and may be regarded as vesicles filled with fat. The products of the digestion of albuminous bodies have certainly the property of depositing themselves as coatings upon fat globules. And this supposition removes the difficulties which appear to result from the want of adhesion between fat and watery solutions in the absorption of the former.—*Henle und Meissner's Bericht*, 1861.

### ON THE FUNCTION OF THE LIVER. BY J. HENLE.

(*Nachrichten von der G. A. Universität u. s. w. au Göttingen*, 1861.)

Henle remarks that Handfield Jones has the merit of first pointing out that the bile is prepared in the biliary ducts from the blood of the hepatic artery, and that the parenchyma of hepatic cells serves for the formation of sugar only. Henle has come to a similar conclusion from anatomical research. He believes the bile to be the secretion of particular glands opening into the bile-ducts, which have no connexion with the lobules of the liver and their component cells. These latter form from the portal blood a substance hitherto identified with sugar. Ligature of the hepatic artery arrests in the most constant manner the secretion of bile. That the ligature of the portal vein should not offer a result so purely negative, Henle explains from the circumstance that the venous branches which arise from the capillaries of the hepatic arteries reach the hepatic veins through the portal system, consequently a disturbance of the portal current can react on the hepatic artery. He expects no change of the glucogenic function from obliteration of the hepatic artery; but a cessation of the glucogenic function would arise from obliteration of the portal vein, as indeed occurred in two pathological cases reported by Frerichs, and also in the experiments of Stokvis and Moss. From a teleological point of view the sugar in the liver may be regarded as consisting of the constituents of the bile as they are again taken up from the intestine; in other words, the bile can directly, through this channel, reach the cells of the liver.—*Henle und Meissner's Bericht*, 1861.

# Part Fourth.

## MEDICAL NEWS.

### REPORT OF THE TRIAL OF ALEXANDER MILNE FOR MURDER.

*In which the Plea of Insanity was found Not Proven.*

With REMARKS by HUGH COWAN, Advocate.

THE trial of Alexander Milne, in the High Court of Justiciary at Edinburgh, on the 7th, 8th, and 9th days of February 1863, for the murder of James Paterson, presented features of more than ordinary interest. The circumstances attending the deed itself—an act of sudden violence at noonday in one of the principal streets of the city, committed, moreover, upon one who was the murderer's friend—were sufficient of themselves to attach great interest to the trial. But the true interest of the trial arose from the question raised, Whether the man who committed that deed was or was not insane? And the importance of the trial lay in this, Whether the insanity of the prisoner, assuming all, that the defence maintained was proved, to be true, was such as could excuse or palliate his guilt? The evidence adduced for the defence amounted at the utmost to this, that the prisoner, by a course of drinking, had brought himself to the verge of delirium tremens, that he had some delusions in regard to the deceased, and that in a fit of madness he stabbed him. But the medical testimony agreed in saying, that at the time when he did so he was not labouring under delirium tremens, that, with the exception of the delusions, he was of sound and intelligent mind, and that there was no reason to suppose that he was labouring under such mental alienation as to lead him to think that he was not doing wrong.

To have sustained the plea of insanity in those circumstances would have been fraught with much danger to the interests of society. And it is right that the man, who, through vicious indulgence, has on the one hand stirred into vigour the bad principles of his nature, and on the other weakened his power of self-control, should underlie the sentence which law, human and divine, attaches to his crime. The recommendation of the jury may lead to the exercise of the royal prerogative of mercy, and, if so, it is better that the case should end thus, than that judicial countenance should be given to the plea of insanity on grounds such as were here presented.

The plea of insanity, while on the one hand it affords, if well established, a conclusive defence to a charge of crime, must on the other be very cautiously received, and the evidence by which it is supported be very carefully scanned, as there is no plea which more readily suggests itself to a man charged with a great crime, or which a cunning man can more easily feign. The case of the prisoner Milne appears to me to bear very evident marks of feigning. The evidence of insanity is sought in his case in the delusions under which he is said to have laboured at the time of the act. These delusions, so far as they are proved by independent testimony prior to the act, are stated by Dr Christison to have been two: (1.) About the robbing of his shop; and (2.) About Paterson trying to poison him. To the first of these Dr Christison attaches little importance, as a new light was thrown upon it by the noise at the prisoner's shop-door, which some of the witnesses proved. The evidence of the porter Laing, however, showed the existence of a delusion in regard to this matter prior to the noise at the shop-door. Referring to the evidence of that witness it seems to me to be the most important evidence adduced as to the existence of delusion at all. For, if he understood the prisoner aright, the prisoner on the Monday night saw robbers where no robbers existed, and, under a delusion, spoke of persons as present where none were. But that evidence stands alone, and is not supported by any other evidence as to Milne at any time seeing other things which had no existence. And this evidence standing thus by itself is readily explained, when we consider, *first*, the suspicions which he seems to have taken up that an attack was to be made on his shop; and, *second*, the strange theatrical manner of the man, which led him to invest with lifelike reality the unreal fancies of his brain. The description which he gave Laing of the robbers on the Monday was mere acting; and while it seemed to Laing to be a serious performance, to Milne it was but a stage rehearsal. The noise at the shop-door on the Monday night gave a colour to the suspicious fears which Milne had previously entertained, and next day, with perhaps somewhat of overcautiousness, he does what he thought prudent to secure his goods and his shop. He conveys the most precious of his wares to Smith and Philpot's strong box, and he orders, from Forrest the blacksmith, a strong bar to protect his door. The second delusion was about the poisoning. That Milne refused to take medicine out of a bottle which Paterson had touched, saying that Paterson had poisoned it. That he complained of Paterson trying to poison him to get his business and his wife is true; but it does not follow that these were delusions. An unfounded suspicion is not a delusion. If Milne had taken up a prejudice against Paterson, if he had either with or without cause become jealous of him, I can well understand that he might refuse to take anything that Paterson touched, and might even, to give weight to an unreasonable crotchet, allege a false reason. On the whole question of these delusions the remark made by Paterson's landlady to him on the morning of the murder is of vital importance. She advised him not to go, saying, Milne might think he was too thick with his wife, and that these foreigners were very cunning. To say the least of it, this was a very odd remark for her to make; but looked at in connexion with the dreadful event which so soon followed it, it does seem to throw a great deal of light on the case. The frequency of his visits to Milne, perhaps the way in which he spoke of his relations with the family, led Paterson's landlady to think that, with or without cause, Milne might readily entertain towards Paterson a jealous feeling which he might be concealing. She had opportunities, too, of seeing Milne, and had doubtless come to think him a very cunning man. If Milne did entertain such a feeling of concealed jealousy towards Paterson, the delusions by which the insanity of Milne is said to be proved cease to be delusions, and we, at the same time, find a motive for the murder. It is not necessary for this to suppose that there was any real cause of jealousy. Daily experience tells us on how slight grounds a suspicious nature comes to think evil of those who are nearest to it. The most intimate friends are for ever separated on some misunderstanding which had no grounds on which to rest. The feeling of jealousy is well-known to take its rise most commonly in the most trivial circumstances. At first it is but slight, but it grows and increases, fed by the slightest and most innocent circumstances; and while the circumstances which occasion it are in themselves the most trifling, and, to an impartial onlooker, utterly absurd, as accounting for the jealousy, to the jealous man they are as fuel to the fire which consumes him. But jealousy is the rage of a man, and Milne will readily think all evil of Paterson. He is poisoning me; I will not take the medicine which he touches; he will rob my shop. Unreasonable suspicions! unfounded jealousy! What else have we here? And so it goes on, until the ill-regulated passion, fanned, it may be, by the course of dissipation which he has gone through, impels him to the deed. And when the time does come, it is done with the deliberation and forethought of a murderer. He goes to purchase a weapon. He sends for his victim, and then stabs him through the heart. In his subsequent conduct there is everything to coincide with feigning, and but little that accords with insanity. He gives four accounts of the act he has done. (1.) To Mr Bruce Allan—He has found Paterson in bed with his wife and has killed him. (2.) To the constables who apprehended him—He has given his working jeweller a prog with a dirk. (3.) To the lieutenant of police—He was jealous of Paterson, who had taken liberties with his wife; they had quarrelled on the subject, and he stabbed him. And (4.) In his declaration—It was an accident. To this last he adheres, and as day by day he tells it to his fellow-prisoners, he adds, that it is impossible he could have killed him intentionally, for Paterson was his friend. Ever since his apprehension he has been speaking of delusions. The delusion of the vapour, of Paterson having actually taken liberties with his wife, and of the conspiracy among his friends to rob his shop, have all appeared only since his apprehension. Two other delusions have also been spoken of: he believes Paterson is not dead, and Paterson has come to see him in prison. These last are among the most likely for a person pretending insanity to feign. The first of them made its appearance for the first time in his interview with Dr Smith; and Dr Smith says, he was quite satisfied that Milne was trying in that delusion to deceive him. The case in regard to that delusion comes to this—that Milne knowing that Paterson was dead, knowing that Dr Smith had come to inquire into his state of mind, and knowing further, that Dr Smith knew Paterson was dead, in a solemn and serious way stated to Dr Smith that he did not believe Paterson was dead, in the hope that Dr Smith, knowing the true state of the case, would go away under the impression that he was labouring under an insane delusion. Dr Smith also says, he was satisfied that the prisoner did not really believe that Paterson's death was caused by accident. In both of these statements the manner of the prisoner was solemn and serious, as much so as in all the other statements which he made to the medical gentlemen, and yet in these two one of these gentlemen was quite satisfied that the prisoner was telling a lie. Evidence as to the existence of delusions, which is gathered only from the conversations of a person accused of a crime, must in all cases be received with caution; but in the present case there is an additional element of difficulty in the theatrical manner of this man and his tragic power. This gives him the power to utter things which he believes not, with such a semblance of sincerity, that the listener is impressed with the feeling that he believes them to be true. It will be remembered also that the statement to the lieutenant of police and Dr Littlejohn was made with great hesitation, and though Dr Littlejohn says he did not think he was coining a story, it is difficult in any other way to account for the remarkable hesitancy which he showed.

On these grounds, therefore, it appears to me that what are represented as delusions prior to the act were but the expressions of jealousy, whether with or without cause, and that what are represented as delusions subsequent to the act are, on general grounds, and in the particular circumstances of this case, to be regarded as feigned; and this conclusion is strengthened by the varied accounts which we find the prisoner give of the death of Paterson, just such vacillating accounts as are commonly given of his crime by a sane prisoner. Dr Christison, whose opinion on this matter is entitled to the highest consideration, while he thinks that it is possible to account for it by ascribing it to the strange vagaries of an insane mind, admits that it is very remarkable, and that if the man be insane it is a new and unprecedented development of insanity. The general import of the evidence as to the conduct of the prisoner prior to the act is, that there was no undue excitement about him—that he was quiet and self-possessed, acting in the way which a sane man usually does.

There is a great difference between this case and that of Thomas Donelly, reported in last number of this Journal. In that case, clear evidence was adduced of the progress of the prisoner's insanity; he was proved to be in a state of insanity prior to the act; he gave but one account of the act, and that account was in harmony with the delusion under which the act was committed.

The prisoner Alexander Milne was placed at the bar charged with the crime of murder, in so far as on Wednesday the 7th day of January 1863, in the shop in Frederick Street, Edinburgh, then occupied by him, or by the firm of A. Milne and Company, of which he was a partner, he did wickedly and feloniously attack and assault James Paterson, working jeweller, then residing with Ann Irvine or Wilson in St James's Square, Edinburgh, and did with a dagger, or poignard, or stiletto, or with some other instrument to the prosecutor unknown, stab or cut him in or near the left breast, or other part of his person, whereby he was mortally wounded, and immediately, or soon thereafter, died.

The counsel for the crown were the Solicitor-General (Young), and Mr Adam Gifford, advocate-depute. The counsel for the prisoner were Messrs Charles Scott, Sir George Home, Bart., and J. Comrie Thomson, advocates.

On the motion of the counsel for the prisoner, and of consent of the crown, the medical witnesses were allowed to remain in Court to hear the evidence adduced as to the facts of the case.

The panel pled generally Not guilty, and specially that at the time when the alleged crime was said to be committed he was insane, and labouring under insane delusions.

The general evidence led in support of the charge established the following circumstances:—On the morning of Wednesday, 7th January, the deceased James Paterson sent one of his lads[1] to the prisoner's to see about some work which had been intrusted to him by Milne the day before. This was about ten o'clock. The prisoner asked, Why didn't Paterson come himself? and told the lad to send Paterson. This the lad did, and Paterson left his lodgings[2] about eleven to go to the prisoner's. His landlady advised the deceased not to go, saying, "He is an excitable man, and too fine with his words; he may think you're too thick with his wife, and they foreigners is very cunning."[3] The deceased, however, went. At ten o'clock the same morning, the prisoner purchased,[4] at a shop in West Register Street, a dirk, for which he paid 3s. 6d., and which he examined, saying this would do. Between eleven and twelve the deceased rushed through a room below the prisoner's shop, saying he was stabbed, and out by a door into the front area.[5] The only access to the room by which he could have come was by a stair leading from the prisoner's back-shop down into the kitchen, which communicated by a door with the room through which he rushed.[6] The area into which the deceased went had no stair up

[1] George Graham, John Paterson. [2] George Graham, John Paterson, Mrs Wilson.
[3] Mrs Wilson. [4] Peter Cameron, John Ruddiman. [5] Ellen Milne.

[6] The measurements taken by Watson, police-constable, were as follows:—From Milne's front shop-door to the railing of stair leading down into kitchen, 25 ft. 11 in. The stair down had thirteen steps. From stair-foot to the room-door leading into front-area, 26 ft. 5 in. From door to north-west corner of area, 13 ft. 4 in. Height of flower-box, 3 ft. 7½ in. Height from top of flower-box to the level of Frederick Street, 3 ft. 7½ in. Height of railing 2 ft. 10 in.

to the street; but, after standing in the area a short time, he rushed all at once to the corner of the area, put his knee on a flower-box, and climbed over the railing into Frederick Street.[1] He stood by the railings for a little, put his hand to his breast, and then staggered up five or six steps into the baker's shop next the prisoner's, crying, I am stabbed.[2] There he endeavoured to seat himself on the counter, but slid to the ground, his back resting against a form. At the same time he said, Oh! I'm stabbed. The police were sent for, and also some ammonia from the chemist's. The deceased was then carried into the baker's back-shop, and laid upon the sofa.[3] Unfastening his vest, a wound about an inch long was found on the left breast near the nipple. The deceased was at that time quite unconscious. He gave a few convulsive sobs, and almost immediately expired. From the time that he climbed over the railings till his death it was ten or twelve minutes.[4] The prisoner gave several accounts of the occurrence, which will be more particularly noticed immediately, in one of which he stated that he had given his working jeweller a prog with the dirk, and in another attributed the death of deceased to an accident. He (the prisoner) was apprehended in his own shop in about ten minutes, having made no attempt to escape, and he had then the dirk in his possession with bloody streaks upon it.

The principal question involved in the case was in regard to the prisoner's sanity. The evidence in regard to that is to be found in the statements of witnesses as to certain alleged delusions under which the prisoner is said to have laboured, in the evidence as to the conduct of the prisoner on the day of the murder, and in the evidence of the medical gentlemen examined. The following was the evidence led, those parts of the testimony of each witness only being given which bore on the question of sanity.

*George Graham.*—I was sent by the deceased to the prisoner about ten o'clock on the morning of Wednesday, 7th January. I asked Milne if he wanted the Albert mounts to-day. He said, Yes; he must have them to-day. "But," says he, "why doesn't Paterson come himself? Send him along immediately." I asked if he was better. He said, "Yes; a great deal better." Milne was then in his front shop. There was nothing remarkable about him. He spoke quite calm to me, just in his usual way. He was quite sober. I was alone with him in the shop.

*Peter Cameron.*—On 7th January last, the prisoner came to my shop between nine and ten o'clock. He asked to see a dirk or stiletto that was in the window. I showed him three. He chose the first one. That is the dirk. He said that would do. He put it in his pocket, and asked the price, which he paid. Nothing else passed. He was quite sober, but had the smell of drink. No one was with him. A little boy was in the shop at the time. *Cross-examined.*—The prisoner looked calm and quiet; there was no trembling in his hand. I never saw the prisoner before.

*John Ruddiman.*—Was in Cameron's shop when prisoner came. He asked to see some of those dirks in the window. He purchased one. He looked at it for a while, and said, "This one will do." He took it out of the sheath and looked at the blade.

*Ellen Milne*, the prisoner's daughter, aged eight years.—I saw father coming down the stair into the kitchen a wee whilie after Paterson had gone into the area. *Cross-examined.*—I mind of a party on the Christmas-day in our house. Mr Kilgour and his wife were there; also Mr Moodie, Mr Brunton, Mr Miller, and Paterson. I saw Mr and Mrs Kilgour go away, but not the rest. Was in bed before that. On the New Year's Day, papa and mamma, the boys, and I, went out in a cab to go to the pantomime. We called at St James's Square, and papa went in to call for Paterson. Papa stayed some time, and then came back. We went to the theatre, but papa said the pantomime was too crowded, and so we didn't go in. Paterson was generally very kind to me. He gave me an orange one day. The day after was a Sunday—the Sunday before Paterson was killed; it was in the room behind the shop.

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[1] Margaret Finlay. [2] Bruce Allan, Miss Forrester. [3] Bruce Allan, Miss Forrester, Dr Hearder.

[4] The wound was thus described in the medical report of the post-mortem examination. Externally, it was a longitudinal slit about an inch in length, having its upper end slightly more inclined to the left than the lower, while the centre gaped considerably from the retraction of the edges. It was situated about two and a half inches below, and to the inside of the left nipple. It was found to penetrate nearly directly into the chest, with a slight inclination upwards. After passing through the skin and subjacent muscles, it completely separated the cartilage of the sixth rib, passing into the cavity of the chest through the central pleura; it then passed into the reflection of the pleura, severing the pericardium, and, dividing both these membranes, it entered the anterior part of the heart, near the apex of the right ventricle, traversing the cavity, and passing through the anterior walls, until it just reached, without dividing, the serous membrane covering posteriorly the outer surface of the heart. The heart was firmly contracted, and its cavities empty, while the cavity of the pericardium was distended with slightly coagulated blood. The cellular tissue connecting the pleura and pericardium was infiltrated with blood, and a considerable quantity had also escaped into the left cavity of the chest.

Papa was up. Paterson staid a good while at that time. Another day, Willy went with Paterson for some cigars. When they came back, papa was lying on the sofa sleeping. On the Monday that Dr Sidey came, I was sent for medicine after the doctor went away. I brought the medicine back in a bottle. Paterson was there: he took up the bottle and looked at it. After Paterson went away, papa said there was poison in the bottle. He said Paterson had put poison in it. He didn't take that medicine. I was sent for another bottle of medicine, and papa took that. On the same day, papa said that Paterson poisoned him with quicksilver. He said that Paterson had poisoned me with quicksilver too. Paterson never gave me any quicksilver. The night of the day I went for the medicine, I heard a noise at the front door. Papa and mamma were not in bed. Papa said there were thieves. He cried out thieves, and went into the front shop. Mamma sat down on the sofa. We were all frightened and crying. Papa stayed in the front shop a wee while, then came back, and went down stairs. I fell asleep, and don't know when he came up again. On Wednesday, 7th January, when I saw papa coming down the stair I went upstairs. Papa was white in the face. He had on his topcoat and a cap. He had nothing in his hand. *By the Court.*—Papa kept the shop the day before Paterson was killed the same as usual. He was two days in bed a few days before Paterson was killed. Was in bed all the Sunday before.

*Bruce Allan* (after detailing the deceased's staggering up into Forrester's shop).—After this the prisoner came out of his shop and came up to me, saying, "The fellow has been poisoning my wife and my children: I have got him in bed with my wife." He appeared to be calm and sober. I put my hand on his shoulder and pushed him gently into his shop, saying, "Go into your shop, sir, and don't attempt to escape." He said, "I'm all right, sir," and went into his shop quite quietly. I was again at the door of Forrester's shop. I was standing on the landing, immediately after the death of deceased. The prisoner came to me and said, "Let me in to speak to him." I asked him Where did the man reside? He said, "St James's Square." I said, "You can't go in here. You have made a pretty job of the man: he is unable to speak. Go into your shop, sir, till I get some one to take charge of you." The prisoner said, "All right, sir," and walked back into his shop. He was just in the same state as when I first saw him—calm and sober. I went for the police; got John Stewart, a constable; took him to Forrester's and showed him the body. I then took him to the prisoner's, and said, "There is the man." The prisoner was then behind his counter. He said, "All right, sir." I then left. *Cross-examined.*—When the prisoner spoke to me the first time I thought the statement he made as to deceased was true. This was from the calmness of his manner. From this impression I looked to see if deceased's dress was all right, and I found it was so.

*Margaret Rogers*, a girl who was in Forrester's shop at the time.—After Dr Hearder came, he told me to get the deceased's address. I went into the prisoner's shop. I saw the prisoner and asked him. He said it was No. 5 St James's Square or Street—I can't say which. He said it twice over. He was very white. Nothing else passed.

*Dr Hearder* (after detailing circumstances attending Paterson's death).—I saw the prisoner a few minutes after the man died. He was in his own shop. I went there. The constable was there. This was the first I had seen of the prisoner. He appeared to be quite calm and collected—pale—to all appearance sober. I asked the constable if information had been sent to the office. He said he couldn't leave the prisoner. The prisoner said he would go at once to the office with the constable.

*John Stewart*, the constable.—Was taken on the 7th January to Forrester's shop by Mr Bruce Allan, and saw the body of deceased; then went to the prisoner's shop. No one was there but the prisoner. He was just standing inside his counter. Mr Allan said, "There he is," and then went away. I said to the prisoner, "What's happened?" He said, "I gave my working jeweller a prog." I said, "What with?" He said, "With a dagger." I said, "Where is it?" He brought it from about his person. I grasped at it, but he drew back, and I missed it; but the second time I succeeded in getting it. It was in the sheath when I got it from him. He commenced talking of the deceased poisoning him the last two or three days,—poisoning his drink, to try and get his business and his wife. He spoke two or three times about it. He said he had found him on the top of his wife on Monday; saying he was a blackguard, for running after married men's wives. He asked me several times where Paterson was,—if he had gone home or what? I didn't tell him. M'Cabe came and another constable. The prisoner said, "I didn't mean to give him much, but I meant to give him a touch." It was ten minutes after Mr Allan left before the other constables came. The prisoner was on one side of the counter all this time and I on the other, walking up and down. He seemed sober. Nothing in his manner struck me as odd. He appeared quite cool and calm, and spoke quite in an ordinary way. He did not ask if deceased was much hurt. We then got a cab and took prisoner to the office. On going into the cab, he gave me a shilling to pay the hire. *Cross-examined.*—There was no smell of drink about the prisoner. He had on a Highland cloak, a blue Glengarry, and black leggings.

*Andrew M'Cabe*, a constable.—I went to the prisoner's shop on 7th January last. The prisoner was there; also Stewart. The prisoner asked Stewart if the man was much hurt. We took him to the police-office in a cab. I asked prisoner what he had done to him. He said he had stabbed him. The prisoner said deceased had laid his wife on the sofa and got a-top of her. This was not in answer to any question. I think he said it more than once. *Cross-examined.*—I smelt no drink on the prisoner, and considered him to be sober. I remember prisoner saying something about deceased poisoning him, but did not pay much attention to it. He said deceased had dropped or slipped something over his drink.

*William Cowan*, lieutenant of police.—I was on duty at the police-office on 7th January, when the prisoner was brought in in custody. He was asked his name and some particulars as to his business, shop, etc. He understood them all, and answered distinctly. I asked Dr Littlejohn, in presence of the prisoner, what injuries deceased had received. He mentioned them generally, and said that Paterson was dead. The prisoner said nothing. It did not in the least agitate him, nor did he look surprised. I asked him what he had got to say about it, and whether he and Paterson were on intimate terms. He said, Paterson used to come about him; that he had been confined to bed for some time, and that Paterson had been in the habit of coming to get orders about work. He said Paterson had had intentions upon his life; that he had first been aware of this the night of Monday after Christmas; that that night Paterson was having some refreshment in his room; that Paterson had taken some mercurial stuff and filled the room with a dense gas or vapour; that he (prisoner) felt he couldn't breathe—felt sick, confused, and sleepy; that he had seen Paterson take up his little daughter and put a paper in her nostrils; that he then got up to look for his wife, and after some time saw her on the opposite side of the room with Paterson on a sofa. I asked, How with Paterson? Was he using liberties with her? Prisoner said, Yes, he was on the top of her. He said he couldn't call out or do anything. He said Paterson had put poison in his drink; he had seen him put white silver-like stuff and small black stuff into it. Holding out his hand he said, "See, I'm poisoned; my whole body is poisoned." He said Mrs Milne had expressed a preference for Paterson; saying, he was a better man than her husband. He said, that after that Monday night Paterson kept out of his way, and sent his man his messages; that on that morning Paterson's man came about some work; that he said to him, "Why doesn't Paterson come himself? Tell him to come." That Paterson did come, and he said to him, "What's this about my wife? What have you been doing with her?" That Paterson just gave a derisive laughing Ha, ha! ha, ha! that he could stand it no longer, and gave him the stab. I showed him the dagger, and asked him if that was the article. He said, "Yes, it is" During this time the prisoner was quite calm; he hesitated a good deal in saying this. Sometimes he said half a sentence, and then seemed to have a difficulty to get words to finish the sentence. There was certainly no excitement about the prisoner. Nothing was said by him as to whether he intended to hurt the man. This was all that passed, so far as I remember. I took a note of all this within an hour after it was said, and have referred to this note in giving my evidence. I thought the prisoner was sour and sullen, but saw nothing else remarkable in his manner. I had no reason to doubt his intelligence. Every question I put he understood, and gave a distinct answer to. He did not wander to any other subject. He took from ten to fifteen minutes to make the above statement. I observed no restlessness in the prisoner's manner. I have seen prisoners brought to the office under delirium tremens. There was nothing in the prisoner's manner the least like that. *Cross-examined.*—I asked prisoner if he had been drinking. He said, "No." Dr Littlejohn put his nose to his mouth and said he had. The prisoner admitted that he had had some brandy and water. The prisoner was not under the influence of drink. Dr Littlejohn was present all this time. I don't think the prisoner mentioned anything more than once. His story was quite connected, and followed out in detail. I believe the word "blow," or "blown," was used in connexion with Paterson filling the room with gas. I understood him to mean that the gas had made him unable to call out or do anything. He may have used the word quicksilver.

*Dr Littlejohn*, surgeon of police.—I was called to Forrester's shop on 7th January, and saw deceased there. When Milne was brought to the police-office, in custody, he had a pale appearance. Was languid and depressed, just as if he had been recently drinking. He put his hand on the bar. I noticed a tremulous motion. Dr Littlejohn then gave an account of the prisoner's statement to the lieutenant, which is the same as that given above. The prisoner perfectly comprehended everything that was said to him. When he was about to make a strange statement he hesitated for a few minutes, as if pondering what to say. I thought it curious. There was nothing else in his mode or manner that struck me as peculiar. He was not excited or restless. I felt his pulse—it was 90, quick and weak. I have seen many cases of delirium tremens. I suspected prisoner was about to enter a state of it. His peculiar air of languor, his pulse, the smell of spirits, and the trembling of his hand made me think this. There was no symptom of delirium tremens at the time. I can't say whether there was any appearance of delirium tremens shortly before. I think I can say positively that he had not had an attack of delirium tremens during the twenty-four hours immediately preceding. I saw prisoner in his cell that night. He was lying quiet, and I did not disturb him. He was put on ordinary prison diet, milk and bread. I saw him next morning at a quarter before ten. He was greatly better. His face had lost the appearance of depression and paleness. His hands were no longer tremulous. His pulse was about 75—still weak. I understood he had passed a quiet night. He knew me perfectly. He answered the questions I put to him intelligently, and said that it was entirely by mistake that Paterson was wounded. He was going to give further explanations. I warned him not. He persisted in doing so, and said, he had had the dagger in his hand, and was aiming some blows at a post at the top of the stair in his shop; that he had sent for Paterson that morning about some work; that seeing Paterson enter his shop, he had told him to come in where he was; that Paterson came quickly in, and that the dagger or knife accidentally entered Paterson's body. The prisoner's manner at this time was much more firm and collected than on the preceding day. There was none of that hesitancy which I had noticed before. I attributed this to the quiet night he had passed. I reported that he might be examined. Dr Littlejohn then proved the report of the post-mortem examination, adding, that the wound was necessarily a mortal wound. *Cross-examined.*—I suspected, when I saw prisoner on 7th January, that he might pass into a state of delirium tremens. He had some drink in him, but was not tipsy. He was not excited with drink. He was rather depressed than excited when I saw him. The prisoner said, that at the time of the vapour or gas, it had been surging about. He said distinctly that Paterson caused the vapours to surge about the room; that they were occasionally dispelled, and then he saw the occurrences which took place. What do you mean by the hesitation you spoke of?—When I asked what he meant by poisoning, he hesitated before he spoke of the vapours, as if he was trying to recollect. I can't say whether the prisoner assigned any reason why he had bought the dagger. I took prisoner to see the body of Paterson on the morning of the 8th, before he was examined. He was much moved. He shed tears, and wrung his hands, saying, Oh, dear! oh, dear! Is it come to this? He seemed much affected. I granted a certificate that prisoner might be examined. It is not usual to do so, but was thought right in this case, as it was alleged that prisoner had been drinking for some time before. The prisoner said, that Paterson had been avoiding him since 29th December. Last Wednesday—4th February—I visited prisoner in gaol by desire of the Procurator-fiscal. The prisoner was brought to me, and asked if he knew me. He said he thought I was a doctor, but didn't recollect who I was. My name was mentioned, and he then went over the different times he had seen me before. He then made statements similar to what he had made on the 7th, but giving a different account of the way in which the wound was inflicted. He said the same thing as to poisoning, and added, that on the Monday night prior to Wednesday, 7th January, Paterson had attempted to rob his shop. His statement was a little fuller in detail than before. He said, that on the Monday night he heard a noise at the door; that he distinctly heard Paterson's voice, as if coming from a crowd outside, and was much alarmed I said he could hardly believe that of a person whom he considered his dearest friend. He said there was some mistake. I then said, Why didn't he forbid Paterson his house—a man who was doing him so much injury? He was silent, and hung his head. I thought this curious. I asked him how he could believe all these things of Paterson? He said they were real occurrences. Last Friday I saw him again along with Dr Inglis. We put to him the inconsistencies of his statements as to the manner of Paterson's death. His statements were not so full as before, but agreed with those of preceding Wednesday. *Re-examined.*—The prisoner evidently knew last Wednesday and Friday why we had come. He knew he was to be tried to-day. He spoke of it on Friday. There were no symptoms of delirium tremens about him. He did not on these occasions speak so strongly of his jealousy of his wife. On the Wednesday he merely repeated what he had seen Paterson do to her, but said nothing as to anything she had said. On the morning of the 8th, when he gave the new account of the occurrence, I said, Then your poor wife had nothing to do with it. He just gave a contemptuous smile, as if acquiescing in what I said. No other allusion was made then to the account he had given the previous day. Was there anything about the prisoner indicating to you, as a medical man, mental unsoundness?—There were some peculiar symptoms about him. I could not have certified then that he was fit for examination. By the certificate I granted next day, I intimated my opinion that he was of sound mind. On Wednesday the 7th, he seemed to me to have been drinking hard. I am quite satisfied that there was no delirium tremens on Wednesday, and my apprehensions that it might come to that were quite removed on the 8th. I am satisfied that he had not suffered from delirium tremens on the 7th, or the day preceding. Did you see anything to indicate that he considered it wrong to stab Paterson?—He seemed to consider that he had received sufficient provocation. *Cross-examined.*—On the 7th, did you think the prisoner believed that he had really seen what he said he saw?—I cannot say. Did you think he was making up a story? I said before, that I was struck by his hesitancy in making the statement. It is not my opinion that he was making up a story. Do you think it is possible, that if the prisoner were under delusion at 7th January, and was afterwards treated with ordinary prison diet, he might come to be under permanent delusion?—If he had been drinking for weeks before I would not be surprised to find him so a month afterwards. I account for the clear manner in which Milne continues to repeat his story of those strange occurrences, by the fact, that there was a sudden withdrawal of the stimulus to which he had been accustomed during the preceding weeks. If they were delusions at 7th January, it is not unnatural that he should remember them a month afterwards. *By the Court.*—In this I assume that he now only remembers them. I did not think the symptoms I observed in the prisoner were the result of a past fit of delirium tremens. They were symptoms of hard drinking, which had gone so far that it was not impossible they might end in delirium tremens.

The declaration of the prisoner was then read. In this he gave the same account of the death of Paterson as he had given to Dr Littlejohn on the morning of the 8th, ascribing the death solely to the result of accident. This closed the case for the crown.

The following evidence was adduced on the part of the prisoner:—

*William Hamilton*, of the Money Order Office.—The prisoner came to our office about eleven on Wednesday forenoon, 7th January, and got payment of an order for £2. He was then quite sober. There was something very piercing in his look. He looked me very hard in the face, and seemed very strange.

*Robert Moodie*, a friend of the prisoner's since 1853, gave evidence as to his conduct during that period, mentioning various occasions on which he had showed great excitement from drink,—in particular on the occasion of his bankruptcy in 1860, when he seemed much excited. He also corroborated the account of Milne's Christmas party given by

*Thomas Kilgour*, who deponed,—I went to the prisoner's last Christmas party. When my wife and I arrived, he said to me, in a military strain, "Now, sir, stand you there." My wife was taken to the back-room. He looked rather peculiar, and not at all like a party who was to receive guests at Christmas time. He appeared to have been tasting, as he smelt of drink. He was excited, and there was a wild roll in his eyes that I didn't like. Throughout the evening it struck me there must be something else wrong with him than drink. The rest of the company, on their arrival, were treated in the same way as I was. We all thought it strange. I was not casting glances to any one. I could not do that without Milne seeing me, because we were all standing in a line. He afterwards brought a sword, and said, "Gentlemen, you will march to the supper-table to the tune of the 'Merry Masons.'" We went in that way just to please him. I did not think I was making a fool of myself, because I can make a little allowance at Christmas time. It was all done to gratify Milne. After descending the stair, we walked three or four times round the kitchen, and he then kicked the door of the supper-room, which flew open, when he exclaimed —"Gentlemen, this is the grand transformation scene of the pantomime." I thought this was just carrying out what he had begun. There was an extraordinary blessing asked by Milne at supper. I did not believe that he was intentionally irreverent. It produced a very disagreeable feeling in my mind. When I heard Milne asking God to look down upon us assembled in a fairy bower, my blood run cold. The impression it made upon my mind was, a man can't really say this in his sober senses. He was sober apparently, and I began to doubt whether he was in his right mind. He cut up the first dish at supper very peculiarly. It was a Scotch haggis, and he completely smashed it before serving any one, and manœuvred with the knife. There was a damper cast upon the company by the blessing, and we were all startled at it. I was sitting at Milne's right. During the evening he caught me by the head, and said, with an Irish accent, "Cry murder, then." I did so; and he let go and laughed heartily. I took that as a piece of fun. During the evening Milne went into the kitchen and brought out the old sword, and Paterson and he had a fence with a stick. I interfered, and he immediately gave up the sword to Mrs Milne, and never asked for it again in my presence. I did this because I dreaded the consequences. I said at the time when he gave up the sword, "He has acted exceedingly well *for Milne*, but I cannot get over the blessing." At another time there was an apple on the table, and, taking the sword, he cut the apple in two upon the plate. This appeared to be a piece of mere folly. It seemed to be fun to him. When the fencing with the sword took place, I and my wife left his company, because we thought, considering what was taking place, that we would be better absent than present. He was not what I could call drunk, but at the same time he had been drinking. He was under the influence of drink. I would not have thought that all the drink in his head which I had seen him drink that night would account for his conduct on that occasion. He was much the same at the end of the night as he was at the beginning. None of us were drinking much, but he took his drink with the rest of us. I do not know exactly how many tumblers of toddy he had, but I know that I had only one. In marshalling us all as he did, he was pompous and serious. I saw him again on Saturday the 3d of January. He was then in bed. His eyes seemed to be quite glazed and wild. Paterson and he seemed to be on good terms. On that occasion he said there was no one he thought more of than Paterson. He made no complaint that Paterson was not dealing fairly with him.

*Dr Charles Sidey.*—I was called to attend the prisoner on Monday, 5th January. I went through the shop to the back-room, where he was in bed. I asked him what was the matter. He replied that he felt unwell, and that he suspected poison had been put into the water. He did not say who he suspected. I told him then that he had been drinking. He was quite calm then; he had no feverish symptom about him. He had no symptom of delirium tremens. I prescribed a camphorated mixture, with a little tartrate of antimony. He asked me to come back next day, and I told him he was not worthy of attendance. He would not do anything I prescribed for him. Mrs Milne was in the room when I was there. She asked me to come back. When he spoke of the poison, I said he surely did not suspect his wife or his children, and that he should turn his suspicion towards himself. I never saw the prisoner since that time till yesterday. *Cross-examined.*—I meant by saying that I thought he had taken the poison himself that he had been drinking too much whisky. When I saw him on the 5th, I had no doubt that his illness was caused by excessive drinking. He promised, when I first visited him, to give up drinking. When I charged him with drinking, he said he had not been doing so since Christmas. I appealed to his wife, and she confirmed my impression. I think he made the remark about the poison before I charged him with drinking. He appeared to be quite intelligent. *Re-examined.*—His hand was quite steady on the 5th of January, his tongue was clean, and there were no symptoms of fever. *By the Court.*—I saw no appearances of insanity on the prisoner when I visited him.

*James Laing.*—I am a light-porter. I have known the prisoner for five years. For the last four years I have put on and taken off his shutters. His shop was in George Street when I went into his employment. I remember putting the shutters on on the night of Monday the 5th January. Milne came forward to the door from the back-shop. He had on his Highland cloak, and was bareheaded. He said to me, "Whisht." I said, "What, sir?" He then said, "Whisht; don't you see the robbers? they're planning to break into the shop." He only said this once. He looked out to the street as if he saw somebody. I also looked, but saw no person on the street at the time. He did not tell me who the robbers were. He said they were trying to poison his wife, his family, and himself, and that they had taken a little of the poison, but it had no effect. The shutters are kept in the area. After this I went down for the shutters, and he said to me, "Watch them." I understood he meant me to watch the robbers. He stayed at the door when I was down. He locked the door. I did not smell drink about him. I had not tasted any that day. I was struck with the look that Milne had that night. He looked very wild and strange. I never heard him speak about robbers or poisoning before that time. When I came next morning there was a great fumbling inside. Both the prisoner and his wife were at the door when it was opened. He had on his trousers, his shirt, and boots. It was half-past nine in the morning. Milne said—"They've been here, but they've not got in." Nothing more passed. I put on the shutters at night. In the evening Mrs Milne showed me an iron bar that had been made to go across the door. She said that would surely please him now. I never saw the prisoner in such a state as he appeared on the Monday night. *Cross-examined.*—I could not say he had been drinking hard at that time. I saw him a little hearty at Christmas time. So far as I know, he had not been drinking hard. He was always merry when he was a little hearty. He was never solemn or serious. I think he was quite sober on Monday the 5th. I attributed his wild appearance that night to what I had heard about his drinking. He was moody and silent during nearly all the time I was with him. He was often very sulky.

*Alexander Forrest.*—I am a blacksmith in Rose Street. I know the prisoner. He came to my shop on Tuesday the 6th January between eleven and twelve o'clock. He said he wanted a strong bar for the back of his shop door. He said he suspected his shop had been attempted to be broken into the night before. He did not say he suspected anybody. He was sober, and appeared to be serious about the bar. I went with him and took the measurement. Paterson came to the door when I was measuring the bar. I knew Paterson by sight. I said that Paterson should come in and give us his opinion about the bar. Milne said to Paterson, "You won't enter this door." He repeated that more than once. Milne also said to Paterson, "If you come in here, you must come through the door." He appeared to be serious when he said that. Paterson just gave a smile and left. He had been at the outside of the door all the time. Milne then said to me, "I am suspicious of him and another." He did not tell me who the other was. We had some consultation as to how the bar should be put on. I made the bar that day, and put it on about four o'clock. Milne was there at that time. He said he thought the bar would do. He then asked me into the back-shop, where he gave me a glass of spirits. I did not notice him take any. He said he had his suspicions, and, raising his finger, said there was a spirit which he used which enabled him to see when other people could not see. I saw Paterson just after I left Milne's shop. We went into a public-house. I asked Paterson why he and Milne were not so friendly. Paterson said he did not know. I then told him that Milne suspected him and another of attempting to break into his shop on the previous night. He replied—"That would be the last of my thoughts." He added that Milne had been in delirium tremens since Christmas. Paterson told me to put on the bar soon, to please Milne. *Cross-examined.*—Milne did not appear to be excited on any of the occasions on which I saw him. He gave his orders to me quite intelligently. There were no marks of violence on the door, but there was a footmark on the outside. I have known the prisoner two years. I think the bar was necessary for the door. I made the prisoner a present of a sword about four months ago. I showed him it in my shop, and he asked me for it. He seemed to covet it very much. He took it in his hand and swung it round about his head. *By the Court.*—There was nothing odd in the manner of his ordering the bar. His is just the kind of door requiring such a bar.

*Janet Forrester*, who kept the baker's shop next door to the prisoner.—I remember of Mr Milne coming into my shop on Tuesday the 6th January. He asked me if I heard a noise at his door the night before, and I said I did, but I did not open my shop-door to see what it was. The noise I had heard was that of some one kicking either inside or outside the door, about nine o'clock at night. It did not enter my head that the shop was being broken into by robbers. I was not alarmed by it, nor did I give myself any trouble about it. He said that his shop was going to be broken into, and when I asked him why he thought so, he said, "The spirit told me so," knocking his hand against his forehead. I went into the back-shop, and Mr Milne went away. That was all that passed at that time. I did not notice any signs of drink about him. When he said the spirit told him his shop was going to be broken into, he talked quite seriously, as if he had believed what he said. I never heard anything against the character of the wife of the prisoner. I believe her to be a respectable woman. *Cross-examined.*—I do not know whether the prisoner attended to his own business or was much in his shop. I have been served by Mrs Milne when I went in for change. On the day before this calamity, when he came into my shop, there was nothing unusual in his manner. I did not pass much thought about the noise I had heard at his door, for I was very busy writing, but it struck me as unusual. He gave no explanation of the noise, but just asked me if I had heard it.

*George Smith*, son of Mr Smith, of Smith and Philpot, and clerk to the firm.—The prisoner came into our shop on Tuesday the 6th January. He had a parcel of jewellery with him, which he wanted put into our safe, because he thought there had been robbers at his door. We thought this was all a delusion. My father, the cashier, Mr Irvine, and Mr Philpot's son were present at the time. My father asked him, where were the police? He said that if he came out they would kill him. He said he knew they would be back again, and he was going to get strong iron bars put on his door. I thought this was a delusion. We took the articles to please him, and get him away, though we did not think there was any reason for doing so. We made an inventory of the jewellery, and gave him a copy of it. He was quite serious in speaking of the robbers. We tried to put the idea out of his head, but it was of no avail. I did not think he was in his right mind. I remember of his coming to our shop on Wednesday morning about ten o'clock. He was dressed in a brown Highland cloak, with a pair of leather leggings, and a Balmoral bonnet. I was reading the newspapers when he came in. He came close up to me, and sat down beside me. I smelt no drink about him. He said his wife and another man had been attempting to poison him, and he wished some one to come and look at his stock, as he wanted to dispose of it. He said they had been trying to poison him all the night before. He said he had caught the man shaking a powder into a tumbler of water he was about to drink. He said he wished my father to come over to look at the articles he had in the shop, for the purpose of valuing it. I thought his story was all bosh. My father came in about eleven o'clock. Milne left the shop about a quarter past ten. He did not stay above a quarter of an hour or ten minutes. When my father came in I told him what Milne had said. *Cross-examined.*—Milne had been in the way of coming about our place for about eighteen months. There was a peculiarity in his manner on that Wednesday morning. He was excited. I had once seen him similarly excited one evening about the end of November. He brought over a brooch, and wanted £25 advanced upon it, which we would not give him. Then he came down to £10, to £7, and then to £3. We said we would have nothing to do with it. He then took the brooch and threw it violently across the counter in a great passion, and one part of it was broken off. A gentleman came into the room, and Milne went away. I never saw him behaving violently before. I did not think Milne had anything odd in his manner usually—he was just like other people. That night he came with the brooch he had been drinking. His excitement on Tuesday the 6th January I attributed to drink. I never thought of any other cause. I attributed his story about the robbers to the effects of drink. He was not violent on the 6th nor on the 7th of January. He was quite calm. He sat beside me, and told me this in a low tone. He was rather serious, but he did not seem to be frightened. I understood his story to be connected with his desire to have his stock disposed of. I thought at the time that he himself believed what he was saying, and was not attempting to impose upon me. I thought it bosh, not because he himself did not believe it, but because there was no foundation for it.

*William Henderson*, accountant in the British Linen Company's Bank, deponed to the prisoner coming to the bank on Wednesday forenoon, 7th January, about eleven o'clock, and getting payment of a cheque for £5. There was nothing peculiar about his manner.

*John Wight*, prisoner in the Prison of Edinburgh, was placed in the same cell with Milne the day after his apprehension. Mr Nelson told me prisoner was a little wrong in his mind, and he wanted me to go in beside him and keep him quiet with a little conversation. Milne was very quiet all day. When I went in he asked me what I was in for, and I told him. During the day we had little or no conversation. He was quiet. He went to bed at the same time—about eight o'clock. After the gas was put out, he lay quiet the forepart of the night; then he got up and came to my bed. He felt all over my body, but didn't say anything; then he went back to his own bed. Then he got up in two minutes after, and ran to the door, and cried—"Murder! murder! Oh, my wife and children!" I lay still for a couple of minutes, and then I got up and got hold of him, and he was all the while calling out "That they were murdering his wife and children." He repeated this over and over again. I got up; he was all shaking when I got hold of him. I tried to soothe him, but did not succeed. The turnkey came down, and the night watchman came down with a light. When he appeared, Milne called out to him that they were murdering his wife and children. His eyes rolled in his head. He got hold of the watchman, clung to him as if for protection, and still kept crying out that they were murdering his wife and children. The watchman removed him to another cell. In the morning he was brought back beside me. He was then perfectly quiet. Another prisoner, named Martin, was put into the same cell that day, and the three of us have been in the same cell ever sinsyne. On the Saturday I was lying very composed, watching him, and he got out of bed again, his eyes rolling in his head, and cried out that he wanted some morphia, and then he lay down again and began weeping. Nothing else happened that night. We got him quieted. The next week Milne began looking at the iron rods that were in the cell for hanging the beds on. He took the nut off one of them, and said, "This comes out." I said, "I know that." We went out for a walk in the airing-ground, and I told the turnkey to take these rods away, for fear he would do something with them. I thought he was wrong in his mind. I was a little frightened at him at first, and thought he was not to be trusted. He told me his shop had been robbed—that some policemen came and knocked at his shop-door; and then he said that they weren't policemen, but that they were Mr Paterson and one Mr Kilgour. I said that was nonsense, and he said he knew Paterson's voice. He said it was Paterson and Kilgour dressed as policemen. He said they wanted to break into the shop. He said also that Mr Paterson was poisoning him. This would be on the Thursday or the Friday of the first week—about a week afte he came to jail. He said that Mr Paterson had given him a poisoned cigar, and that he put some stuff into the cup he was drinking out of, and that Mr Paterson chewed something in his mouth and blew a gas from it all through the house. He said that in consequence of the poison Mr Paterson gave him the skin was coming off his feet and his hands. I looked at his feet and hands; there was some scurf on them, but nothing to speak of. He said the powder that Paterson blew through the room dimmed all his eyes and coloured all his wife's face,—that he saw Paterson give his wife a signal, and she went downstairs, and Paterson followed her; and that Paterson then had connexion with her. He said all this was the effect of the vapour. *Cross-examined.*—I have been in the same cell with the prisoner all the time till now. There is no difference upon him since he was first put in. On the night I have referred to, Milne got up quietly and felt all over my body, and then he went and lay down on his bed. There is no light in the cell, and I could not see what he was doing. It was pitch dark. He was pretty quiet all next day. He was well enough behaved. I asked him what he thought of his carrying on the way he was doing last night, and he said, "Its all owing to dreaming about my wife and children." He said he had dreamt that some person was there murdering his wife and children. Next night he got up and cried out for morphia, and behaved as I have already described. There was a light burning in the cell that night, and there was another prisoner named Martin there with me. I never asked him about that night's behaviour. I thought he had been dreaming again. In his conversation with me during the day he always spoke about murdering his wife and children, and about Paterson poisoning him. He called it a dream about murdering his wife and children, but he did not say that Paterson poisoning him was a dream. He said he saw in his dream the people murdering his wife and children, but he did not say who the people were. He said when he was awake he saw them quite distinctly. I had not been speaking to him about Paterson. He told me that he was there for murder, on the Saturday of the first week. He did not say who was murdered, and I did not ask. I did not know otherwise. I asked him on the Monday or Tuesday following who was murdered, and he said it was Paterson, but that Paterson was not murdered, and that the police were keeping him locked up out of sight, and that after he was condemned Paterson would come and marry his wife and get the business to himself. I said that was nonsense. and he said it was not nonsense, but that it was truth. He told me he was standing in his shop, playing with a dagger he had bought, when Paterson's man came for some work, and that he told the man to tell Mr Paterson to come himself for the work. When Mr Paterson came he told him to stand back, but instead of standing back Paterson rushed forward, and that he touched him with the dagger, and that Paterson then went down stairs. He laid the dagger on the table, and there was no blood upon it, and went after Mr Paterson, and when he got to the door he saw Mr Paterson jumping over the area. He also said he went into Mr Forrester's shop, and saw Mr Paterson there, and then went back to his own shop; and when the policeman came to the shop he gave him the dagger. He also said he saw Mr Paterson's corpse lying in the police-office. He was quite firm when he told me this. He told me first that Paterson was not killed, but that the police had him locked away, and at the end of this long story he said he had seen the corpse in the police-office. He then commenced dancing and singing. He told me if Paterson was hurt at all, it was an accident. He said that Paterson was his friend, that he liked him very well, and that they used always to go about together, to the theatre and other places. He said so on the Tuesday, and again through the week, down to the present time. He seemed to mean that because Paterson was his friend he could not have hurt him intentionally. He never mentioned any ill-will against Paterson. He never spoke to me as if Paterson deserved to be punished for what he had done. He did not speak as if he had done right in stabbing Paterson,—he always spoke of that as an accident. He first told me about the vapour and of Mr Paterson having connexion with his wife either on the Wednesday or Thursday following the day he was put into prison. I am quite sure it was not in connexion with the stabbing that he mentioned about the vapour, and about Paterson and his wife. He seemed to understand what he was saying when he told me so. He still speaks of the stabbing as an accident, and whenever he speaks about Paterson he calls him his friend. He never spoke of Paterson as a man who had done him an injury, except in that story about the vapour, and what followed. *By the Court.*—He frequently repeats, even down to the present time, that Paterson is not killed, and is locked away by the police. He never again mentioned about having seen his corpse in the Police-office. When the prisoner told me that Paterson was not killed, and that he would get his wife and business, I did not remind him that he had seen his corpse in the Police-office. I thought I had no right to do so in the state he was in. On other subjects he speaks quite rationally. These conversations occur between us suddenly. After telling me that, he begins to dance and sing. I am under a charge of horse-stealing, but have not yet been tried.

*George Martin*, another fellow-prisoner, who had occupied the same cell with prisoner since his apprehension, corroborated the statements made by the prisoner as to his delusions, adding,—The prisoner told me that Paterson had been in his cell, and told him he was in business in the South Side, and drawing £5000 a-day. I often spoke to Milne about his trial, but he did not seem to take any notice of his own trial. He was more concerned about my trial than about his own. *Cross-examined.*—I think he knew what he was to be tried for. He said it was for the murder of Mr Paterson. He said that several times. He said it last Thursday. I think he was serious in saying that Paterson had been in the cell with him, and that he was in business in the South Side. He mentioned also that he had a grand banquet in his house, and that they had a grand march through the shop and down again. He said the room was all hung with holly leaves, and that he was showing the company his swordsmanship, and that he had cut an apple in two through a wine-glass, and handed a slice of the apple to a gentleman on his right; that he sung the song of the "Star-Spangled Banner." *By the Court.*—I think Milne still believes that Paterson is alive, but at other times he speaks of him as dead, saying, "Poor Paterson."

*Hugh Mackay*, warder in Calton Jail.—On Friday night, 9th January, I heard a great shouting of murder and kicking at the cell door. I opened the door, and saw the prisoner clinging to Wight and shouting murder, and he also took hold of me by the shoulders, and said, "Oh, will you protect my wife and children?" Wight appeared very much frightened. The prisoner said he had been visited by three men, who said they were doctors, but he believed they were conspirators, for there was a conspiracy on foot to murder his wife and children. He asked me to get the police, and I told him that I would get the police to protect his wife and children, and so removed him to a padded room. He was quite awake when I saw him. The padded room is kept for prisoners who are insane or labouring under delirium tremens. He immediately sat down on the padded stool, and remained there for some hours awake. He seemed in great distress both of body and mind. His eyes had a peculiar look, and he kept staring at me for several hours. About four o'clock in the morning he stood up, and came and examined the button of my coat. He then said, "My God, I know where I am! you are the jailer." He then went back to his stool. He again remained quiet for about an hour. He then stood up and folded his arms, and said, "My name is Norval, on the Grampian Hills." I gave him in charge of another warder at six o'clock. For the purpose of quieting him, I told him his wife and family were quite safe, as I had got a guard of police at his house. *Cross-examined.*—His appearance on subsequent days was quite different. He seemed composed and settled. On the Friday night I was beside him, I formed the opinion that he was labouring under a nervous state of delirium tremens. I have been in India, and have seen several cases of delirium tremens. *By the Court.*—The prisoner was not treated for delirium tremens. He was not in delirium tremens.

The Lord Justice-Clerk at this stage explained to the jury that it was their duty under the Lunacy Act, if in the course of the trial they should come to be satisfied that the prisoner was at present insane, to interpose and say so, and to find accordingly.

The following medical evidence was led:—

*Dr James Simson.*—I am surgeon to the prison. On 9th January last I was directed by the Procurator-fiscal to examine the prisoner. I was desired to take with me Dr Christison and any other medical gentleman well versed in mental disorders. The object was to ascertain the state of the prisoner's mind. I accordingly arranged with Dr Christison and Dr Smith of New Saughton Asylum, and we went together to the prison at four o'clock. I subsequently visited the prisoner on the 12th with Dr Smith, and on the 13th with Dr Christison. I saw prisoner again on the 14th, and granted a certificate as to his state of mind. I had seen prisoner on the morning of the 9th for a quarter of an hour about eleven o'clock. His pulse was small and quickish; his tongue loaded; there was no delirium tremens about him. There was a dreamy appearance about his eyes; he complained of a pain in the forepart of his head. I had a long conversation with him on general subjects. In the afternoon he spoke about the murder. Had a long conversation with him. Got the whole history from Christmas-day downwards. His accounts were much the same as those detailed by previous witnesses. I did not think that the prisoner was feigning in those statements. I have seen a good many cases of feigning, and try to detect them. I took the usual means to do so. I have since seen the prisoner again and again; he has always stuck to the same story. I tried to carry him further in his story, but he wouldn't go further. If you had frequent opportunities of seeing a man who was feigning, would you have difficulty in detecting him?—No; but I have seen a man persist in feigning insanity for two months, and then give in. I detected no symptoms of feigning in the prisoner. His health has improved in prison. If the prisoner is not feigning, but actually believes those things, do you think his mind is unsound?—Yes; he labours under delusions, and is of unsound mind so far. If, on the day of Paterson's death, he laboured under delusions, would you think him insane?—Yes, if under delusion. The prisoner hesitated a good deal in making the statements to us. He is better and worse both in bodily and mental health. At times he is clearer than at other times. Last Saturday he was not so clear, probably from the excitement of his trial coming on. *Cross-examined.*—I discovered nothing wrong with the prisoner's mental health but those delusions. Then, when you speak of his mind being clear, you mean free from those delusions?—Not altogether. In reference to the vapour, I said to prisoner, Do you believe that Paterson had power to do so? and he said, I certainly believe it, but I cannot understand it. Did you ever find the prisoner free from these delusions?—No; but he began to doubt them. When?—In the beginning of last week, he said he couldn't explain how it was, but there was a possibility he might be wrong. He was reading a book, "Macnish's Anatomy of Drunkenness," where delusions are spoken of, and he said he began to think his might be a case of that kind. To avoid mistake, what were the precise delusions you found in the prisoner's mind?—There were three or four delusions,—1. Paterson poisoning him with a cigar; 2. The vapour; 3. Putting poison into water; and 4. The conspiracy on Christmas-night. He said that during supper he heard Kilgour, Paterson, and Moodie make a conspiracy to break into his shop. It was another delusion that by the use of the vapour Paterson had power to make his wife consent to improper liberties. Did the prisoner tell you all these on your first visit?—Yes, every one,—that is, at four o'clock. Did he make his statement quite clearly and distinctly?—Oh, very. Did he seem to understand it all?—Perfectly. Did he answer the questions and give the information asked?—Yes. You knew what he was in jail for?—Yes. You would consider it your duty to try and find if there was any connexion between the act and the delusions?—Yes. Did you find any connexion?—No; he said it was accidental. Was he quite persistent in that?—Yes. And consistent?—Yes; he never varied. Have you ever been able to trace any connexion between the two?—None whatever. You were present in Court yesterday, and heard the account he gave to Mr Bruce Allan, to the constable and to the lieutenant of police?—Yes, I heard it. According to his account to them, he stabbed Paterson on purpose,—how do you account for that on the assumption of insanity?—When a monomaniac is taken up charged with a crime, he is apt to invent an excuse. I have seen this over and over again. Although you could not get the prisoner to connect the act with the delusion, do you yourself connect it with any delusion?—I don't quite understand the question. You did not get the prisoner, in your conversations with him, to connect the act with any delusion; but, in your opinion, was the act connected with any delusion?—If you mean my impression, I cannot tell. Assume that the stab was inflicted by the prisoner, and that it was not accidental, can you as a medical man form an opinion as to its having any connexion with any delusion of the prisoner's?—I can form no opinion upon it. Have you ever met with a case in which an insane man, well ascertained to be a lunatic, committed an act of murder and admitted it?—Yes. First admit and then deny it?—No. Have you ever read of such a case?—No. In the course of your reading, and in your experience, you never found a murder committed under an insane delusion first admitted and then denied, or attributed to accident?—No. I suppose insane people acting under a delusion generally give an account consistent with the delusion?—Yes. If he changed in the course of a night to accounting for the act as an accident, wouldn't that show he was not insane?—It would be an element. Could you account for it in any other way?—If he had been in an asylum. You say you saw no symptoms of feigning?—Yes. Is that not one?—Yes; it is not very easily explained. That would mean that he knew he had done wrong, and wanted to escape?—Yes; but it might be accounted for by his being a monomaniac, sane on all but one point. But a monomaniac is consistent?—Yes. You heard the prisoner's declaration read?—Yes. *Re-examined.*—I have had a good many opportunities of seeing lunatics. It is common for them to show a good deal of cunning. When not asked about their delusions, they frequently conceal them. It is often difficult to discover their delusions. They are cunning enough to let them remain unrevealed. If, the day when he made his declaration, nothing was said to touch upon the delusions, would he likely avoid them?—Possibly, if nothing was said about them. If he was feigning delusions to escape punishment, wouldn't his declaration be the very place where he would speak of them?—Very likely. Wouldn't the declaration be the very place to pretend madness?—Very likely. Is it at all inconsistent with monomania to have a temporary lucid interval.—No; it often is so. If the state of bodily health improves, may delusions depart altogether?—Yes, they may. Bodily health has a great effect on them. *By the Court.*—Did you at any time see symptoms of delirium tremens in the prisoner?—No, never. Did you hear the evidence of Mackay?—Yes. Was there any appearance of delirium tremens in his account of prisoner on 9th January?—No, none. What account do you make of the scene he described?—The explanation is either that he had been dreaming, or that, having had the previous delusions as to robbery, he might have thought his wife and children were murdered. Were these facts reported to you as surgeon?—Yes, the day after. I saw the prisoner that morning. He was well,—a little tremor about him as if he hadn't slept very well; nothing more. In consequence of his general health, I gave him some medicine. Did it not occur to you that what he suffered that night might have been occasioned by the withdrawal of a stimulus to which he had been accustomed?—No, not the excitement. Often, on the withdrawal of a stimulus, there is depression, but not excitement. Is delirium tremens not occasioned by the withdrawal of a stimulus?—There is a difference of opinion; I think not. Delirium tremens is the result, in my opinion, not of the withdrawal of a stimulus, but of a poison in the alcohol. Of course you would not prescribe alcohol?—No. You have seen the prisoner constantly since, what opinion do you form of his present mental state?—He is under delusion. That is not an answer.—In everything except those delusions he is quite sound. So far as the delusions go he is not sane. Is he as sane now as he ever was?—I think he is now as much insane as he ever was since I saw him.

*Dr John Smith*, consulting physician to New Saughton Asylum.—I have had considerable experience in the treatment of insane persons. I went with Dr Christison and Dr Simson on 9th January to see the prisoner, in order to ascertain the state of his mind. We had a long conversation with him. He made a variety of statements to us as to impressions on his mind. What was the opinion you formed as to his state of mind?—My opinion was that the prisoner was under certain impressions which seemed to be delusions, and which I thought would on investigation prove to be so. If they *were* untrue, he would be under delusions?—Yes. He believed them to be true. I saw the prisoner again on 12th January, and my opinion remained the same. Did you consider whether the prisoner was feigning?—Yes. My impression was that he was not feigning. Would you tell the grounds of that opinion?—The readiness with which he answered questions; his apparent simplicity; he used no apparent art, but there was a naturalness and openness in what he said; he was calm and composed throughout. If he labours under these delusions, is it your opinion that his mind is unsound?—It is. You were present in Court, and have heard the evidence of the different witnesses as to the conduct of the prisoner, etc., what is your opinion of his state at the time Paterson was killed?—My opinion is that the prisoner's mind was unsound. Does the statement made in the prisoner's declaration, taken in connexion with the other circumstances of the case, shake your opinion?—No. It is not an uncommon thing for an insane person to be sound on all points but one or two?—No. Is there anything in the evidence to make you hesitate in the opinion you have expressed?—No. *Cross-examined.*—I was about two hours with the prisoner each time I saw him. Do you consider him a man of weak mind or of good intelligence?—I think him a man naturally of fair intelligence. Was there anything whatever wrong with his mind except what you call delusions? —Nothing; everything else was right. Did you try to reason with him as to any of his delusions?—Yes. Which?—As to the conspiracy, the poisoning of his drink, and his wife. You of course had no means of forming an opinion as to whether these were true or not?—No. Or whether he believed them?—His mere asseverations. Did you ask him about Paterson's murder?—Yes. Did you find it connected with any of the delusions?—Yes, with the poisoning and with his wife. How so? —The course of examination led to it; it all bore on the murder. I thought the act of homicide was committed for the purpose of getting rid of the chief conspirator, and the man who had interfered with his wife. Was that from what the prisoner said?—It arose from the conversation. The point was whether he did it with that view or not. Did you ask him directly?—Yes. What did he say?—I brought it out. Did he say to you that he committed the act for that purpose?—Yes. What words did he use?—I can't remember. Did he say directly or indirectly that his motive was to get rid of a person who had committed these injuries?—I can't remember the words, but he gave me that impression. Did he not say it was an accident?—He said so. That wouldn't give you the impression above spoken of? —No. When he said it was an accident, did you think that he believed it was so? —I thought not. Did he ever say anything about the act of homicide but that it was an accident?—No; he explained the way it occurred. His account to you was not in accordance with what he gave in the police-office?—No. With the declaration? —Yes. These two are quite different?—Yes. They can't both be true?—No. Which do you assume to be true?—I have great doubts of the one he gave me; the other may be true; it is difficult to say. In your opinion of the prisoner's mental state, which do you consider true?—He might have murdered the man under a delusion, and then have said when it was put to him, some time after, that it was an accident. Did you ever meet with such a case?—It is quite possible. Did you ever meet with such a case?—Not in a case of murder. Any other?—any case in which he did the act, and then said it was an accident?—Yes, in which the man denied it. Well, what case?—It is not at all uncommon for patients in an asylum when charged with an act to deny it. Suppose an act committed under a delusion, can you mention any case in which the man denied having done it?—I never met with a case to the same extent as this; but patients charged with serious acts committed under delusions often deny them. Tell me a case.—I can't condescend on any at present: they are frequent in asylums. What sort of delusions?—Under suspicion that other patients are speaking ill of them. Monomaniacs?—Yes. Mention any case.—Last summer some patients were playing cricket; one of them, under the delusion that a gentleman passing was speaking ill of him, struck him with his club, and then denied having done so. Did he?—Yes, to the extent—— He said he didn't intend to hit him so hard?—Precisely. Perhaps he didn't?—Possibly. Is that the nearest case you can find?—Yes. Have you ever read of a monomaniac committing a crime and then ascribing it to accident?—I am not aware of any. Isn't it in accordance with your experience that he rather glories in the deed?—Not always. Can you give any case?—He may sometimes say he couldn't help it. But you never read of a case?—I am not prepared to say so. What do you make of the different accounts given by this prisoner?—It is remarkable, but may be explained by the change of circumstances. Would a night's quiet rest account for it?—No. Does it not show some reasoning power?—Yes. It shows he knew he did wrong in putting the man to death?—To some extent. To the whole extent?—I don't know. May it not be accounted for by this, that in thinking over the matter he thought the safest course for him was to say it was an accident?—It may. Any other way of accounting for it?—No. The prisoner in conversation has expressed doubts of Paterson being dead. Did you think he believed he was not dead?—No. You think he was trying to impose upon you in that?—Yes. And also when he gave you a circumstantial account of how the thing occurred by accident?—I did. He was sincere in his manner?—He was. He couldn't be in his matter of deceiving you?—No. If calm and sincere in his manner then, could you believe him in his other statements?—Yes. Was his manner more calm or more sincere?—He repeated his statements over and over again. Had you more reason to doubt his account of the occurrence than his account of his delusions?—Yes; from the way he described it I thought it improbable. More improbable than the blowing of the vapour?—Oh yes! You thought it not true from its improbability?—I thought so. To what do you attribute the state in which you found the prisoner?—His health was bad; he was pale and sallow. Was it drink?—Yes; his course of life. *Re-examined.*—I have made insanity my principal study. It is possible that if I had had time I might have remembered cases such as the present. I do not say that there are no such cases. Does the supposition that the prisoner intentionally told a lie in his declaration exclude the theory that he was insane the day before when the deed was done?—No, it does not. Do you consider it possible that if the prisoner was insane at the time of the deed there might afterwards be something like a gleam of sanity in which he might try to save himself by a falsehood?—Yes. That would not be inconsistent with insanity?—No. Might he afterwards have again fallen into insanity?—His mind might have been so far rectified as to admit the murder, and then have relapsed. I don't ask you about a sudden cure, but is it possible?—It is. Does the declaration shake your opinion that at the time when the deed was done the prisoner was insane? —No. Is it a possible development of the form of monomania that the man might be quite well aware of the heinousness of the crime, and that he would suffer for it, and yet not be able to keep from committing it? *The Lord Justice Clerk.*—Mr Thomson, if all the physicians in Europe were to swear that, I would tell the jury not to believe it. *Mr Thomson.*—I find this stated in medical books, and I cannot think the question preposterous. *The Lord Justice Clerk.*—The act must be under a delusion, and if so, the man cannot have a perfectly clear mind; his judgment is obscured and impaired. The question was repeated. *Dr Smith.*—I cannot answer the question. *By the Court.*—When I saw the prisoner on the 13th, he was just the same as when I saw him on the 9th January.

*Professor Christison.*—I saw the prisoner on the 9th January last, in prison. I had a long conversation with him. What opinion did you form of his state of mind? He was perfectly intelligent on all matters except certain impressions as to events connected with the act for which he is now here. What were these impressions?—They appeared to me to be delusions, but which at the same time it would require the evidence of others to prove to my satisfaction that they were delusions.—You mean to say whether they were true or not?—Yes, and whether he had spoken of them prior to the act. Did he seem to believe them?—He did so. Assuming that they were delusions, what would be your opinion of his state of mind?—Why! that at that time he laboured under delusions, and was of unsound mind; that is, at the time when those delusions existed. But at the time you saw him?—He was not then under delusions. He spoke of them as past, as delusions which formerly existed. Did he call them delusions?—No, he spoke of them as impressions. Did he strike you as feigning?—I could not observe any indications of his feigning delusions. Did you look for such indications?—Yes. Did you observe any?—No. Was there anything peculiar about his eyes?—He had the dreamy expression of persons describing such impressions; it is a very peculiar look, well known to doctors. You have heard the evidence, what do you consider to have been the prisoner's state of mind? —All that the prisoner told about the robbers has been borne out by the evidence, but a different colour has been given to it by the noise which was made at his shop-door. The poisoning?—The little girl spoke of his delusion about poisoning, but no witness has spoken to anything about the vapour, or his suspicions of his wife, until after he was apprehended. You think the delusion about the poisoning is proved by the evidence of the little girl; if so, would you think his mind unsound?—Certainly to some extent. The prisoner gave different accounts of Paterson's death; do you consider that unusual?—Certainly; it is very unusual that a man should first ascribe the act to a delusion, and then explain it away as an accident. But it is possible that he might do so, and yet be insane?—Well! possible, considering the strange vagaries of the insane mind; but I cannot say that I remember any instance of such a thing. Assuming that these were delusions, what is your opinion of the prisoner's state of mind?—I think the prisoner's case may be accounted for thus, that he was under delusions in regard to these impressions, but very sound otherwise if they were delusions. *Cross-examined.*—We are inquiring into a particular act; you have heard the evidence; assume that the prisoner stabbed Paterson intentionally. Does anything you saw of the prisoner or anything you have heard indicate to you that, at the time of the act, the prisoner was labouring under an alienation of reason so that he thought he was doing no wrong?—Nothing. *By the Court.*—When you saw the prisoner, did you think he believed that he had been enveloped in vapour caused by Paterson?—I saw no reason to doubt that he believed it; but I desiderated evidence to show that he had spoken of that before his apprehension. Was there anything in his manner to lead you to doubt he believed it?—I saw no reason to doubt from the way he spoke that he believed what he said. And so with the other statements?—Everything that I saw. But you spoke of them as past?—I mean that he did not think that the practices were continued. He was not at that time under any existing delusion of the senses?—No. Should you say under any delusion of the understanding?—His recollection of the past was such. You would not consider a man under this kind of delusion recovered until he came to disbelieve the delusion?—I would consider him in the way of recovery. In feigning insanity, is it easier to feign a past delusion than a present one?—Yes. The prisoner gave two accounts of the act?—I was not aware of it at the time. Was it not very remarkable that he should give an account that night and a different one next morning?—It was. Can you account for it on the theory of insanity?—Only that it was one of the vagaries of the insane mind. From all you have seen there is no reason to suppose that this man was under delirium tremens at the time of the act?—Certainly not then.

This closed the case for the prisoner. The Solicitor-General addressed the jury on the part of the crown, and Mr Scott on the part of the prisoner.

*The Lord Justice Clerk* proceeded to charge the jury. In the course of his observations he said,—The question for you to consider is, whether there is evidence of the alleged delusions really being in active operation and influencing the conduct of the prisoner prior to the time of the act. I need hardly tell you that if the evidence of those delusions is to be found merely in the subsequent statements of the prisoner, that would require most careful sifting to guard against fabrication. But it is different if it depends on independent testimony prior to the act. It is a very important and singular fact that, while the prisoner gives, on the afternoon of the murder, an account in accordance with the previous delusions, after he has spent a quiet night in the police cells he gives a totally different account of it, quite irreconcilable with it. This is said by the Solicitor-General to be unprecedented, and perhaps it is so. The medical gentlemen can recall no previous case in which such a thing has occurred. But there is a great deal of force in Dr Christison's observation, that it is impossible to account for the vagaries of an insane mind. You must not, therefore, take this strange alteration as conclusively showing the sanity of the prisoner. The second story is told by the prisoner in a very connected and distinct manner, and he seems to have taken the first opportunity in the morning of telling the new story. It was evidently the result of his cogitation during the night. It appears that the prisoner has since, over and over again, told both the stories which he had told before. He first of all tells the one story, in which he speaks of these delusions, and then side by side with it he tells the other story, in which he ascribes the act to accident, and says it is impossible it could be otherwise, as the deceased was his dearest friend.

The most important question is, Was the prisoner insane at the time of the act?—The doctrine of criminal responsibility is exceedingly simple. If a person knows what he is doing,—that is, if he knows the act he is committing,—if he knows the true nature and quality of the act, and if he apprehends also its effects, he is responsible; but if he is in such a state that he does not know the act or its true nature and quality, or the effects which may follow from it, he is not responsible, provided he is in that condition through mental disease. If you are quite satisfied that the prisoner was under insane delusion at the time of the act, you need not inquire whether he knew right and wrong. If the delusion be once established, the law will presume from that that he did not know right and wrong. But you must be quite satisfied that he was under an insane delusion. It is not sufficient to say that a man is in an anomalous state, from whatever cause,—in a state which makes the bad part of his nature get the better of him,—a state, so to speak, of moral depravity. Weakness of mind combined with moral depravity does not make a man insane,—there is no greater mistake. But if the mind is diseased, the man is insane. And if the mind is diseased, it is no matter what has caused it, so be that the insanity is actually produced and present at the time.

The jury retired, and, after an absence of an hour and a quarter, returned with a verdict, finding, by a majority, the prisoner guilty as libelled, but with a recommendation to mercy. In respect of which verdict the prisoner was sentenced to death.

## MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION XLII.—MEETING IV.

*21st January* 1863.—JAMES SPENCE, Esq., *President*, in the Chair.

### I. OSTEO-CANCER OF THE FEMUR REMOVED BY AMPUTATION.

*Mr Spence* showed a specimen of osteo-carcinoma of the lower part of the femur which he had lately removed by amputation. The tumour had been of very rapid growth, only four months and a half having elapsed from the time that it first showed itself. From the external appearances the case was supposed to be one of soft medullary cancer. However, when a section was made of the bone, the medullary cavity was found filled with dense, hard, osteoid cancer, the preparation resembling in this respect two others which he (Mr Spence) had exhibited to the Society. Between the periosteum and the periphery of the bone was a good deal of soft medullary matter.

### II. GALL-STONE DISCHARGED THROUGH THE ABDOMINAL PARIETES.

*Dr James Sidey* showed a gall-stone about the size of a walnut, which had been discharged from an abscess in the right iliac region. The case was that of a woman who had been delivered of a child about a fortnight before she came under observation. When seen she was suffering from great pain in the right iliac region, where a tumour of the size of an egg was found. The tumour gradually increased in size, the skin became thin, and fluctuation was perceptible. It was accordingly opened, and purulent matter came away. About a fortnight afterwards the gall-stone appeared at the wound, and was picked out with the fingers. The only history which could be obtained was, that twelve months before she had a febrile attack, with symptoms of peritonitis, but whether or not the gall-stone came down at that time it was impossible to say. It might be added, that a smaller gall-stone came out two days after the large one.

### III. ON GUNSHOT WOUNDS, WITH REMARKS ON THEIR TREATMENT.

*Mr Alexander Neill* (Assistant-surgeon 65th Regiment) read a paper on gunshot wounds, founded on observations he had made while serving in New Zealand. The results of the cases which came under treatment were very satisfactory, and Mr Neill ascribed his success to the employment of cold dressing in the first instance, and to the very simple mode of treatment pursued throughout. (This paper will be found at page 790 of the present number of the Journal.)

*The President* expressed the obligations of the Society to Mr Neill for his interesting paper. He quite agreed with the author that now-a-days there was no special difference between military and ordinary surgery; under all circumstances the great general principles of surgery were to be carried out. A good many examples of gunshot wounds occurred in this city and neighbourhood, and he (Mr Spence) had laid before the Society an account of several which had occurred in his practice; since then he had seen various others, which illustrated several important points connected with this class of injuries. There was a point of some importance to which Mr Neill had not alluded regarding the accompanying flesh wound. As originally pointed out by Mr Guthrie, the condition of the muscles involved was peculiar; they underwent softening apparently from inflammation, and pyæmia often supervened. Mr Spence had met with a case of the kind last summer; the patient had been wounded in the calf of the leg by small shot; most of the pellets were got out, everything appeared to be going on well for some weeks, when symptoms of tension of the limb manifested themselves, pyæmia supervened, and the man died; on post-mortem examination the gastrocnemius was found in the peculiar softened condition described by Mr Guthrie. Mr Spence had met with the same thing in one or two cases of stabs. As to the treatment: healing by the first intention was hardly to be looked for, for, from the nature of the injury, the vitality of the part must have been lowered, and sloughing was apt to occur. In India some cases, chiefly among natives, had been recorded of healing by the first intention, but such cases were generally regarded as exceptional. Mr Neill had alluded to a peculiar form of bronchitis which occasionally followed gunshot wounds, the occurrence of which coincided with a dry condition of the wound, and which was recovered from when the purulent secretion was restored. Mr Spence had seen bronchitis here, or at least a peculiar condition of the lungs accompanied with râles, which corresponded with a dry condition of the wound, and was the precursor of pyæmia. Unfortunately the state was seldom recovered from, even if the purulent secretion was re-established to a small extent. Of course simple bronchitis might occur, but the condition to which Mr Spence had alluded was important as often ushering in pyæmia.

*Mr William Brown* asked whether Mr Neill would not in part ascribe the success he had met with to the genial nature of the climate of New Zealand.

*Mr Neill* said there could be no doubt that the climate of New Zealand, as well as the state of the men at the time, had contributed greatly to the favourable results obtained. The men had for some time before led most regular lives; they had had no opportunities of drinking, their diet was excellent, and altogether they were in the best possible condition.

*Dr P. H. Watson* alluded to the remark of John Hunter in reference to the state of military surgery in the end of last century,—that if there was a speciality in the requirements for that department, it was that the military knew rather less of surgery than the civil practitioner. This reproach was quite unmerited at the present day, and of this there could be no better proof than the very large measure of success which attended upon the treatment employed by Mr Neill. Dr Watson had had great satisfaction in hearing a record of the experience of one fresh from the wars, who insisted so much upon the importance of simplicity in treatment, and he was no less gratified to find that nothing new had been introduced since he (Dr W.) had been personally engaged in the treatment of these injuries. Mr Neill attached great importance to the treatment by water dressing, and from his own experience he believed that in the early stage the best application was either simple water dressing, or lint soaked in oil, so applied as not to lie so closely over the orifice of the wound as to seal it up, but so as to allow the blood and other fluids to escape. So soon as inflammation set in in earnest, warm applications were better, and were also more agreeable to the patient; indeed, regarding the choice of these varieties of dressings, it was probably best to be guided by the sensations of the patient. When suppuration was commencing cold dressing when first applied not uncommonly irritated the part, but as it became warm it became more soothing. In such circumstances either tepid water or a warm poultice was indicated as appropriate. Where it was desirable to apply cold to a wound, it would be well, if in our power, to employ Baudens' method: the wound was covered with lint soaked in water, over this a piece of ice was suspended in a canvass bag, the fragment of ice being of such a size as to take several hours to melt; this method required less attendance than any other mode by which cold could be continuously applied, and in many cases besides in gunshot wounds, where cold was a valuable therapeutic agent, this plan would be found to answer admirably. The degree of cold could be easily regulated by applying several plies of lint, or only a single one over the wound. Mr Neill had spoken of the division of gunshot wounds into *simple* and *compound*, according as fracture of bones was or was not present. Probably a better division was into *simple* and *complicated;* simple, including those where only the soft tissues and parts of little importance were involved; complicated, embracing those where vessels, nerves, bones, and important internal cavities or organs were implicated. Mr Neill had given the particulars of a case where a bullet had passed through the orbit and lodged in the brain; the case was a very satisfactory one, on account of the successful result. Such an injury with such a result was not at all common, though the records of surgery contained several examples of the kind. It was indeed wonderful, in exceptional cases, how balls lodged in the cranium and gave rise to very little irritation, less often than was usually seen in civil practice from compound fractures involving the parts contained in the skull. Dr Watson remembered the case of a young man whom he saw two months after receiving a wound of the head. The ball had evidently penetrated the skull and lodged. Nothing had been done in the way of interference with the wound: the patient had become very emaciated, had a peculiar old look, complained of frontal headache, had a slow pulse, and his urine was loaded with phosphates. On trephining the skull a wound of the membranes was exposed, and the ball was found situated at the depth of an inch in the substance of the softened brain substance. After four weeks Dr W. lost sight of the patient, but during that time he went on well, began to gain flesh, lost the phosphates in his urine, and had no longer the aged, pinched visage. An important point to be attended to in the treatment of gunshot wounds of the scalp is the fact, that a ball may impinge on the surface of the skull, make a scalp wound, and only appear to have bruised the bone, while a portion may really have lodged. Dr Watson remembered a case where, some time after a wound of the head, cerebral symptoms having set in, the wound was laid open, and the surface of the skull cleansed, when a small tuft of hair, the eighth of an inch long, was seen sticking in a minute crack in the calvarium. Trephining was performed, and lying between the bone and dura mater a piece of lead resembling a flattened pellet was found; this had evidently carried in the hair along with it. The lead was surrounded by only a small quantity of flaky pus. The patient made a good recovery. Mr Neill had spoken of bullet-forceps, and had stated that in his experience the old-fashioned bullet-forceps were as good as any. Dr Watson considered that it was rare that forceps with blades of any length were either needed or could be satisfactorily employed. In general, the forceps used for drawing the incisor teeth or ordinary dressing-forceps were to be preferred; for, unless the ball could be felt with the finger, it was rarely expedient to attempt its removal, unless by manipulation it could be felt lodging elsewhere. When the ball was lodged in a limb, and could not be felt from the wound, it could sometimes be felt through the integument at some other part. In such circumstances the ball should be cut down upon by a free incision, when it could then be seized by short-bladed forceps and easily removed. Dr W. believed that most of those who had had much experience in the treatment of gunshot wounds had discarded long-bladed forceps. They might look well enough, and were perhaps suited to catch and remove bullets from the coat sleeve, but on the living body they were very likely to do injury. Numerous forceps of various forms were sent out to the Crimea on trial, but in most cases they did harm. Dr Watson remembered a case, that of a man who had sustained a gunshot wound over the lower jaw, and in which the bullet had lodged under the fascia of the neck, over the bifurcation of the carotid. The wound had healed, when a surgeon thought it would be well to extract the ball. It was found, in commencing the operation, that the external jugular vein lay just over the part in which the incision had to be made. An attempt was made to hold the vein aside, but unfortunately it was opened into, and gave rise to troublesome bleeding. The surgeon, alarmed at this result, with the sharp end of a probe then proceeded to scratch his way through the intervening textures to the ball. At last a Coxeter's scoop was got round the ball, and the surgeon attempted to remove it through an opening which was much too small to admit of its extraction being satisfactorily effected. The tissues of the neck were drawn up, emphysema of the side of the neck was produced, and though the patient ultimately recovered, he narrowly escaped death in consequence of diffuse inflammation of the head and neck. The kind of forceps which had recently been used in America, and known as Tiamann's bullet forceps, if long bullet forceps were ever to be used, seemed better suited to satisfy the requirements of such cases than any others. They had long slender shanks, the joint of the blades being about three quarters of the whole length of the instrument from the bows. The shanks crossed each other both at the joint and also near the bows, so as to enable them to be opened and shut within the wound, and thus admit of manipulation being freely effected. The extremity of each blade terminated in pretty stout teeth set outwards, like the incisors of a mouse, so that they would seize the round surface of a bullet firmly, even when only a portion was within the grasp of the instrument. When closed the blades occupied little space, and passed smoothly through the wound, so that the instrument could be used as a probe to examine for the situation of the ball. They were of course only suited for the extraction of leaden balls, iron and other hard bodies eluding their grasp. If any other form of long bullet forceps was desired, uterine polypus forceps would be found most manageable, and therefore most useful. Dr W. also alluded to the impropriety of employing a probe in the case of recent wounds, and recommended that the finger should, in such circumstances, alone be employed to explore their track.

### IV. TWO CASES OF MURRAIN (APHTHA EPIZOOTICA) IN MAN.

*Mr Craig* (in the absence of the author, Dr G. W. Balfour) read a paper on this subject, which will be found at page 704 of the last Number of this Journal.

*Professor Gamgee* had listened with much interest to this communication. He had for a long time past turned his attention to the dangers occasioned by the extreme prevalence of contagious diseases among cattle. Great Britain, since the free importation of stock had been legalized, had sustained a loss which amounted to an almost fabulous amount. During the first decennium there was an absolute loss of twenty-three millions sterling, to say nothing of the injury sustained by the great prevalence of the disease in non-fatal cases. In 1840, the first observations on epizootic aphtha were made. It was not then known to be contagious, but was attributed to atmospheric causes, the prevalence of particular winds, bad ventilation, and the like. It might, however, at once be stated, that the disease was due to the importation of Dutch cattle. This was doubted for some time, but now there could be no uncertainty on the subject. During the first year, although the contagious character of the affection was often overlooked, it was soon noticed that the disease appeared only among those cattle which had been in communication with bought stock; or, where no intercourse of this kind could be traced, that there had been a communication by human beings passing from one herd to the other, or that there had been a means of contagion by articles of food, etc. Accordingly it was speedily established, that the propagation of the disease was entirely due to contagion. Professor Gamgee had not himself met with any cases of murrain in man, but the recorded cases were so numerous, and he had heard of so many from cowfeeders and farmers, that he entertained no doubt on the subject. Young children were especially liable to be affected, the chief symptoms being diarrhœa and vomiting. The interesting observation had recently been made, that the milk need not be fresh in order to produce disease. A case had occurred near Edinburgh within the last three months, where the milk of a cow affected with the disease (aphtha epizootica) had been made into a custard-pudding; the whole family partook of it, and all experienced violent intestinal irritation. If we saw calves, pigs, dogs, and such strong animals, killed by the milk of diseased animals, it was not surprising that infants should suffer from the same cause. He must say, that medical men had not had their attention sufficiently directed to the causes of stomatitis and of various eruptions which might be fatal or very troublesome in children. Last year, three outbreaks of the disease took place among the dairy cows of Edinburgh. It was no doubt puzzling to find such irregularities in the effect of using the milk of diseased animals as had been noticed. There was no question but that the milk of cows suffering from the aphtha epizootica was largely used without producing any injurious result. The explanation appeared to be this, that it was not the milk itself which was poisonous, but the matter of the vesicles which was apt to be mingled with it: a little precaution would be sufficient to prevent the admixture. He (Professor Gamgee) had on various occasions noticed that the milk of animals affected with the disease, but having no eruption on the teats, might be used with impunity. But so careless were dairymen, that he had seen pus, blood, and scales go into the milk. He had recently been in Dublin in company with the editor of an agricultural journal. They saw blood drop into the pail from the bloody hands of a man milking a cow affected with aphtha epizootica. Such things ought not to be allowed; but the Society knew well that interference on behalf of the public had hitherto been a thankless task. As matters now stood, we had diseases among our cattle which we had no right to have; importation had caused them; and for every animal we had imported we had lost two at least. Without speaking as a politician, there was no doubt that the farmer would be much benefited by stopping the indiscriminate importation of cattle. The result of the present system was only too apparent; in spite of the immense importation which had taken place, the price of meat had risen. The production of stock in this country had materially diminished, owing to the prevalence of aphtha epizootica and the dreaded pleuro-pneumonia. These diseases had killed upwards of sixty per cent. of the stock in all our large towns. In Edinburgh, from the 1st of July 1861 to the 1st of July 1862, 791 cows had been slaughtered for human food on account of these diseases. Perhaps the vilest part of the whole was, that the carcases of 284 diseased animals had during the same period been taken out to the pigs, which had been fed with the raw flesh. He was sorry to see the Town Council permit the sale of diseased meat to be used as food by the pigs, which were afterwards to be consumed by the public of Edinburgh. Pigs fed in this way could not but become diseased; and it was not surprising to find Mr Turner state, that the muscles in very nearly two per cent. of the subjects which came to the dissecting-rooms of the University were affected with the trichina spiralis. The whole subject was eminently worthy of the attention of all medical men who took an interest in the health of the community.

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## SESSION XLII.—MEETING V.

*4th February* 1863.—BENJAMIN BELL, Esq., *Vice-President*, in the Chair.

### I. SPECIMEN OF A HUMAN HORN.

*Dr John Struthers* exhibited a specimen of a *human horn*, which had been presented to him by his former pupil Dr P. W. Stark of the Lancaster Asylum. It was taken from a female aged about 70. It grew among the hair a little behind the forehead. Several others were growing at the same time from one to two inches apart. It was almost detached, and was easily pulled off. It was of a rounded form; $3\frac{1}{2}$ inches in length; the diameter near the base was $\frac{3}{10}$ of an inch, at the middle $\frac{3}{12}$, and towards the point $\frac{2}{10}$. It tapered gradually though not uniformly, and ended in a blunt point. It had a double curve, giving it a spiral form. When laid on the table the first two inches lay flat, forming a considerable curve, say inwards, while the distal third rose forwards from the table, curving backwards towards the root, and a little outwards. The point rose to nearly an inch from the table, and was an inch and three-fourths from the base in direct distance. The surface of the horn was irregular, rough, hard, and marked with longitudinal streaks. It was of a light-brown colour, marked with white patches. On fracture, the interior presented a more uniform light-brown colour.

### II. CALCULUS FROM THE FEMALE BLADDER.

*Mr Annandale* showed a calculus, the size of a pigeon's egg, removed by Mr Syme from the bladder of a woman fifty years of age. The symptoms of stone in the bladder had existed for twelve months.

### III. EXTENSIVE EXFOLIATION OF THE RADIUS.

*Mr Annandale* also showed an exfoliation consisting of almost the entire length of the radius, removed from a boy twelve years of age.

### IV. ON THE ORDEAL NUT AS A NEW AGENT IN OPHTHALMIC MEDICINE.

*Dr Argyll Robertson* read a paper on this subject, which will be found at page 815 of the present Number of this Journal.

*Mr Benjamin Bell* considered Dr Robertson's paper to be very valuable and suggestive. He had not seen quite the same effect produced on vision by dilatation of the pupil as Dr Robertson had described. When the pupil was dilated by belladonna, he (Mr Bell) had not found any very great degree of interference with the accommodation of the eye produced; there was no doubt a certain haziness of vision, but that was all. He had often had his own pupil dilated accidentally, but it had made no difference on his short-sightedness; it seemed merely that rather more light came into the one eye than into the other. In elderly people who were presbyopic we generally found the pupil very small, while in Dr Robertson's case myopia accompanied contraction of the pupil.

*Professor Balfour*, having been the first to describe the species (Physostigma venenosum) from which the ordeal nut was derived, was glad that the subject of its therapeutic properties had been taken up. He was sorry that Dr Christison had not yet analyzed the nut perfectly, and that Dr Fraser had not made out the alkaloid, which was no doubt its active principle. Having induced Dr Fraser to take up the inquiry as to the physiological and therapeutic effects of the plant, he (Dr Balfour) was anxious that that gentleman should have the credit of having first discovered its action upon the pupil. The subject had been publicly mentioned at the defence of the theses, and Dr Fraser had received one of the medals for his dissertation, which he regretted had not already been published. Dr Balfour was glad that the subject had fallen into such good hands as Dr Robertson's.

*Dr Matthews Duncan* having inquired if the effect on the pupil was produced when the substance was administered internally, *Dr Robertson* stated that his experiments had hitherto been entirely confined to its local application; while *Dr Fraser* stated that the effects were equally produced whether it was swallowed or introduced endermically.

### V. ON THE CONNEXION BETWEEN TUBERCULOSIS AND INSANITY.

*Dr T. S. Clouston* (of the Royal Edinburgh Asylum) read an elaborate paper on the above subject. Dr Clouston commenced by stating, that though the more frequent occurrence of phthisis among the insane than among the general population had been long observed, the subject had not yet met with that attention which its importance demanded. It was not easy by a comparison of statistics to arrive at correct conclusions on this subject, for there could be little doubt that in many cases where death was set down as due to "exhaustion," "general decay," or "marasmus," the real cause of death had been due to some form of tuberculosis. The diagnosis of disease in the insane was often difficult or impossible, and it was therefore not sufficient to trust to the *apparent* death-rate from phthisis and other tubercular diseases, but it was essential to examine carefully the records of a sufficient number of post-mortem examinations, to note the frequency of tubercle in every organ of the body, and to compare the results with the frequency of the same lesions in the sane: and even then the history of each case must be examined into before it could be determined what relation the insanity and tuberculosis bore to one another, and in particular which lesion had been first developed.

Dr Clouston had carefully gone over the pathological register of the Royal Edinburgh Asylum since 1851, and had examined the records of the 463 post-mortem examinations which had been made, and by an examination of the case-books had obtained a tolerably complete history of each of them. From these records it appeared, that in the 463 cases, tubercular deposit had been found in greater or less quantity in 282, or 60·9 per cent.; of the total number of cases, 263 were males, and tubercular deposit was found in 136 or 51·7 per cent.; in the 200 females it was found 146 times, or in 73 per cent. This frequency of the deposit seemed startling, and appeared to indicate an undoubted connexion between tuberculosis and insanity. Dr T. K. Chambers had given the results of 2161 post-mortem examinations at St George's Hospital, and had found tubercular deposit in 550, or about one-fourth of them. In comparing the statistics of the sane and insane, whether phthisis were taken as the assigned cause of death, or the relative frequency of tubercular deposit was noted, tuberculosis was found to be much more common among the insane than among the sane. Three persons were recorded as having died of phthisis in the Royal Edinburgh Asylum for every two who died in the eight principal towns of Scotland above twenty years of age. For every five bodies in which tubercular deposit was found in St George's Hospital, twelve were found in the dissections made in the Royal Edinburgh Asylum.

Assuming, therefore, that it was proved that tuberculosis was more common among the insane than among the general population, the next question which arose was, why was this the case? Was insanity a predisposing cause of tuberculosis? Did the mode of life in asylums determine this frequency? or, Did the tuberculosis predispose to insanity? These questions were very complicated, and could only be answered by a careful examination of the clinical history of a sufficient number of cases: they were not mere curiosities of vital statistics, but involved important considerations regarding the conditions of life among the insane, and the causes of insanity itself.

Dr Clouston divided this inquiry into two heads, treating, *first*, of the influence of the tuberculosis on the insanity, and, *second*, of the effect of the insanity on the tuberculosis. By carefully drawn up tables, applying to the 463 patients whose bodies had been examined, and comparing the *forms* of insanity in the tubercular and non-tubercular cases at the commencement of the insanity and at death, it was shown that among the tubercular, every form of insanity tended more towards dementia before death than among the non-tubercular; that this tendency was twice as strong in mania as in melancholia, and that the majority of monomaniacs died unchanged; while among the non-tubercular the results were different. Half of the tubercular cases were demented, only one-fourth of the non-tubercular were so; one-tenth of the former died maniacal, one-fifth of the latter; one-tenth of the former were melancholics, only one-twelfth of the latter; one-seventh of the former were monomaniacs, only one-ninth of the latter; one-eighth of the tubercular only were general paralytics, while one-third of the non-tubercular laboured under this disease. In these results all forms of monomania were classed together; but if one form, monomania of suspicion, were taken separately, a striking result was arrived at. *All* the females with this form of insanity, with a single exception, were found after death to have tubercular deposit; while only six of the males were exempt. General paralysis was the form of insanity most exempt from tuberculosis, and the relations between these diseases therefore deserved special consideration.

Since 1851 the post-mortem appearances of 92 general paralytics had been recorded in the Edinburgh Asylum: of these, 84 were men, and 8 women. In 27 of the men, and in 6 of the women, tubercular deposit was found in the lungs. On examining the histories of these 33, it was noticed that in nearly all, the mental affection had commenced with symptoms of depression. No history could be obtained in the case of 7 men and 1 woman, who had been admitted at an advanced period of the disease, but of the others there were only 2 who had not at first exhibited symptoms of melancholia, and in these 2 the tubercle was nearly obsolete. Many of them were suicidal, many obstinately refused food, while in others the symptoms of depression were not so great. Many of these patients at a subsequent stage of the disease exhibited the excitement and ambitious delusions which generally characterize general paralysis, but even then in a slighter degree than is usually observed. There were few cases of general paralysis where excitement was entirely absent, but these cases would be chiefly found among the phthisical. At present there were in the asylum only 3 cases of general paralysis where the disease had commenced with well-marked symptoms of depression, and, in all of these, physical examination demonstrated the presence of tubercle. Dr Clouston was not aware that this intimate relationship between tuberculosis and general paralysis with depression had ever before been pointed out. The number of cases was no doubt insufficient to establish a general law, but it was enough to point to a decided connexion between the conditions. The tuberculosis could not be ascribed to the refusal of food,—for most of the patients, however depressed, took food well, and seemed in good bodily health. In most the phthisis had caused little inconvenience, and had been rarely detected till after death. The latency of the pulmonary affection was more marked in this than in any other form of insanity, not only as regarded the absence of the usual symptoms, such as cough and expectoration, but from the infrequent occurrence of marked emaciation or feverishness. In no other form of insanity was the existence so purely vegetative, and so independent of nervous and animal influences; so that a man with extensive disease of the lungs might present no appearance of suffering from any local disease. It might be said that this resulted from the influence exerted by general paralysis in checking pulmonary disease; even if there was an influence of this kind, it was not sufficient to account for the anomalous result. In many of the cases the disease of the lungs discovered after death was so extensive, that it could not have existed before the commencement of the cerebral affection without having produced manifest symptoms, and in many it was evident, from the appearances found at the autopsies, that the tubercle had been in an advancing, not in a stationary condition. There were exceptions to what had been said regarding the latency of the symptoms, for in 5 of the cases the existence of tubercle was manifested during life by the ordinary signs, such as cough, hectic, and exhaustion. Tuberculosis appeared to do little to shorten the life of the general paralytics amongst whom it occurred. Among those in whom a large amount of tubercular deposition had been found, the average duration of life had been two years, which differs little from the duration of the disease in ordinary cases. It appeared that, in general paralysis, the tendency to death from the cerebral affection was so strong, that the pulmonary affection did little to accelerate it. In fact, the intracranial lesion, by paralyzing the centres from which the pneumogastric nerve arose, deprived the pulmonary lesion of much of its exhausting effects, neither cough, wakefulness, nor want of appetite being produced. Various authors had pointed out the fact that many cases of general paralysis were characterized by symptoms of depression, but no explanation had hitherto been given of these exceptional cases. Although nearly all the tubercular general paralytics had been melancholic, Dr Clouston did not conclude that all melancholic general paralytics were tubercular. The general paralytics, and those in whom there was *much* tubercular deposit, together amounted to 67 per cent. of all the cases examined. The large mortality in asylums was thus at once accounted for.

The influence of tuberculosis in insanity could not be determined by merely ascertaining its frequency in the various forms of the disease. The ordinary classification of cases of insanity was not so complete as to prevent us from having well-marked types of brain-disease embracing many cases of each form. We might have maniacal, melancholic, and demented general paralytics, but general paralysis was a more distinctive form of insanity than either mania or melancholia. Dr Clouston had noticed cases, where, from the mental symptoms alone, he had been able to predict that the subjects of them were likely to die of phthisis. The insanity might be of different forms: the cases had no one symptom in common,—they had no diathetic mark or physical sign,—yet they took their place in one's mind as a natural group. If acute at first (whether maniacal or melancholic), this stage soon passed off, and they lapsed neither into a chronic condition nor into a state of deep dementia, but into a peculiar irritable, excitable, sullen, and suspicious state. There was a want of fixity of the mental condition, and a want of interest in all occupation and amusement; the intellect was not at first so much obscured, as that there was a great disinclination to exert it; occasional short unaccountable attacks of excitement or unprovoked paroxysms of irritability showed themselves. The condition might be called a mixture of subacute mania and dementia, the symptoms of one or other condition predominating in different cases. As the disease advanced, the symptoms of dementia came to predominate, but the mental faculties were rarely completely obscured, and there were rarely the distinct remissions met with in ordinary periodic insanity. If any single condition characterized this group of cases, it was a tendency to be suspicious. This state Dr Clouston would denominate "phthisical mania." The patients were not so apt to get stout as the simply demented, the appetite was often capricious, the pulse was generally weak and rapid, and there was a general want of tone of the system. In many cases the insanity commenced insidiously, and showed itself by an alteration of the conduct and affections, by irritability and waywardness, and a progressive weakening of the intellect, without any great excitement or depression. However demented they became, fitful flashes of intelligence showed themselves; and in them more frequently, perhaps, than in any others, increased intelligence, and a sort of unveiling of the mental faculties, occurred before death. Under the term "phthisical mania" Dr Clouston would include only those cases who died with tuberculosis within five or six years after becoming insane, and in whom the development of the diseases had appeared to be somewhat contemporaneous; the old chronic cases of insanity, when tubercle became developed after many years, or the acute cases of melancholia, with refusal of food, he would exclude, because in these there might be causes for the development of tuberculosis apart from the insanity. Dr Clouston found 33 cases of this "phthisical mania" among the men, and 42 among the women, among the whole number of those in whom *much* tubercular deposition had been found. In all of these 75 cases, symptoms of phthisis occurred within five years of the commencement of the insanity, and in the majority within two years. In these cases Dr Clouston regarded the insanity as the result of a strong tubercular diathesis, a tendency which was then being developed, or about to be developed, into tuberculosis. Supposing that this explanation accounted for the cases of phthisical mania, it might be asked what connexion there was between the insanity and tuberculosis in the other cases, which constituted nearly one-half of the whole in which phthisis had been the assigned cause of death. In 31 of the men, and in 38 of the women, who presented much tubercular deposit, the insanity had existed for seven years or upwards before the symptoms of tubercle appeared, and in them the relation must either have been accidental, or the result of diminished innervation injuriously affecting the nutritive processes, and secondarily inducing phthisis. The diet of the patients in the asylum was quite as good as they had been accustomed to, and their other hygienic conditions were favourable.

The existence of tuberculosis influenced most unfavourably the *prognosis* of insanity. Few patients who had manifested phthisical symptoms, or in whom tubercular deposit had been detected after the commencement of the insanity, ever recovered their soundness of mind. A few cases recovered where the insanity had come on after the phthisis had become chronic, but scarcely any case of phthisical mania ever recovered. This almost universal incurability was a strong argument in favour of the view that in those cases the insanity was the result of the imperfect nutrition of the nervous system in the pretubercular, or in the commencement of the tubercular stage of phthisis.

But it would be said, why were not all phthisical patients insane if the tubercular diathesis were the cause of the insanity in these cases? Some brains had a much stronger tendency to derangement of their functions than others, and it was in them that the impaired nutrition of tuberculosis acted as an exciting cause of insanity. The greater frequency of hereditary predisposition to insanity among the tubercular than among the non-tubercular, showed that tuberculosis more than any other cause developed such a predisposition into an actual disease. And in how many ordinary phthisical patients did we find an irritability, lassitude, fancifulness, and fickleness of purpose, that bordered on an unhealthy state of mind. It had been Dr Clouston's experience that phthisical patients could seldom apply themselves to any continuous mental exertion. Their intellects might be clear and unclouded to a preternatural degree, but their efforts resembled more the brilliant flashings of an ill-supplied lamp than the continuous steady light of a healthy mind. If any one were asked who had watched closely two or three phthisical relatives during their illness, he would tell of the absurd fancies, amounting almost to delusions, of the sudden and causeless changes from hope to despondency, from cheerfulness to irritability, of the whims and wanderings of mind, and transitory moments of delirium, that accompanied the disease. All these symptoms had a cause in an ill-nourished brain; and when they were more developed, they became insanity. Four cases of temporary delirium in patients with phthisis, mentioned as having got quite well in a day or two after coming into the asylum, were the connecting links between phthisical irritability and phthisical mania.

Dr Clouston next considered the effect of the insanity on the tuberculosis. On examining the histories of the 282 insane tubercular patients it was found that exactly one-half of the cases had died within the first three years after becoming insane, and about one-fourth within the first year. Now, it was extremely improbable that a predisposition to tuberculosis and an actual deposit of tubercle should have been developed within three years, and even allowing that in some cases the existence of insanity and the occurrence of tuberculosis were mere coincidences, we could not help concluding that there was direct connexion between the two conditions. Many continental physiologists and pathologists had attached much importance to the effect of morbid influences of the pneumogastric in occasioning disease of the lungs. Without denying that section or injuries of the pneumogastric might interfere with the healthy condition of the lungs, there was no evidence to show that the pneumogastric, its ganglia, on the part of the medulla from which its roots arose, was more frequently diseased in those who died of phthisis than in other cases of insanity. In general paralysis, in which the pneumogastric roots were so much involved, that the power of swallowing was ultimately interfered with or destroyed, phthisis was less common than in any other form of insanity. In epilepsy, the seat of which was probably in close proximity to the origin of the pneumogastric, phthisis, according to the statistics of the Edinburgh Asylum, was not so frequent as in ordinary cases of insanity. The tendency to tuberculosis diminishing rapidly in proportion to the length of the insanity, although partly explained by the rare occurrence of phthisis as life advanced, was pretty clear proof that, on the whole, insanity did not predispose to the development of phthisis.

It had often been remarked by asylum physicians that phthisis frequently ran its course in the insane without giving rise to any symptom but emaciation, and this often so trifling as not to attract special attention. Dr Clouston had carefully examined the histories of the patients in whom much tubercle had been found after death with a view to determining the time at which phthisical symptoms appeared, and the number in whom they did not appear at all. In one-third of the men, and in a little more than one-fourth of the women, tuberculosis manifested itself by no symptoms during life. The greater number of the men in whom it was latent was accounted for by the greater number of general paralytics among them. In about one-fourth of all the cases symptoms of phthisis appeared within three months before death, in one-fifth between three and six months, and in only about one-tenth, more than a year before death. By "symptoms of phthisis" was meant cough, dyspnœa, expectoration, and hectic; for in many cases put down as latent, the presence of tubercle was detected by physical examination. It appeared that long-continued phthisis, though exceptional, was not altogether unknown among the insane. It was remarkable how inconsiderable was the effect of even advanced pulmonary disease on some of the insane; patients were constantly seen going about as usual, when suddenly the appetite failed, they looked haggard and weak, and in a few days died exhausted; after death the lungs were found totally disorganized; generally, however, the patients showed signs of failing health for two or three months, rarely for much longer, before the characteristic phthisical symptoms appeared. Every asylum physician should examine the chest of any patient who showed any signs of failing health and strength, especially if accompanied with listlessness and languor. In some cases, no doubt, the disease, when once developed, ran its course just as in the sane. The irritability of the ganglia regulating the peristaltic movements of the intestines was also lessened, for although ulceration of the intestines existed in the majority of the phthisical insane, diarrhœa is not so common, and when it existed was not so troublesome as in ordinary phthisical patients. On the whole, however, diarrhœa was more common among the phthisical insane in the latter stages of the disease than either cough or expectoration.

Most writers on insanity had alluded to a species of metastasis occasionally occurring between insanity and phthisis; where the one disease appeared the other abated or disappeared. This, however, was probably more apparent than real. In most cases where the occurrence of insanity corresponded with a diminution in the severity of the phthisical symptoms, the phthisis was no doubt only masked, and there was no proof that the pathological changes going on in the lungs were in the least retarded. Dr Clouston did not believe that insanity was ever relieved or cured by the commencement of phthisis; but that in all cases where such appeared to be the case the insanity was one of the signs of the pretubercular stage of phthisis. No doubt, acute excitement or deep melancholia often disappeared on the supervention of phthisical symptoms, but disappeared only to be replaced by dementia and permanent weakening of the mental powers.

The general results to which Dr Clouston's investigations had led him were the following:—

"1. Phthisis pulmonalis is much more frequent as an *assigned cause of death* among the insane than among the general population.

"2. Tubercular deposition is about twice as frequent in the bodies of those dying insane as in the sane.

"3. Phthisis pulmonalis is the 'assigned cause of death' in only about one-half of those in whom tubercular deposition is found after death.

"4. The brain in the cases of tuberculosis is not so frequently diseased in a marked manner as it is in those dying of other diseases among the insane. In the majority of the cases the brain is pale, anæmic, irregularly vascular, with a tendency to softening of the white substance of the fornix and its neighbourhood, and the grey matter of lower specific gravity than in any other cases of insanity.

"5. Tubercle is not more frequently found in the nervous centres among the insane than among the sane; and when found, it does not in all cases, or even in the majority of them, produce any symptoms, and is not connected with any particular form of insanity.

"6. Tubercle of the peritoneum is not more frequent among the tubercular insane than among the same class in the sane. In the former it is more frequently associated with melancholia and monomania of suspicion than ordinary tuberculosis of the lungs.

"7. The average age at death of the cases of tuberculosis is about three years below the average age at death among the insane generally, and the average age of those in whom *much* tubercular deposit is found is five years below the general average.

"8. The proportion of the tubercular who had had previous attacks of insanity is about the same as among the insane generally.

"9. There is hereditary predisposition in seven per cent. more of the cases of tuberculosis than of the insane generally.

"10. Monomania of suspicion is the form of insanity in which tuberculosis is most frequent, and general paralysis stands at the other end of the scale which marks the frequency of tuberculosis in the different forms of insanity; mania stands next to general paralysis, and melancholia to monomania of suspicion; while the tendency to dementia in all forms of insanity is greater among the tubercular than among the non-tubercular. A majority of the cases of general paralysis and mania die non-tubercular; a majority of the cases of melancholia, monomania, and dementia exhibit proofs of tuberculosis after death.

"11. In all the cases of general paralysis who were tubercular the disease had commenced with depression.

"12. In a certain number of cases (about one-fourth of all those in whom tubercle was found) the insanity is of such a peculiar and fixed type that it may be called 'phthisical mania.' In all these cases the phthisis is developed so soon after the insanity, that tubercles must have been already deposited in the lungs, or a strong tubercular tendency been present, and about to pass into actual tuberculosis, when the insanity appeared. We know that the chief characteristic of tuberculosis is an impaired energy in the nutritive processes. And as a badly nourished bone becomes carious or necrosed from slight causes, or a badly nourished skin becomes subject to parasites, so disordered action results in imperfectly nourished brain cells from causes which would not be felt by a healthy brain. It is not the enfeebled nutrition directly, so much as the perverted action to which the enfeebled nutrition predisposes, that produces the insanity. The peculiar mental state, the incurability of the insanity, the appearance of the brain after death, and its lowered specific gravity, all point to such a cause for the derangement.

"13. There is a special connexion between deep melancholia, with long-continued suicidal tendencies and refusal of food, and lung disease—either gangrene or tubercular disorganization.

"14. There are a few cases in which the insanity is only a kind of delirium occurring in previously-developed chronic phthisis, and soon passing off.

"15. The prognosis is most unfavourable if tuberculosis occurs in any case of insanity.

"16. Half of the cases ot tuberculosis die within three years after the commencement of the insanity.

"17. There is no proof that the 'morbid influence of the pneumogastric nerve' has anything to do with the tuberculosis in cases of insanity.

"18. Long-continued insanity does not tend to the development of tuberculosis more than to the production of other diseases.

"19. Phthisis is entirely latent in between one-third and one-fourth of all the cases among the insane, and in almost all the others it is latent for a considerable time. This latency is most frequent in general paralysis, in which the majority of the cases of phthisis exhibit no symptom whatever.

"20. There are very few cases where the commencement of insanity benefits the phthisis; but in a few, where the phthisis is very chronic, an attack of insanity may be followed by the permanent disappearance of the phthisical symptoms, or attacks of mania may alternate with the symptoms of phthisis. In by far the majority of such cases, however, the phthisical symptoms are merely masked, while the deposition of tubercle goes on."

*Dr Skae* at this late hour would not detain the Society by any observations. He had listened with great interest to Dr Clouston's paper, which he would characterize as eminently suggestive, and as an important contribution to our pathological knowledge. The conclusions at which Dr Clouston had arrived were worthy of the most careful consideration.

## PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

### SESSION XXII.—MEETING II.

*December* 10, 1862.—Dr PATTISON, *President*, in the Chair.

### I. INTRA-UTERINE FŒTAL HÆMORRHAGE, ACCOMPANIED BY DISEASED PLACENTA.

The Secretary read the following notes of a case, by *Dr Parker*, of Nova Scotia:—

"Mrs ——, now about 35 years of age, was, before marriage, a strong and robust person, and since then has enjoyed—except when pregnant and recovering from the puerperal state—very good health.

"Her immediate relatives are healthy, with the exception of a sister, who many years ago, when 7 years of age, retired to rest apparently well, and awoke in the morning paraplegic, and has continued so ever since. Her husband is also a member of a healthy family: he is an active muscular man, and has yet to learn what sickness is. I am informed that none of the relatives on either side have ever been the subjects of the hæmorrhagic diathesis, although Mrs —— says that she on one occasion had a tooth removed after a painful and protracted operation, and that the cavity bled for nearly twenty-four hours, and the dentist had great difficulty in plugging it, so as effectually to control the hæmorrhage; but she adds, that since then she has had three or four extracted without anything unusual occurring. She has been pregnant seven times, and I have attended her in six confinements. With all her children, after the third month, she has suffered from a pain and soreness in and above the right iliac fossa, which has invariably increased as pregnancy has advanced, and has required treatment on several occasions. The movements, in utero, of all her children have been characterized by debility. With the two first they were observed to grow gradually weaker after the fifth month, until five or six weeks before delivery, when the fœtus, to use her own words, 'began to sway to and fro,' and the accustomed motion ceased. In April 1851, when in her eighth month, an alarm of fire occurred in the neighbourhood of her residence, to which she attributed the death of her first child. Not long after this the fœtal movements ceased, and in five or six weeks she was delivered of a still-born son. It was evident from its appearance that it had been dead for some time. With her second child, in July 1852, she was equally unfortunate, having, when eight months pregnant, lost another son, which, like the first, had died in utero some weeks before delivery. She could not attribute this last misfortune to any alarm, physical exertion, or accident. Indeed, warned by the first, she had been especially careful, throughout her second pregnancy, to avoid anything that might tend to injure the child, or to produce a miscarriage.

"Since October 1853, when her first daughter was born, she has been delivered of five living children, four daughters and a son; of these, two very delicate looking girls still survive. The son and one girl, when between eighteen months and two years of age, died in convulsions from head affection. With her fourth living child, a daughter, the face presented, and I delivered her by forceps. The placenta was so firmly adherent to the uterine walls as to be separated with great difficulty. Her life was in jeopardy from excessive and long-continued flooding. A milk leg still further complicated the case, and it was full three months before she could cross the room unassisted. This limb, the same in which varicose veins had existed from the time of her first pregnancy, is still weak and occasionally troublesome.

"I have dwelt thus long, and with particularity, on the mother's history, as it has relation to and is probably to a great extent explanatory of the phenomena observed and about to be detailed in connexion with the case of her seventh and last child, the subject of this communication. When between two and three months pregnant with this child, after rather a long walk, she was suddenly attacked with flooding, which continued more or less for the best part of twenty-four hours. *Before* quickening she had two similar attacks, but none *after*. She was delivered of a daughter at four P.M., 14th December 1860. The presentation was natural, and there was nothing unusual observed about the labour until the first stage was nearly completed, when there was a sudden gush of fluid, as if the liquor amnii were escaping. On making an examination I found that such was not the case, and then became aware of the fact that the fluid was fresh red blood escaping from between the membranes and uterine walls. I immediately ruptured the membranes, and to my surprise, before I had withdrawn my hand, three or four small-sized, moderately firm, and very dark coagula escaped from the interior of the membranes, and by the pressure of the waters were forced into the bed. All the liquor amnii was of a very dark red colour.

"The child, a well-developed girl, came quickly into the world, apparently lifeless, and perfectly livid from head to foot. The placenta was promptly thrown into the vagina and removed. There was slight uterine hæmorrhage, which was soon controlled. The umbilical cord was long, of medium size, and without inequalities of surface. The investing membranes were thin and very transparent. The umbilical arteries were flattened, and ran their spiral course with as much regularity as if the hand of man had wound them throughout the entire length of the cord. In fact the funis looked as if a narrow blue ribbon had been first wound firmly and spirally around a uniform central body, and then the whole had been closely enveloped in nearly transparent glass. The placenta was perhaps a little larger than usual: it was very much indurated and friable throughout. Its different component lobes were smoother and more rounded on the uterine surface than natural. Chronic inflammatory action had effected in its texture the change to which the term 'scirrhus' is often improperly applied.

"I did not recognise any change in structure or appearance in the membranes lining and covering its internal surface.

"The child was resuscitated with difficulty; and when respiration was established, she was occasionally almost suffocated by frothy blood and mucus, which issued from the mouth and nares. This sanguineous oozing almost ceased an hour after its birth, but soon returned with the same suffocating symptoms just noted. The slow hæmorrhage and difficult respiration, together with convulsions, which came on the first night, continued at intervals until its death, sixty-nine hours after delivery. The surface of the body, which at first was persistently livid, ere its death became blanched and exsanguine. The slow oozing came from the mouth, pharynx, nares, and apparently also from the larynx. There was no hæmorrhage from the rectum, bladder, vagina, or navel. The urine, meconium, and evacuations from the bowels were natural. There were no purpuric spots or extravasated blood to be seen anywhere about the body. No nourishment could be given by the mouth. Sponging the cutaneous surface with wine, and giving nutritious and stimulating injections, materially aided in keeping it longer in life. This, with astringent washes for the mouth, constituted the treatment. I regret to add that no examination of the body was made after death.

"On looking analytically at the details of this case, it will be found to contain important material for further thought and observation:—

"1*st*, It has relation to and bears on the physiological question of fœtal nutrition.

"2*d*, It well illustrates several practical points, long since suggested and discussed, connected with placental disease.

"3*d*, It raises the question—not heretofore mooted that I am aware of,—Is fœtal hæmorrhage a result of diseased placenta?

"4*th*, The circumstances detailed (maternal and fœtal) suggest another question, What effect has diseased placenta on the extra-uterine life and health of individuals?

"5*th*, This case has also relations to medico-legal science not unworthy of consideration.

"I have searched in vain for a case or cases analogous to the one just recorded.

"The nearest approach to it that I can find is that recorded by my friend Dr Moir, in the Edinburgh Monthly Journal of Medical Science for April 1849. As the conclusions at which Dr M. arrived in connexion with the case he records, both as regards the source of the blood, and on the subject of the liquor amnii as a source of nourishment to the fœtus, are opposed to the teachings of the case in question, I hope I may be excused for quoting him at some length.

"'The liquor amnii,' he says, 'came away during labour as a dark bloody-coloured fluid, which did not coagulate. . . . . A full sized healthy female child was born, and *the placenta appeared to be perfectly healthy*.' Dr Moir observed soon after its birth that the meconium which it then passed 'was of a very dark red colour, and that on being wiped off the sheets it left a reddish stain.' Several cloths containing meconium of the same appearance which passed during the day and evening, after the administration of a teaspoonful of castor oil, were observed by Dr M. 'Next day the stools gradually assumed a healthy appearance, and have continued so; the child is healthy and well.' Dr Moir asks, 'Whence the bloody liquor amnii? Was it from the intestines of the child? or from the uterus or its membranes? Most probably it was from the latter, because, had it been from the child, we should have had some appearance of blood in the stools after the whole of the meconium was discharged, but there was none.' He adds, 'If correct in the former supposition, the fœtus in utero must have swallowed the bloody liquor amnii, and this must have undergone the process of digestion. Are we therefore to infer that the liquor amnii is always, or occasionally, one of the sources from which the fœtus derives nourishment? The general opinion hitherto has been that it does not, but the present case seems to favour the conclusion that it does.'

"The case under consideration would appear to *suggest* that, in Dr Moir's case, the blood was not from the membranes, but that it was fœtal, and from the mucous lining of the rectum or bowels; while I think it proves conclusively that the liquor amnii is not swallowed in utero, and hence cannot be a source of nourishment and growth to the fœtus. Were such the case this child must assuredly, with the liquor amnii, have swallowed some of the blood which oozed from the mouth while in utero, and the meconium and discharges from the bowels must have been coloured with it; but such was not the case. It would rather prove that the complex act of deglutition could not be performed by the fœtus until released from the uterus, and subjected to entirely new and stimulating influences. In comparing and remarking on these two cases, it is to be remembered that in Dr Moir's the placenta was healthy, in mine diseased. In the former the blood did not coagulate, in the latter it did.

"A notable feature of this case is, that the mother was the subject of repeated placental disease, and it is corroborative of a fact long since recorded, that where a woman has once suffered from diseased placenta, in succeeding pregnancies she is likely to be similarly affected,—the first attack acting, as has been well observed, as an exciting or predisposing cause to the same in after life.

"Having made no notes until the birth of the sixth child, I cannot speak with absolute certainty in reference to the five first placentæ, but the impression is strongly on my mind that the two first were much diseased; and the existence of the abdominal pain and other characteristic symptoms in *all* her pregnancies would lead to the inference that not the two first and two last only but that all the placentæ had undergone morbid changes.

"Another symptom of diseased placenta, not unfrequently observed by practical men, and exciting fears lest 'placenta prævia' should exist, was present with the mother of this infant in her last pregnancy. I have stated that there were several discharges of blood from the vagina between the second month and quickening. There is no doubt but that the placenta, at this early period, was diseased, and that slight separations from the uterus gave origin to these brief floodings, as also to the discharge of blood mentioned as occurring during labour, before the membranes were ruptured.

"Indeed, from the indurated and rounded appearance of the uterine surface of the placenta, and from its early detachment and expulsion from the uterus, I should judge that it had for some time retained its maternal connexion by a very slight tenure; and also, that in this pregnancy the inflammatory action (which was found at the time of the last labour to have involved the uterus, and caused firm adhesions between the placenta and its walls) was confined to the placental structure.

"The source of the blood being evident and visible, the question arises, What was its cause?

"No such result of diseased placenta has, that I am aware of, been pointed out, but I think it not unreasonable to infer that they were here intimately related.

"If with diseased placenta a feeble living, or a still-born child comes forth, we point to the one as the origin of the other,—may we not here do the same?

"But, says an objector, Was it not purpura? or was it not due to the hæmorrhagic diathesis? I reply there were no external visible signs of the former, and the absence of bleeding from any other part than the mouth would lead to the conclusion that it was not an ordinary case of either, while to neither could it lay claim by hereditary right. But even were it admitted under one or other of these heads, might we not with equal propriety refer to the diseased placenta and bleeding mucous surfaces, as cause and effect?

"In this connexion I will briefly allude to the question which is suggested by the history of this patient and her progeny,—What effect has placental disease on children who arrive at fœtal maturity and live?

"To this question, personal observation does not enable me to reply; and, as far as my knowledge extends, nothing has been written on the subject. Individuals will necessarily only be able to furnish limited facts, but the attention of the many being directed to the matter, important and striking statistical results may flow from combined and recorded observations. Taking this case—an isolated one it is true—as an illustration, it would appear that the future health and vigour of such children are impaired. Out of five children born alive,—two died ere they had entered their second year from head affection with convulsions,—two survive, to appearance, thin and delicate; while the last ended its brief existence as above mentioned. Should future observation on this point prove, what I strongly suspect is in reality the case, that the extra-uterine standard of health of such children is lowered, it will be an additional incentive to medical men to follow up the invaluable suggestion of Professor Simpson in relation to diseased placentæ, and strive to combat that which, from its consequences, may not only affect individuals, but in the course of time, by multiplication and perhaps hereditary influences, communities.

"The medico-legal bearings of such a case are important, and, on this ground, if for no other reason, publicity should be given to similar cases. One can readily imagine a child born alive and shortly bleeding to death or suffocating (as this child would most certainly have done, had it not been most assiduously attended by a faithful nurse) from blood oozing from the parts named not being promptly removed; and if illegitimacy and other suspicious circumstances should surround the assumed case, it is not improbable that the life or liberty of the individual might be involved in the issue.

"Before closing these remarks, I would again refer to the peculiar appearance presented by the umbilical cord, which, while it may have been often observed by others, was to me entirely new. Was this also a result of the diseased placental mass?"

*Dr Pattison* thought that Dr Parker's case was very important in a medico-legal point of view, as, in otherwise suspicious circumstances, the death of the child might have been attributed to criminal interference on the part of the mother.

*Dr Keiller* remarked that it was a very curious and rare occurrence to have hæmorrhagic oozing from the mucous surface of the fœtus. It was difficult to understand how the clots were the products of the fœtal blood, inasmuch as the latter fluid, in escaping into the amniotic fluid, would be diffused and prevented from coagulating. It was much more probable that the clots and the sanguineous coloration of the liquor amnii were due to blood which had come from the placenta. It was to be regretted that no post-mortem examination had been made, as by this means a good deal of the obscurity in which the case was involved might have been cleared up.

*Dr Cochrane* said that he had had a case where the umbilical cord, which was tightly twisted round the neck, was found torn across close to the abdomen when the child was born. The amnion was natural in colour, so that there could be no doubt that the cord had been torn across during the expulsion of the body of the child.

### II. CAULIFLOWER EXCRESCENCE OF THE CERVIX UTERI REMOVED BY MEANS OF THE GALVANO-CAUSTIC WIRE.

*Dr Alexander R. Simpson* showed a preparation of a cauliflower excrescence which he had removed some months ago by means of the galvano-caustic wire. The patient was an elderly woman from the country, who had become very much reduced in consequence of the constant drain of blood from the affected part; for the fungous mass was very soft and vascular, and poured out blood at every examination. The constant fœtid discharge, moreover, was extremely disagreeable; and though there could perhaps be little hope of a complete eradication of the disease, the ablation of the morbid mass seemed clearly indicated. To have attempted to remove the cervix by means of the knife or scissors, where the tissues were too friable to be seized hold of with the volseila, would have been worse than useless; and in a case precisely similar he had seen an attempt to remove the cauliflower excrescence by means of the écraseur fail in consequence of the excessive hæmorrhage that attended the effort to introduce the chain into the vagina. He decided, therefore, on using in this instance the galvano-caustic apparatus; and having succeeded in passing a loop of platinum wire round the tumour, the current was set on, and in a few seconds the morbid mass was separated from the body. Blood necessarily escaped during the application of the wire, but not a drop was lost after the neck began to be constricted; and when a duck-bill speculum was introduced into the vagina as the patient lay on her back still asleep with chloroform, the white surface of the stump could be seen presenting a most satisfactory appearance. Only, within the canal of the cervix two suspiciously granular fungoid spots could be detected, and these he (Dr A. R. S.) destroyed as effectually as possible by means of the heated loop of platinum wire. The patient presented no unfavourable symptom, was soon able to be out of bed, and had been sent back to her home in the country restored for the time to some degree of health and comfort; but regarding her ultimate history he had no information.

## MEDICAL APPOINTMENT.

**J. Jardine Murray**, F.R.C.S.E., to be Surgeon to the Brighton and Sussex Infirmary for Diseases of the Eye.

## PUBLICATIONS RECEIVED.

Bryant,—Clinical Surgery. By T. Bryant, F.R.C.S., etc. Part IV. London, 1862.

Callender,—Anatomy of the Parts concerned in Femoral Rupture. By George W. Callender. London, 1863.

Dunn,—Medical Psychology. By Robert Dunn, F.R.C.S.Eng. London, 1863.

Lee,—Nice et son Climat. Par Edwin Lee, M.D. 2ème Edition. Paris, 1863.

Lee,—Syphilitic and Vaccino-Syphilitic Inoculations. By Henry Lee, F.R.C.S. Second Edition. London.

Ross,—Ringworm, Scall-head, Baldness. By George Ross, M.D. London, 1862.

Savage,—Illustration of the Surgery of Female Pelvic Organs. By Henry Savage, M.D. London, 1863.

Shea,—Manual of Animal Physiology. By John Shea, M.D., etc. London, 1863.

Smith,—Enuresis (Incontinence of Urine) in Children and in Adults. By William Abbott Smith, M.D. London, 1862.

Spencer,—Principles of Biology. By Herbert Spencer. London, 1863.

## PERIODICALS RECEIVED.

American Journal of the Medical Sciences,—January. Philadelphia, 1863.

Births, Deaths, and Marriages, Monthly, Supplemental, and Quarterly Returns.

Boston Medical and Surgical Journal,—Jan. 8, 15, 22, 29, 1863.

British Medical Journal,—Jan. 31, Feb. 7, 14, 21. London, 1863.

Bulletin Génèrale de Thérapeutique,—Jan. 1. Paris, 1863.

Dublin Medical Press,—Jan. 28, Feb. 4, 11, 18, 1863.

Dublin Quarterly Journal of Medical Science, Feb. 1863.

Gazette des Hôpitaux,—Jan. 27, 29, 31, Feb. 3, 5, 7, 10, 12, 14, 17. Paris, 1863.

Gazette Hebdomadaire de Médecine, etc.—Jan. 30, Feb. 6, 13, 20, and title & index. Paris, 1863.

Gazette Médicale d'Orient,—January. Constantinople, 1863.]

Gazette Médicale de Paris,—Jan. 31, Feb. 7, 14, 1863.

Health and Meteorology of Manchester,—January 1863.

Journal de Médecine et de Chirurgie,—Feb. Paris, 1863.

Medical Times and Gazette,—Jan. 31, Feb. 7, 14, 21. London, 1863.

Nederlandsch Tijdschrift voor Geneeskunde,—Sept. to Dec. Amsterdam, 1862.

Norsk Magazin for Lægevidenskaben,—No. 10, vol. 16. Christiania, 1862.

Revue de Thérapeutique Medico-Chirurgicale,—Feb. 1, 15. Paris, 1863.

Vierteljahrschrift für die praktische Heilgunde,—Vol. 4. Prague, 1862.

Virchow's Archiv,—Nos. 1 to 6, Vol. 25. Berlin, 1862.

Wochenblatt der Zeitschrift der Aerzte,—Nos. 49 to 52 for 1862. No. 1 for 1863. Vienna.

# Part First.

## ORIGINAL COMMUNICATIONS.

### Article I.—*On the Present Aspect of the Doctrine of Cellular Pathology: a Lecture delivered at an Evening Meeting of the Royal College of Surgeons of Edinburgh, 27th February* 1863. By William Turner, M.B. Lond., F.R.C.S.E., F.R.S.E., Senior Demonstrator of Anatomy in the University.

Since the period when the higher powers of the compound microscope began to be applied to the investigation of morbid processes and the determination of the structure of tumours and other new formations, pathologists have been familiar with the fact, that cells and nuclei, in various stages of development and modifications of shape, enter largely into the composition of the new products of disease. The recognition of this very important structural fact must be regarded as the starting-point of the doctrine of Cellular Pathology.[1]

But whilst all, I suppose, are prepared to grant the cellular nature of morbid growths, and the influence exercised by nucleated cells in pathological processes, yet there has been much difference of opinion respecting the source from whence these cells arise. To attempt to follow out on this occasion the mode of origin of all the structures, and to discuss all the changes which may take place in disease of the textures, would indeed occupy far more time than could reasonably be allotted to a single lecture; and our information on many points is, perhaps, not as yet sufficiently definite to enable one to state with precision many of the events which do occur. But I have thought that it might not be uninteresting to many now present, and quite within the compass of the time placed at my disposal, to give a brief account of some of the many careful and beautiful observations which have within the last few years been recorded on the mode of origin of the corpuscles which occur in such quantities in the so-called inflammatory exudations, and of the formation of the corpuscles of pus, cancer, and tubercle.

And, indeed, it is not inappropriate that we in this place should inquire into the progress which has of late been made in the development of our knowledge of this department of our subject; for it was in this city, nay, within the walls of this very College, now some twenty years ago, that the present learned and distinguished Professor of Anatomy in the University, who at that time occupied the post of Conservator of the Museum of our College, by pointing out the share which the existing cells and nuclei of the textures take in the production and progress of various morbid processes and in the formation of new structures, offered the first satisfactory evidence of, and gave the clue to, the correct conception of the process.[1]

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[1] It was by Johannes Müller and his pupils Schwann and Henle, nearly thirty years ago, that this great step was made in pathology.

But whilst claiming for our own countryman the credit of having adduced the first illustrations of the share taken by the pre-existing cells and nuclei of the textures in the production of various morbid processes, and their increase in these processes by endogenous multiplication,[2] we must at the same time accord to Professor Virchow of Berlin the merit of having, by the vigour of his writings, by his own numerous observations, and the investigations which he has stimulated his pupils to perform, more prominently brought forward the germinative changes which occur in various textural diseases, and in the inflammatory process, and greatly extended their applicability. His Archiv abound with papers by himself and his pupils, from which much information may be derived; and in his Lectures on Cellular Pathology he has placed within our reach a clear and systematic exposition of the subject.

Before proceeding to discuss the evidence which has been advanced of the mode of origin of morbid cell-structures, it may be advisable to say a few words respecting the leading opinions which have been entertained of the manner of development of the cells in the normal formation of the tissues. If we turn to the standard works on the structure and development of the textures, published only a few years ago,[3] we shall find it stated that, in the usual course of development, cells may arise in one or other of two ways:—

1. By the intermediation of and descent from other cells.
2. By the free formation of cells in an amorphous or slightly granular blastema.

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[1] These proofs were advanced by Mr Goodsir in courses of lectures delivered in the College in the summer of 1842, and in the winter 1842-43. The leading facts were afterwards published in the Anatomical and Pathological Observations (Edinburgh, 1845). In a paper "On the Origin and Development of the Diseased Condition of Peyer's Glands, which occur in some forms of Continued Fever," read before the Medico-Chirurgical Society of Edinburgh, February 1842, and published in the London and Edinburgh Monthly Journal, April 1842, and in a paper on "The Structure of the Human Kidney," in the same volume, p. 474, additional illustrations of the share taken by nucleated cells in pathological processes and in the development of new cells are adduced. It is interesting also to note, that the examination of the diseased condition of Peyer's patches was one of the first circumstances which led Virchow to entertain his present conceptions of the mode of origin of new cell-formations (Cellular Pathologie, Lect. 18).

[2] Anat. and Path. Obs., pp. 13, 17, 68, 108, pl. i. figs. 1, 2, 3, 13.

[3] Dr Sharpey's Introduction to Quain's Anatomy, 1848; Kölliker's Handbuch der Gewebelehre, 1852.

The first mode of cell-formation is illustrated by the process of endogenous cell-multiplication, originally pointed out in the early stages of cell-development in the ovum by Martin Barry, and since repeatedly confirmed by numerous subsequent observers. The multiplication of cells by division—fissiparous multiplication—as in the early stages of formation of the blood corpuscles, also comes under this head.

The second mode of cell-formation is that which, originally advanced by Schleiden and Schwann, was adopted, with some slight modifications, by Henle.[1] It constitutes the well-known *cell-theory* of those writers. According to this theory, by a process analogous to that of crystallization, a few granules aggregate together in a fluid blastema, either at once to form the nucleus of a cell, or else at first to form a nucleolus, around which a nucleus is afterwards produced. Around this nucleus matter is then gradually deposited, until at length a cell is formed. This theory of the free formation of cells in a blastema for some years held its ground as the principal method by which new cells arose during the normal development of the textures.

The progress of investigation has, however, gradually caused physiologists to give up first one and then another of the supposed instances of spontaneous, exogenous, free cell-formations, which were originally adduced as examples of the process, until at length the theory of free cell-formation has ceased to hold a place in our conceptions of normal histogenesis;[2] so that the doctrine of the development of normal cells by descent from pre-existing cells is almost universally admitted.

But though banished from the field of normal histogenesis, the mode of origin of cells in a fluid blastema by a free spontaneous generation is still considered by some to be the one which prevails in pathological cell-formations; and the well-known inflammatory exudation-corpuscles, and the cells of pus, cancer, and tubercle, are regarded as produced in this manner. In the pathological processes which lead to the production of these corpuscles, the exudation, which is not unfrequently poured out from the bloodvessels amongst the textures, is looked upon as the equivalent of that fluid blastema in which Schwann supposed the spontaneous cell-formation to occur in the course of normal development. And as small granules and visible molecules are often seen in the exudation, the cells are supposed to arise from them. That an exudation may take place amongst the textures in inflammation is evident enough in many cases, as in phlegmonous erysipelas, the effusion into the sac of an inflamed serous membrane, and the exudation on the surface of a stump after amputation. But that the cell-forms met with in such exudations are produced in them by any precipitation or aggregation of visible molecules is a mere hypothesis, based on no satisfactory observations, and opposed to the whole course of embryological and histogenetic inquiry.

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[1] Although Schleiden is usually considered as a supporter of the theory of free cell-formation, yet that distinguished botanist fully recognised that the new cells of plants were never formed except within those already existing. In support of this statement, I may refer to his Principles of Botany, pp. 103, 257, 465, 527.

[2] The change which has taken place in the statements of physiologists on this matter may be well seen by comparing Kölliker's Handbuch, edition 1852, with his English edition 1860;—see, also, Leydig's Histologie, p. 9. 1857.

The frequent existence of multitudes of small granules and visible molecules in such exudations as the fluid of a pleurisy, or a pericarditis, has apparently led those pathologists who look for the formation of cell-forms by the mere aggregation of such particles to the conclusions they have arrived at. But the more probable explanation of the signification of these particles is to regard them not as histogenetic in their office, but as histolytic, *i.e.*, as products of the disintegration of texture, not as agents in its production. And that the cells both of normal and pathological structures may undergo such a degeneration into granules was long ago conclusively established by the observations of Reinhardt,[1] and has been satisfactorily confirmed by numerous subsequent observers.[2]

And here I may remark how important it is that, in our endeavours to trace the mode of origin of the cell-structures met with in pathological new formations, we should not lose sight of the principles which regulate the development of the normal textures. Undoubtedly, in the two cases the results of such formations vary greatly: in the one being useless and purposeless as regards the wants of the economy, in the other ministering to its requirements. But the principles which regulate the development of a texture, whether it be normal, or whether it be pathological, are the same. The proposition *Omnis cellula e cellulâ* applies alike to each. Thus, whilst all *à priori* reasoning, from the basis of normal development, points to the descent of these pathological structures from previously-existing textural elements, yet in this, as in any other question amenable to careful observation and experiment, we ought not to rest satisfied with such a form of reasoning, but apply ourselves to the investigation of the subject. I shall now, therefore, proceed to the consideration of the evidence, founded on numerous examinations, and recorded by various observers, which has been advanced in support of this doctrine of continuous cell-development.

To render such a doctrine at all admissible, it is necessary that its originators and supporters be in a position to show that in all the textures and organs in which the formation of the corpuscles of pus, cancer, and tubercle take place, pre-existing cellular and nuclear particles occur.

In many of the textures and organs, as in muscular fibre, cartilage, the grey matter of the nervous centres, and the various forms of epithelium, such existing cells and nuclei are universally recognised by anatomists, not only in the early stages of development of those textures, but persisting throughout life. In other parts, again, as in bone and nerve fibre, they have not been so generally admitted; but the observations made, now many years ago, by Mr Goodsir,[1] and since confirmed by Donders, Virchow, Kölliker,[2] and Beale,[3] leave no room for doubting the existence of nuclei in the lacunæ of bone; and their presence in nerve-fibres has now been so frequently figured and described by independent observers, that we cannot but accept them.

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[1] Traube's Beiträge, b. ii. 217.

[2] See, especially, Paget's Lectures on Surgical Pathology, p. 380.

It is, however, to their existence in the connective tissue that attention has of late years been particularly directed, and this more especially by Virchow. In some forms of connective tissue, indeed, as in that which constitutes the thick skin of the Sun-fish, the cells and nuclei may be seen lying amongst the fibres without the use of any reagent.[4] But, as a rule, before a demonstration of these structures can be effected, it is necessary to add to the texture some reagent, which, by rendering the fibres so transparent as to be almost or entirely invisible, permits the cells or nuclei to be seen. And, in the various forms of connective tissue met with in man and the higher animals, these cells, instead of being round or oval as in the skin of the sun-fish, are variously fusiform and stellate in shape. Anatomists had long been familiar with the fact, that, when acetic acid was added to a portion of tendon or fascia, numerous minute oval bodies, previously concealed by the fibres, came into view, and these were described as the nuclei of the texture;[5] but that these nuclei were enclosed within distinct cells—the connective-tissue corpuscles—and that the cells possessed in many cases a radiated and anastomosing arrangement was not known until Donders,[6] Virchow,[7] and Von Wittich[8] pointed it out. The demonstration of these cells in the connective tissue, their connexions with each other, and their relation to the fibrillated bundles, which thus represent merely a form of intercellular substance, must be regarded as a most important contribution to our anatomical and physiological knowledge. For the connective tissue, beyond all others, is most extensively distributed throughout the body, so that an ever present cell and nuclear containing texture is by it provided. Not only does it, in its various modifications, constitute many special structures, as tendons, ligaments, fasciæ, the cornea, vitreous body, Whartonian jelly of the umbilical cord, etc., but it enters into the formation of the skin, mucous, serous, and synovial membranes, of muscles, blood and lymph vessels, of nerve-centres, and of nerves. It is found also in the glands in considerable quantity, varying, however, in amount in different instances, and serves as a sort of framework in which the vessels and tubes or follicles with their epithelial contents are embedded. That it is present in the majority of the parts I have enumerated, all anatomists will, I suppose, admit; but there are not wanting pathologists who deny its existence in some of them, and employ this as an argument against the general applicability of the doctrine of continuous cell-development. Of these parts, therefore, it will be advisable to speak with more precision. Thus it has been stated that no structure containing corpuscles exists in the white substance of the brain, and that the cells of pus, cancer, and tubercle, sometimes found in it, could not, therefore, arise from any such source.[1] But that cell-forms do lie amidst the nerve-fibres of the white substance has been satisfactorily established by the researches of Virchow, who has pointed out a very delicate connective tissue containing cells and nuclei, termed by him Neuroglia,[2] between the fibres and bloodvessels of the white substance, not only in the brain, but in the olfactory and auditory nerves; and I have placed under the microscope, in the adjoining room, several preparations in which the presence of numerous distinct corpuscles between the fibres of the white substance of the healthy human brain may be clearly seen.[3]

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[1] Anat. and Path. Observations, p. 65, 1845.

[2] Microscopic Anatomy, 1860, p. 175.

[3] Gulstonian Lectures, pl. viii. 1861. Mr Goodsir has for many years taught that nuclei exist not only in the lacunæ of bone, but in the delicate cellular membrane which is prolonged along the Haversian canals. The presence of nuclei in the bone-corpuscles I have been in the habit of demonstrating for several sessions to my class, and preparations were exhibited at the lecture.

[4] I have given an account of the structure of the very remarkable skin of this fish in the Natural History Review, April 1862. The cells, or connective-tissue corpuscles, were seen in great numbers lying amidst the fibres, but in no case did I detect a radiated or anastomosing arrangement.

[5] J. Goodsir, Anat. and Path. Obs., p. 1. Reference may also be made to Mr Bowman's article "Muscle" in Todd's Cyclopædia, and to Dr Martin Barry in Phil. Trans., 1841, in recognition of the persistence of nuclei in the textures after development has ceased.

[6] Zeit. für Wissen Zool., bd. iii. p. 348.

[7] Würzburg Verhandl., bd. ii., 1851.

[8] Virchow's Archiv, 1855.

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[1] Dr Bennett, in his Lectures on Clinical Medicine, pp. 153, 216, 3d edition, 1859.

[2] Cellular Pathologie, Lecture 13.

[3] These preparations were made by Mr Stirling, the assistant-conservator to the Anatomical Museum of the University, who has shown great skill in carrying out various methods of displaying the structure of the nervous system. They consisted of a section through the corpus callosum, a section through the white medullary centre of a cerebral convolution, and a section through the white substance of the cerebellum. They were obtained from a healthy human brain hardened in spirit, then steeping the sections for about ten minutes in solution of chromic acid, and afterwards tinting with carmine. By this process the corpuscles, tinted red, could be most accurately examined; and, without entering at this time into any detailed description of their arrangement, it may be sufficient to say that they were well-defined oval or circular corpuscles, in many of which nuclei could be seen, that they were seated between the nerve-fibres, and that they could not be confounded with the nuclei of the coats of the bloodvessels, for, as could be readily and precisely ascertained, they were altogether distinct from them. Lockhart Clarke, also, as quoted below, p. 442, has described nuclei abundantly scattered throughout both the white and grey substance of the cerebrum and cerebellum.

By the observations of Lockhart Clarke[1] and others,[2] a delicate connective tissue, containing multitudes of minute cells and nuclei, has been demonstrated between the fibres and bloodvessels of the columns of the cord, and the presence of connective tissue between the fibres of the ordinary distributory nerves is a matter of everyday observation.

In the kidney, also, some have denied the existence of any connective matrix substance between the tubes and vessels of the cortical part, and the accuracy of the observations originally made by Mr Goodsir[3] has been disputed. But the investigations of Dr Johnson,[4] and, still more recently, the exhaustive treatise of Arnold Beer,[5] have satisfactorily proved the correctness of his statements. The last-named observer has also described and figured the cell-elements of this connective matrix substance.

In the liver the connective tissue may be traced readily enough along the vascular trunks, forming the well-known Glisson's capsule; and in some animals, as the pig, polar bear, and camel,[6] it may be seen distinctly to separate the ultimate lobules of the gland from each other.

The brief review now given will, I think, be sufficient to show that cellular or nuclear bodies exist both in the proper structures of the organs and in the matrix of connective tissue which binds them together. This universal diffusion throughout the textures of bodies possessing definite organic forms is in itself a powerful argument in favour of the supposition that they possess definite properties.[7] The existence of these structures during the periods of development and growth, their persistence after these processes are completed, and the changes which take place in them during disease, all speak strongly of their physiological and pathological importance.

And now we may proceed to the examination of some of the leading pathological changes which take place in the elements of the textures. And, FIRST of all, we will speak of those which occur in inflammation, and which lead to the formation of the so-called exudation-corpuscles and of pus.

1*st*, *In the Connective Tissue.*—This, above all others, is the tissue in which the process of suppuration occurs most frequently and readily, which is in part accounted for by its extensive distribution and in part by its structure. For, in the latter respect, it approaches most nearly to embryonic tissue, *i.e.*, to tissue in which metamorphosis is most readily induced.

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[1] Philosophical Transactions, p. 441, pl. xxii. fig. 48, 1859.

[2] F. Goll. Denk. der Mediz-Chir. gesell. d. Kanton Zurich, 1860. J. Dean, Micros. Anat. of Lumbar Enlargement of Spinal Cord, 1861.

[3] London and Edin. Monthly Journal of Medical Science, May 1842.

[4] Article "Ren" in Todd's Cyclopædia. Mr Bowman also evidently recognised this substance, for he states that "the ducts and bloodvessels are seen to be embedded in a sort of matrix, apparently homogeneous, but probably having a cellular structure." Phil. Trans., 1842, pp. 70, 71. See, also, Kölliker's Handbuch.

[5] Die Bindesubstanz der Menschlichen Niere, Berlin, 1859.

[6] The distinct separation of the lobules of the liver of the camel from each other is beautifully shown in preparations in the comparative anatomy collection in the University of Edinburgh Anatomical Museum.

[7] Goodsir, Anat. and Path. Obs., pp. 1, 24. Paget, Lectures, p. 57, 1st ed. Savory, Phil. Trans., 1855, p. 254.

From recent investigations into the effects of irritants upon, and the process of suppuration in the loose areolar or subcutaneous connective tissue, it has been seen that although an exudation may, and certainly does, not unfrequently take place in it, yet that the changes which result in the formation of pus are set up, not in that exudation, but in the corpuscles of the connective tissue.[1] Their nuclei at first present distinct indications of being about to divide, for constrictions form at intervals in their sides, and as the process advances a complete separation is effected. This division may be multiplied to such an extent as to fill the corpuscles with many nuclei, which gradually assume the characters of pus-cells, and form small microscopic points of pus. Through persistence and extension of the process, the various corpuscles in the affected area undergo this change; their cell-walls and the intercellular substance disappear, the numerous minute spots of pus run together, and an abscess of a size proportioned to the extent of the irritated area is produced. In the more compact forms of connective tissue, as in tendons and periosteum, phenomena of a similar nature have been traced.[2]

In the cornea, which is now regarded as a modified form of connective tissue, the changes induced by irritation and inflammation have been most carefully studied, and the opacity which arises has been seen to commence in the connective-tissue corpuscles.[3] Dr His, who has especially studied the process of inflammation in this structure, has distinctly pointed out that the changes which occur depend upon increase in size of the cornea-cells and division of their nuclei, which division in some cases proceeded to such an extent, that twenty or more nuclei existed in a single mother-cell. By a further continuance of the process, these nuclei gradually assumed the appearance of pus-corpuscles.

2*d*, *In Cartilage.*—In cartilage, a texture in which, as in the cornea, we are enabled, through the absence of bloodvessels, to eliminate all effects which, in the so-called vascular textures, might be supposed to be produced by the occurrence of an exudation, the well-known researches of Professor Goodsir,[4] amplified and extended by Dr Redfern,[5] have proved most conclusively that the inflammatory and ulcerative processes in it are due to the formation of masses of nucleated cells within the cartilage corpuscles. By the persistence of this proliferating process, alteration and destruction of the wall of the cartilage corpuscle and the intercellular substance is occasioned. Dr Redfern, however, expressed doubts, whether from these proliferating cartilage cells, pus corpuscles could be formed. But more recent researches[1] would appear to have demonstrated that, in purulent inflammations of joints, the pus-corpuscles may take their rise not only through changes in the synovial membrane, but through endogenous multiplication in the cartilage-cells themselves.

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[1] Virchow, Lecture 19; Förster, Atlas, taf. xxxi. fig. 3; Billroth, Beiträge, pp. 26 and 37; C. O. Weber, Virchow's Archiv, vol. xv.

[2] Billroth, op. cit., taf. ii. fig. 11; C. O. Weber, op. cit., taf. viii. fig. 1.

[3] Virchow in his Archiv, b. iv.; Strube, Der normale Bau der Cornea, 1851; His, Beiträge sur norm. and path. Hist. der Cornea, Basel, 1856; Rindfleisch, Virchow's Archiv, 17th vol. p. 239, 1859; C. O. Weber, op. cit., p. 11; Förster, Atlas der Pathologischen Anatomie, 1859, taf. xxxiii. fig. 3.

[4] Anat. and Path. Observ., p. 17.

[5] Abnormal Nutrition in Articular Cartilages, 1850.

*3d, In Bone.*—If a bone, in which the processes of ulceration and suppuration are going on, be microscopically examined, it may be seen that, in addition to the softening due to the removal of the earthy matter, other textural changes have set in. The Haversian canals have become dilated by the absorption and removal of their osseous walls, so that adjacent canals are thrown together and spaces and cavities are produced in the texture. At the same time these cavities become filled with a cellular growth—the granulation substance—from the free surface of which pus-corpuscles are thrown off;[2] the immediate cause of the removal and absorption of the bone, and the formation of the spaces and cavities being the development of the cellular growth. But these changes are not confined to the production of a cellular mass in the Haversian canals; for alterations may be observed in the lacunæ and canaliculi of the intermediate osseous texture. Minute drops of fat in the first instance appear in them, and, as this form of degeneration increases, the lacunæ and canaliculi, owing to the softening and absorption of the intervening substance, gradually widen out, until at length several adjacent spaces are thrown together. In this manner, that perforated worm-eaten appearance is produced which is so characteristic of a carious bone.[3] But along with this destruction of the osseous substance, a growth of cells in the enlarged spaces now occurs. These cells would appear to arise in one or other of two ways, either by the multiplication of the original nuclei of the bone lacunæ,[4] or by the extension into the spaces of granulation-cells with accompanying bloodvessels, either from the periosteum, medullary membrane, or Haversian canals.[5] From these granulation-cells pus-cells may proceed in the usual manner.

*4th, In Muscles.*—In inquiring into the effects of irritants upon, and the mode of formation of pus in muscles, we shall experience perhaps somewhat greater difficulties in deciding upon the source from whence the pus-corpuscles arise than in the textures previously considered. For a muscle is in its structure a complex organ, one which consists not merely of the proper contractile muscular fibres, but of a considerable quantity of connective tissue, which subdivides the muscle into its constituent bundles, and also lies between the fibres of which the bundles are composed. Irritating agents applied to a muscle, act not only on this intervening connective tissue, but on the muscular fibres themselves.

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[1] C. O. Weber, Virchow's Archiv, vol. xiii. p. 79; Förster, Atlas, pl. xxxiii. fig. 1, p. 65; Barwell, on Diseases of the Joints, 1861.

[2] Goodsir, op. cit., pp. 15, 71.

[3] Von Bibra, Annalen der Chemie und Pharm., vol. lvii. p. 356, 1846; C. O. Weber, Virchow's Archiv, vol. xv.

[4] Virchow, Lecture 18; Förster, Atlas, taf. xxxi. fig. 6, taf. xxxiv. fig. 4; Barwell, p. 238.

[5] Billroth, Beiträge, p. 51; C. O. Weber, op. cit.

For whilst fully recognising the great influence exercised by the connective tissue or its allies, bone, cartilage, and epithelium, in the various pathological changes which may take place, not only in the parts composed exclusively of these textures, but in those in which they may be intermingled with other tissues, yet I am not disposed to limit the production of these changes so exclusively to the connective substances as Virchow would appear to do, or to exclude the cells and nuclei of the other textures from all participation in the production of morbid structures. And in the case of muscles, the proper nuclei of the fibres undoubtedly play a most important part, not merely in the formation of pus, but, as I shall afterwards point out, in the development and propagation of cancer in their substance.

When inflammation takes place in a muscle, in addition to the long-recognised destruction of the muscular substance occasioned by fatty degeneration of many of the fibres, productive changes may be traced in the nuclei of others of the fibres and in the corpuscles of the interposed connective tissue.[1] These appear to arise in the first place in this connective tissue, in which the phenomena of nuclear division and endogenous cell-multiplication, occasioning the production of large mother-cells, occur in a manner closely corresponding to that which has already been described in the areolar and tendinous tissues. By these processes cell-forms are generated completely identical in appearance with those of pus. The multiplication of these cells in the connective tissue not unfrequently goes on to such an extent as greatly to interfere with the examination of the fibres themselves. But in favourable specimens a distinct nuclear multiplication may be seen to have occurred within the fibres. And this process may be traced in different specimens, from the simple separation of a nucleus into two, to the much more advanced stage in which, through the persistence of this nuclear division, long rows or crowds of corpuscles are formed, which gradually assume the form and general appearance of pus-corpuscles. By the continuance of this process, destruction of the fibre occurs; it assumes a very jagged and irregular outline, and ultimately altogether disappears.

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[1] Virchow, Archiv, iv. p. 313; viii. p. 32; Cellular Pathologie. Förster, Handbuch, p. 383; Atlas, taf. xxxi. fig. 5. Böttcher, Virchow's Archiv, xiii. p. 227. Billroth, op. cit. C. O. Weber, op. cit. Sczelkow, Virchow's Archiv, xix. p. 215, 1860.—Of the above authorities, Weber has most completely worked out these suppurative changes in the muscle; see his plates viii. and ix. Both Böttcher and Weber have described and figured the nuclei both of the muscular fibre and the sarcolemma as contained in distinct cells, which they consider to belong to the system of connective-tissue corpuscles. But, as has been well said by Dr Haldane (Edin. Med. Jour. Nov. 1862, p. 443), whether we adopt this view, or look upon them as nuclei proper to the fibre, the further process of development of the new cells from them is the same.

And here I would especially point out the important evidence which these observations on the origin of pus-cells within the muscular fibre affords of the correctness of the doctrine, that they take their rise from pre-existing textural elements, and not in a structureless or granular exudation. Even the most strenuous advocates for the exudation hypothesis are constrained to admit that the increase of cell-forms in inflammations of cartilage and the cornea is due to germinative changes in the proper cell-elements of those textures. And this they endeavour to explain by stating that these tissues are altogether extra-vascular, and consequently beyond the reach of any exudative infiltration, whilst the muscular, glandular, and other tissues are more or less abundantly supplied with vessels. But though bloodvessels exist in the substance of the muscles, and that too in large numbers, yet the individual fibres—nay, the minutest corpuscles of the interfibrous connective tissue—are in their kind, though not, of course, to the same degree, as extra-vascular as cartilage or the cornea. Are we then to suppose that an exudation poured out from the adjacent vessels passes through the sarcolemma into the substance of the fibre, and that from this the pus-corpuscles found within it take their rise? Such a supposition is in the highest degree improbable; for a muscular fibre is a compact solid structure. It is composed of contractile sarcous particles, with intervening scattered nuclei, which are included within a membranous sarcolemma, and so closely packed together that no space or vacuity in which an exudation could find room exists. The same mode of argument may be applied also in explanation of the formation of pus-corpuscles within the proper elements of others of the textures, as the lacunæ of bone, epithelium, etc., all of which are distinctly extra-vascular. And thus, without doing more than merely allude to the absence of anything like satisfactory proof of the spontaneous origin of pus-corpuscles in an exudation, it is possible, by employing the principle of exclusion, to show the complete insufficiency of such an hypothesis to account for their mode of origin.

*5th*, *In Nerves.*—The production of pus in nerves so closely corresponds to that which I have just described in muscles, that it is needless to enter into details. It may suffice to say, that it may proceed both from the corpuscles of the connective tissue lying between the fibres, either by a multiplication of their nuclei or by an actual division of the cells themselves, and from the proper nuclei of the neurilemma of the fibre.[1]

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[1] Billroth, op. cit., p. 47. C. O. Weber, op. cit., p. 24. Hjelt, Virchow's Archiv, xix. p. 364, 1860.

I may however, perhaps, more particularly refer to an observation of Professor Förster on the origin of the so-called exudation-corpuscles in the white substance of the brain, in a case of encephalitis.[1] New-formed cells existed in such numbers as to obscure more or less completely the nerve-fibres. In some the nucleus was single, but in others two and three were present. In other cases the cells themselves were observed to be constricted in the middle between the two nuclei as if in the act of dividing. When the circumference of the new-formed inflammatory mass was examined, the pre-existing cells of the connective-tissue matrix of the white cerebral substance were seen to be undergoing an abundant increase by division. Through this process the new-formed cells had evidently been produced.

6*th*, *In Mucous Membranes.*—When a mucous membrane inflames, not only is there a discharge, generally profuse, of more or less modified mucus from its surface, but at the same time a rapid production of inflammation-corpuscles occurs, and a great tendency to the formation of pus exists. Whence, then, do these corpuscles arise? To enter properly into the consideration of this question, it will be necessary that we should briefly direct our attention to the anatomy of the membrane itself. The general conception entertained of its structure is that it consists of three parts: 1*st*, An extremely thin, transparent, and homogeneous membrane, perfectly continuous, and without any aperture or appearance of structure. This membrane was particularly described by Mr Bowman, and named by him "basement membrane." 2*d*, An epithelium, varying in its shape and thickness in different localities, and consisting of one or several layers of cells situated on the superficial or free surface of the basement membrane. 3*d*, Areolar connective tissue intimately related to the deeper or attached surface of the basement membrane, and therefore completely separated by it from the epithelium on the free surface. In this connective tissue the nerves, blood and lymph vessels lie. This general conception of the structure has been extended not only to the various ducts, glands, and fossæ, which open on the free surface of any mucous membrane, but to the serous and synovial membranes and the skin; although it is admitted that, whilst the epithelial and connective tissue elements are always capable of demonstration, yet, that the basement membrane is not at all times to be isolated. But numerous careful investigations made of late years into the structure of the mucous and other membranes and the skin have led many anatomists to doubt the existence of any such entire, continuous, homogeneous membrane between and separating the epithelial and connective-tissue elements as Mr Bowman described, and thus it is now believed that no sharp line of demarcation between these textures exists. They gradually pass into each other, and in some localities, indeed, the cell-elements of the one are anatomically continuous with those of the other. It would be possible, did time permit, to adduce many observations in support of this statement, but it may suffice, perhaps, to refer only to those made on the urinary, tracheal, intestinal, and buccal mucous membranes.

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[1] Atlas, p. 63, taf. xxxii. fig. 3.

The structure of the mucous membrane of the bladder, ureters and pelvis of the kidney, has been most carefully described in an essay by Burckhardt.[1] He has pointed out that beneath the flattened scale-like epithelium which forms the free surface of the membrane there exists a second layer of cells, some of which are rounded or oval, others caudate; and these latter are so arranged as to lie with their long axes perpendicular to the surface of the membrane. Many of these cells are much elongated, and possess remarkable modifications of shape.[2] In the intervals between their narrow tail-like processes, the oval and rounded cells lie. The cells of this second layer rest directly upon and not unfrequently their caudate ends pass somewhat into a third layer, called by Burckhardt the matrix. This matrix consists of numerous rounded or oval cells, slightly separated from each other by short and delicate fibres. By its deeper aspect it gradually passes into connective tissue, possessing the usual characters. The bloodvessels pass through the connective tissue into the matrix, and form a capillary network immediately beneath the epithelial cells of the second layer.

In the tracheal mucous membrane, after many observations, the same anatomist satisfied himself that no structureless basement membrane existed between the epithelium and subjacent connective tissue.

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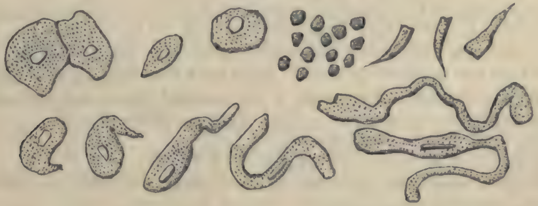
[1] Virchow's Archiv, 17th vol. p. 94, 1859.

[2] Some time ago, Dr W. T. Gairdner requested me to examine a urinary sediment obtained from a man, at that time a patient in one of his wards in the Royal Infirmary. I found in it various cell-forms, as mucus-corpuscles, and cylindrical and squamous epithelium-cells. But, besides these, there were other forms, such as are represented in the lower of the two rows in the accompanying fig. 1. Those to the left were probably cells of the second layer described in the text. But, in addition, several examples of the curiously elongated and variously twisted cells, such as are represented to the right of the same row, were observed. Burckhardt has also figured, from a drawing of Professor Beckmann, cells of a somewhat similar character which were observed in the examination of the mucous membrane of the ureter.

Fig. 1.



In the mucous membrane of the small intestine a very remarkable arrangement connected with the epithelial cells of the intestinal villi has been recently described, which would appear to establish a direct anatomical continuity between them and the corpuscles of the sub-epithelial connective tissue of the villus.[1] Long processes have been seen passing off from the narrow deeper ends of the epithelium-cells, which enter the substance of the villus, and become continuous with the corpuscles of the sub-epithelial connective tissue. The presence of such connecting structures of course does away altogether with the existence of a continuous basement membrane in this locality. In the buccal mucous membrane, also, long processes have been traced by Billroth[2] from the epithelial cells into the sub-epithelial connective tissue of the papillæ of the tongue. It will now, therefore, be more correct to consider a mucous membrane as consisting of one or more layers of epithelial cells resting on a bed of connective tissue.

Various observers have now demonstrated that, when a mucous membrane inflames, the corpuscles of pus may take their rise either in the interior of the epithelial cells themselves or in the corpuscles of the subjacent connective tissue.[3] Thus they have been seen within the enlarged epithelium-cells of a hepatised lung,[4] in the epithelial cells of the bladder and ureter,[5] in the epithelium of the bile-duct in a case of pyle-phlebitis,[6] in the squamous epithelium of the conjunctiva and the ciliated epithelium of the bronchial tubes,[7] and, as Dr Haldane has recently described,[8] in the epithelial cells of the trachea and bronchi in variolous inflammation of those tubes.

But though pus may form in the interior of the epithelial cells, yet it is probable that the chief seat of its production is in the corpuscles of the sub-epithelial connective tissue; and of its origin in this tissue several observations may be adduced. Thus Burckhardt (op. cit.) has described pus-cells as arising from division of the nuclei of the cells of the sub-epithelial matrix layer of the urinary mucous membrane. Rindfleisch (op. cit.) has seen the same changes taking place in the connective tissue of the conjunctival and bronchial mucous membranes; and in these cases, as in the formation of pus in the ordinary areolar connective tissue, with the increase of the pus-cells the fibrous element of the connective tissue gradually disappears.

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[1] Heidenhain in Moleschott's Untersuchungen, vol. iv. p. 251, 1858.

[2] Müller's Archiv, 1858, p. 159. The following additional examples of the connexion between epithelial and sub-epithelial structures have been observed. Luschka (Virchow's Archiv, ix. p. 567) has seen the epithelial cells of the endocardium connected by long processes with the connective-tissue corpuscles. Stilling, Bidder, Kupfer, and Luschka (quoted by C. O. Weber) have traced connexions between the epithelial cells lining the central canal of the cord and the cells of the grey substance. Lockhart Clarke has also described connexions between the epithelial cells of the central canal and the small cells or nuclei immediately outside them. Phil. Trans., 1859, p. 456.

[3] Virchow, Cell. Path., Lecture 19.

[4] Buhl, Virchow's Archiv, vol. xvi. p. 168, 1859.

[5] Remak, ibid., vol. xx. p. 198. Both Remak and Eberth (ibid., vol. xxi. p. 106) have observed appearances which seemed to show that mucus-corpuscles also may form in the interior of the epithelium-cells of the mucous membrane.

[6] Buhl, ibid., vol. xxi. p. 480.

[7] Rindfleisch, ibid., vol. xxi. p. 486.

[8] Edinburgh Medical Journal, November 1862, p. 438.

An examination which I made a few weeks ago of the inflamed mucous membrane of the pelvis of a kidney and ureter illustrated in a very striking manner some of the phenomena connected with the formation of pus in the sub-epithelial connective tissue.[1] The free surface of the membrane was covered with a layer of granular and molecular matter, beneath which, and extending for some distance into the substance of the swollen and thickened membrane, were multitudes of small closely-crowded cells, like pus or the so-called exudation corpuscles. They were so numerous that the natural texture was completely hidden by them. Still deeper the cells diminished in numbers, and arranged themselves in groups, which evidently corresponded to the sites of the original corpuscles of the connective tissue, and between these groups of cells portions of the fibrous elements of the texture might be seen. But in all these deeper parts of the tissue, where the cell-forms were present, and evidently, from their general aspect, most capable of undergoing development and increase, the amount of granular and visible molecular matter, both free and within the cells, was comparatively small. Now, if the hypothesis of the formation of pus or exudation corpuscles by the aggregation of granules or visible molecules were correct, we should naturally expect to have found, in those parts of the inflamed membrane in which the process of cell-formation was to all appearance most actively going on, the molecular and granular matter in considerable quantity. But, on the contrary, it was at a minimum in these localities, and was present in greatest amount on and next the free surface of the membrane, *i.e.*, at the part where, from various causes, as contact with the urine and greater separation from the sources of nutritive supply, disintegration of texture would most naturally take place. Hence I cannot but conclude that the granules and visible molecules found in such cases are products of textural disintegration, and not agents in its formation.

7*th*, *In Serous Membranes*.—In the serous as in the mucous membranes satisfactory evidence has been adduced to show the production of pus both from the epithelial cells and sub-epithelial connective tissue of which these membranes are composed, although it is probable that in them the endogenous formation of pus in the epithelium-cells takes place to a much smaller extent, and is less enduring than in the connective tissue. The investigations of Rindfleisch[1] would indeed appear to prove that when a serous membrane inflames, along with the exudation of a coagulable material from the blood, the epithelium loosens and falls into its constituent cells, the nuclei of which may undergo increase and development, and the epithelium cells themselves even may assume the appearance of pus-corpuscles. But it is more probable, as Cohnheim's observations indicate,[2] that the cells after being detached, being cut off from their nutriment, undergo fatty and granular degeneration, so that their cell-membrane disappears, and their complete destruction takes place.

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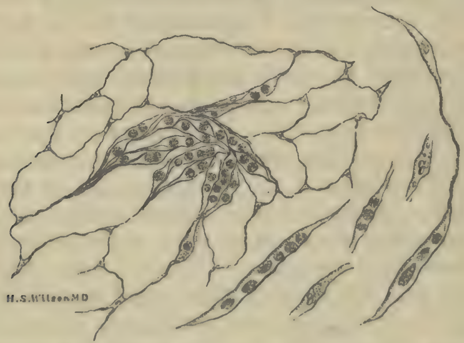
[1] Both in this and the serous membrane described further on, the examination was conducted on thin sections made at right angles to the surface with a Valentin's knife. The sections were treated with glycerine and acetic acid. By this method all the structures may be examined *in situ*, and with no disturbance of their natural relations.

After the epithelium has been detached, the formation of pus goes on in the corpuscles of the sub-epithelial connective tissue. And I may, in support of this statement, describe the appearances which I have seen in the course of the examination of an inflamed pleura, which I made early in the month of January of the present year. The free surface of the membrane was covered by a thin layer of soft yellow inflammatory lymph, beneath which the membrane was swollen and very vascular. The lymph itself consisted of small, pale, faintly granular cells, having the well-known appearance of the so-called exudation-corpuscles. They were compactly arranged, so that the quantity of intercellular substance was but small. The epithelium had altogether disappeared. The sub-epithelial connective-tissue corpuscles were swollen, and, instead of possessing a single nucleus, contained in some cases two, in others a larger number, even up to half-a-dozen, of distinct, pale, in many cases circular, bodies, closely corresponding in form and appearance to the corpuscles of the inflammatory lymph with which the surface

Fig. 2.[3]



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[1] Virchow's Archiv, vol. xxiii. p. 519, 1862.

[2] Ibid., vol. xxii. p. 516.

[3] Fig. 2.—Corpuscles of the sub-epithelial connective tissue of the inflamed pleura described in the text.

of the membrane was covered. These were, without doubt, derived by division from the original nucleus of the connective-tissue cells. This process of endogenous multiplication was seen to the greatest advantage in those parts of the connective tissue in which the proliferation was in its early stage, for here individual as well as small groups of corpuscles could be separately examined; but in those portions of the membrane in which the production of new cells had been for some time going on, the new structures were so numerous as entirely to obscure the original elements of the texture. Changes of an exactly corresponding nature have been traced by Cohnheim (op. cit.), and with much more detailed minuteness, in inflammations both of the pericardial and peritoneal membranes.

8*th*, *In the Skin.*—In the skin, as in the mucous and serous membranes, both epithelial cells and a sub-epithelial connective tissue are met with: the former constituting the cuticle or epidermis, the latter the cutis or true skin. Both the cuticle and cutis participate in the process of suppuration.[1] The corpuscles of the connective tissue, of which the cutis is composed, go through all those productive changes of nuclear division and mother-cell formation which I have already described as occurring in the formation of pus in the connective tissue in other parts. The cuticle or epidermis possesses considerable relative thickness, and consists of many layers of cells, which may be regarded as arranged in two strata—a superficial or horny stratum, and a deeper, softer stratum, known to the older anatomists as the rete Malpighii. The more superficial of these two strata does not appear to take any part in the suppurative process, but either becomes elevated, as in the formation of blisters, or else separates in fine scales, as in the course of pointing of an ordinary abscess. The cells of the rete Malpighii are those in which the pus is produced. They pass through all those changes of constriction and division of the nucleus which result in the formation of groups of nuclei in a single cell, and various forms of division of the cell may not unfrequently be seen. From these, as from the instances already cited, pus corpuscles are produced in a quantity proportioned to the intensity of the process and the extent of the area affected. At the same time, as so frequently happens, not only in inflammation of the skin but of other textures, many of the cells undergo destruction by fatty degeneration.

9*th*, *In Secreting Glands.*—If we were now to direct our attention to the secreting glands, and inquire into the changes which take place in them in the course of the inflammatory process, we should find that both the secreting epithelium-cells and the connective-tissue matrix take part in the process. In illustration, perhaps, it may suffice if we limit ourselves to an examination of one of these glands, and we will take, as an example of the rest, the kidney. It has long been known that the secreting epithelium of the kidney-tubes is materially affected in inflammation of the gland, and the various changes which it undergoes have been so often described that it is needless for me to enter into them; but the part which the connective-tissue matrix plays in the process has been comparatively little studied. The treatise of Arnold Beer, already cited, will, however, supply us with some excellent illustrations of the changes which occur in it. In some forms, and at certain stages, of inflammation of the cortical substance of the kidney, he has observed the interstices between the urinary tubes, normally occupied by the connective-tissue matrix, greatly increased in size and containing a number of small, pale, circular corpuscles, corresponding in appearance to the lymph or white blood-corpuscles; but which, as he fully satisfied himself, were in no way derived from the bloodvessels. Where these changes were only commencing, the influence exercised by the corpuscles of the connective tissue in the production of these new cell-forms might be traced. They were much enlarged, their processes broader and more distinct, their star-like shape in consequence much more strongly pronounced, and in them several round nuclei were to be seen, indicative of the commencement of an endogenous development.[1]

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[1] Virchow, Lecture 19; Billroth, Beiträge, p. 41; Weber, op. cit.

In other cases he has seen in the enlarged intertubular interstices small round cells, having all the character of pus corpuscles (p. 176). In these places, also, the connective-tissue corpuscles were much increased in size, and presented many of the characters of branched ganglion cells; and in these star-like or spindle-shaped structures numerous nuclei were contained. Many other changes have also been traced in the intertubular connective substance, such as the occurrence of fatty degeneration, the spindle-like cells of the connective tissue at first presenting fat-granules in their interior; but as the process continues, all traces of these corpuscles disappear, and the fatty particles are crowded together or scattered about irregularly in the intercellular substance (p. 87). In the same connective-tissue matrix occur those changes which result in the production of the various forms of contracted kidney.

The rapid glance which we have now cast over the productive changes which occur in the textures when the inflammatory process is set up, will, however, I think, be sufficient to show that the formation of the so-called exudation-corpuscles, as well as those of pus, depends upon changes in the textural elements of the parts themselves, and not in the exudation poured out amongst them. But this influence of the textural elements is not limited to the later stages of inflammation, and observations are not wanting to prove that in the early stages also they play a most important part. On this point, however, I must content myself with simply referring to the many excellent observations of Professor Lister,[2] which, from the methodical manner in which they were conducted, from the care which has evidently been taken to eliminate errors, not only in the observations themselves, but in the conclusions which have been drawn from them, must be regarded as most satisfactory contributions to our knowledge.

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[1] Beer describes these changes as constituting an interstitial cellular hyperplasia, p. 56, *e. s.*

[2] Philosophical Transactions, 1858.

II. CANCER.—When a cancer is examined microscopically, it is seen to consist essentially of cells and nuclei, embedded in an interstitial substance or stroma. The cells and nuclei constitute the well-known cancer-cells and nuclei, and present forms and appearances which may be regarded as characteristic of the disease. They are evidently new formations. The stroma, again, may consist either of the remains of the original texture of the part, as is mostly the case in scirrhus, or it may be, like the cancer-cells between which it is situated, a new formation also. In what manner, and from what source, do these new-formed structures arise? To these questions I think I shall be able satisfactorily to reply, that they arise from changes in the textural elements—the pre-existing cells and nuclei—of the part, and do not spring up spontaneously by any process of precipitation or aggregation of granules in a structureless or granular exudation.

In the development of these new-formed cancer-cells, the corpuscles of the connective tissue play an important, though not an exclusive, part; for I think that I shall be able to advance sufficient evidence that the cells and nuclei of others of the textures participate in the process, and this not only in one, but in others of the leading varieties of cancer.

There is, indeed, no organ in the body in which the productive changes leading to the development, growth, and extension of cancer can be more exactly studied than the muscles. For the textures of which they are composed can be readily and conveniently prepared for microscopic examination, the margins of the muscular fibres can be precisely defined, and the alterations of structure occurring in them can be noted without much difficulty. In the scapular muscles of the man whose case Mr Syme has related to us this evening I had such excellent opportunities of tracing many of the phenomena attendant on the development and growth of soft cancer in the muscles, that I could not advance a better illustration of the process.[1] The cancer had evidently commenced at the glenoid fossa of the scapula, and had extended from this part backwards on both the dorsal and ventral surfaces of the bone, so as to form a large and well-marked tumour. By its progress, not only was the glenoid fossa and the neck of the scapula softened and destroyed, but the muscular structures on the dorsal and ventral aspects were involved in, and changed by, the disease. It is of the alterations which they displayed that I shall especially speak. The fibres of the proper muscles of the scapula had to a great extent lost their normal ruddy tint. Those situated over that part of the bone with which the tumour was in connexion were displaced from their position, thickened, and incorporated with the exterior of the tumour, for which they formed a sort of capsule. But this loss of colour was not confined to those parts of the muscles actually in contact with the tumour; it was seen also in the fibres beyond, which were pale, dull yellow, or buff-coloured.

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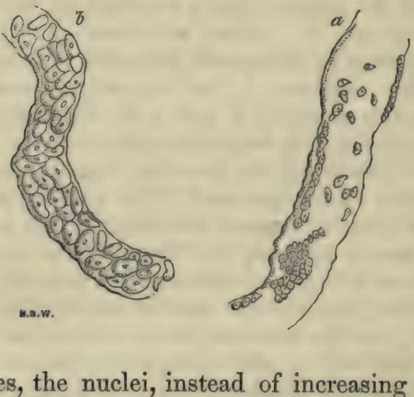
[1] Before the delivery of this Lecture Mr Syme related the case of and exhibited a patient, from whom he had excised the whole of the scapula, leaving, however, the arm, for cancerous disease of the scapula and its muscles. See on p. 951.

When examined microscopically, very important structural changes were seen to have taken place in their interior. In many of the fibres fatty degeneration might be traced in its various gradations, from that first stage where the distinctness of the transverse striation was scarcely affected, and where but a few fatty granules existed, to that later stage where the fibres were so completely filled with highly refracting granules, that all trace of the proper muscular substance was completely destroyed. But in others of the fibres changes of another character could be traced, especially after the addition of acetic acid. These were produced by a multiplication more or less marked in different cases of the nuclei of the fibre. In some fibres the nuclei formed long strings or chains,—those in the same row lying with their ends almost in contact, and closely following each other. The number of nuclei in a row varied considerably in different cases, from two to a dozen, or even more. Some of these strings were situated close to the edge of the fibre, and, therefore, must be regarded as a multiplication of the nuclei of the sarcolemma. Others, again, were in its substance, and produced by a proliferation of the nuclei proper to the fibre itself. In some of the fibres, the nuclei, instead of increasing in this linear manner, had multiplied in such a way as to form little clusters in the

Fig. 3.[1]



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[1] Muscular fibres from the exterior of the soft cancerous tumour, displaying different stages of proliferation of nuclei. *a*, Early stages. *b*, More advanced stage.

centre of the fibre; and in cases in which this increase had largely taken place, the whole diameter of the fibre at that spot was completely occupied by closely packed nuclei. In many specimens, both the linear and clustered arrangements could be seen in the same fibre, and when these were largely developed, all traces of transverse striæ had disappeared, and the fibres were completely filled with the young brood. A still further stage, viz., disintegration of the fibre, was observed in many of these; they had lost their regularity of outline, and deep indentations were present in their sides. They were evidently breaking up and adding their quota of proliferating contents to the collections of nuclei and small oval and rounded cells, which existed in large quantities in those localities in which these disintegrating fibres were seen.

These appearances seem to me to afford conclusive evidence that, when cancer affects the muscles, destruction of their fibres takes place, through proliferation of their own proper nuclei and those of the sarcolemma. And that as this destructive process extends, the cancer grows and involves more and more of the muscular substance. The increase in its size and the enlargement of its boundaries being evidently due to the multiplication of the nuclei, which, through the disintegration of the fibres, are continually being added to the circumference of the cancerous tumour, and there assume the appearance and characters of true cancerous elements.

In the instance I have now brought forward, it has been stated that two changes—the one fatty degeneration, the other nuclear proliferation—were taking place in the fibres of the muscles of the scapula. But the product, in the one case, was merely fat, which of itself possesses no further power of increase; whilst, in the other, nuclei were produced, which, by their subsequent development and growth, assisted materially in the extension of the disease.

The development and growth of cancer in muscles, through multiplication of their nuclear elements, is not, however, confined to soft cancer, but is shared also both by epithelial cancer and by scirrhus. In scirrhus, excellent illustrations have been figured by Professor Weber of Bonn, in the muscular fibres of the tongue,[1] and by Dr Neumann in the pectoralis major muscle, in a case of hard cancer of the breast;[2] but, as their observations in most particulars agree with those I have just related, it is needless for me to detail them.[3] In the muscles of the tongue and lips, Weber has also traced (pl. xi. figs. 1, 2, 3, 4, 5, 6, 7) the development of epithelial cancer, not only through proliferation of the nuclei of the muscular fibres, but of those of the connective-tissue corpuscles also. By which processes, through endogenous reproduction, large mother-cells possessing epithelial characters were formed.

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[1] Op. cit., pl. xi. fig. 10.

[2] Virchow's Archiv, vol. xx. p. 152, 1860.

[3] Dr Haldane has, in the Edinburgh Medical Journal, Nov. 1862, p. 439, recorded a case, apparently of soft cancer, in which such changes as I have described were taking place in the muscles. In the same paper, he gives an excellent account of the production of a new formation, which he considers to be syphilitic, in the muscular substance of the heart, by proliferation of the nuclei of its fibres.

Now, in all the cases I have adduced, the formation of the cancer structures might be traced from the first division of the nuclei of the fibre up to the completed stage of cancer-cell development. And that, too, in a texture which, from its structure, gave no room for the existence of any exudation in it, and in which, therefore, the new-formed structures must have arisen from changes in the pre-existing elements of the tissue. And, indeed, a careful perusal of the writings of one of the most active advocates of the hypothesis, that cancer-cells arise by an aggregation of granules in an exudation, has satisfied me that, as yet, no conclusive evidence has been advanced in its support.

I have entered somewhat at length into the mode of development and growth of cancer in the muscular fibres, partly because it is in them that I have obtained the most satisfactory proofs of the nature of the process, and partly because from the construction of the fibre there is less possibility of committing errors of observation than in others of the textures; yet I do not wish it to be supposed it is in the muscles only that the textural changes, resulting in the production of cancerous growths, can be or have been followed. In the connective tissue Virchow first traced the development of cancer from pre-existing tissue elements, and he has figured and described the process from a case of carcinoma of the breast, in his nineteenth lecture. Professor Förster has also beautifully illustrated its production in this tissue in a case of carcinoma ventriculi (taf. xxvii. fig. 1). In this case, all the stages might be seen from the simple multiplication of the nuclei of the connective-tissue corpuscles to the more advanced condition in which the corpuscles were distended, with crowds of the new-formed cancer-cells.

These examples may be sufficient to show that pathologists are quite justified in ascribing the origin of cancer to changes in the pre-existing tissue elements. Their growth also in the locality which they primarily affect may, in a great measure, be accounted for by an extension of these textural changes beyond the spot in which they first began, so that not only the tissue first affected, but even those adjacent, may become involved in the disease. But we may consider that there is also proved for cancer a power of spontaneous growth, not only of the individual cells, but of the entire mass; and this latter by the endogenous multiplication of the nuclei and the formation of brood or mother cells. Examples of this mode of increase have been figured by Virchow,[1] Paget,[2] Bennett,[3] and Förster.[4] Some time ago, in examining portions of a soft cancer from the lower end of the femur, I saw many examples of these brood-cells, and in illustration of the process I append the following figure.

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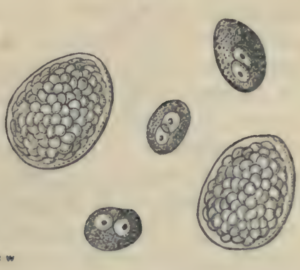
[1] Archiv, iii. 197.

[2] Lectures, vol. ii. p. 439.

[3] Op. cit., p. 215.

[4] Atlas, taf. xxvii. fig. 2.

Fig. 4.



The extension of cancer in a part, through the textural changes just described, opens up a very interesting question, and one which ought not to be altogether passed over. In how far is the proliferation of nuclei in the tissues in the neighbourhood of a cancer to be taken as an index of the commencement of the disease in those tissues? Pathologists have long recognised[1] that cancer-cells not unfrequently are infiltrated amongst the tissues for some distance beyond the actual seat of the tumour itself, and that therefore the great rule followed by surgeons, of removing, not only the tumour, but a portion of the tissues surrounding it, is based upon a sound principle. Now, from what I have already stated, it would certainly appear that, in endeavouring to estimate the extent to which the surrounding textures are involved in the disease, it is not sufficient to take into consideration those parts merely in which well-marked cancer-cells are met with; but that any appearance indicative of multiplication of the nuclei of the pre-existing tissue elements, inasmuch as this is a stage initiatory to the development of cancer-cells, is to be regarded with suspicion, although this multiplication, inasmuch as it also precedes the formation of the corpuscles both of pus and tubercle, cannot be looked upon as conclusive.

III. Tubercle.—The peculiar material known as tubercle possesses, for the most part, one or other of two chief forms, which are distinguished as the miliary or granular and the cheesy tubercle; and between these many intermediate varieties may occur. Of these two leading forms the miliary is the most typical. When examined microscopically, it is seen to consist of small, pale, nucleated cells, some of which possess a single, others several nuclei. The cheesy tubercle, again, not only contains such cell-forms as are met with in the miliary, but many others, which, from their shrivelled, shrunken aspect, or from the fatty granules contained in their interior, indicate the commencement of degenerative changes. Mingled with these cell-forms are numerous granules and molecules, either fatty, calcareous, or pigmentary, with cholestearine crystals and fragments of tissue, evidently the products either of degenerative changes in the tubercle-cells themselves, or in inflammatory material in the adjacent parts, or derived from disintegration of the texture in which the tubercle is situated.

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[1] See Bennett on Cancerous and Cancroid Growths, p. 104. 1849.

How then do these tubercle-cells arise? The doctrine entertained up to a very recent period, and still taught by many pathologists, is that they arise in an exudation or deposit from the blood poured out into the tissue or organ in which the tubercular matter occurs. But evidence has been advanced to show that the tubercle-corpuscles, like those of pus and cancer, may and do take their origin in changes in the textural elements of the parts themselves. For proofs of this, as of so many of the other matters discussed in this Lecture, as well as for the first conception of this mode of origin of tubercle, we are indebted to Professor Virchow. He has pointed out that the connective tissue is the source from which these tubercle-cells proceed, and that their formation is always preceded and accompanied by changes in its corpuscles (Cel. Path., Lecture 20), which are closely comparable with those already described in the formation of the inflammatory exudation-corpuscles and of pus. By the persistence of this process in a part, a small cellular knot or tubercle, the miliary tubercle, is produced. Förster has also traced and illustrated in his Atlas (taf. xxxvi. fig. 1) the development of tubercle from the connective-tissue corpuscles in a case of pulmonary tuberculosis.

But when these tubercle-cells are once formed, they do not appear to possess as a rule much power of spontaneous growth, or even of maintaining themselves for any length of time, and in this respect they present an important difference to cancer, the cells of which not only grow and maintain themselves, but even multiply through endogenous development.[1] The cells of tubercle exhibit decided tendencies to shrivel, or wither, or to undergo fatty or other degeneration, and produce either cheesy tubercle, or some of the intermediate varieties between that and the miliary.

The epithelium-cells, also, in certain localities, would appear to assist in the production of tubercle by the formation in their interior of a brood of young corpuscles. But on this point I must satisfy myself with referring to the observations of Virchow and Schroeder Van der Kolk on the origin of tubercle from changes in the epithelial cells of the air vesicles.[2]

It would therefore appear, from what has now been stated, that the changes in the textural elements which lead to the formation of the corpuscles of pus, cancer, and tubercle, seem, up to a certain

[1] Rindfleisch, however, in a recent paper in Virchow's Archiv (vol. xxiv. p. 571), describes endogenous development in the tubercle-cells in a case of miliary tubercle of the base of the brain. He states that he has traced the development of the tubercle-cells in this case from the nuclei of the external coat of the arteries of the pia mater.

[2] Quoted in Paget's Lectures, vol. ii. p. 595.

time, to be the same, or that, so far as our means of observation enable us to determine, closely corresponding processes occur. Then, after reaching a certain stage, the resemblance between the processes ceases, and the corpuscles begin to assume their specific characters; although it not unfrequently happens that, after the development has apparently been completed, it is very difficult to decide, by a mere inspection, what is the nature of the corpuscles before us. But what it is that gives the specific characters to these corpuscles, I do not attempt to explain. It is still one of the mysteries of organization, and a satisfactory reply can only be given to it when the question, What is disease? is definitely answered.

But I trust that the necessarily brief review which we have now made over this very important department of pathology will be sufficient to show that the doctrine, that pathological cell-formations arise from pre-existing textural elements, has not been founded on insufficient data. And although I have far from exhausted all that might have been adduced in its support, yet enough, I think, has been brought forward to show that it is deserving of proper and careful consideration. Moreover, its universal applicability commends it to our notice. It explains the production of new formations, not in some merely, but in all the textures, and does not, like the exudation hypothesis, confessedly imperfect in the cases of inflammation of cartilage and the cornea, leave any residual quantities unaccounted for.

And to all this it may be added, that it is a doctrine based upon the most precise knowledge of the anatomy of the textures which has yet been attained. And, as daily observation teaches us, it is not until after a correct appreciation of the structure of a part has been acquired that we can hope to obtain an exact conception of the nature of the diseases to which it is liable. Every improvement, therefore, in our methods of anatomical investigation—every new fact added to our store of textural knowledge—gives precision to our pathological theories; and as years roll on, and fact is added to fact, we may in time, perhaps, be enabled to give a correct reply to the question, What is disease? and, still more importantly, to resolve the great problem of its effectual treatment.

## ARTICLE II.—*Cases of Ovariotomy.* By THOMAS KEITH, M.D., F.R.C.S.E.

MRS S., aged fifty-five, applied to Dr Thomson of Dalkeith, towards the end of August last, on account of an abdominal enlargement, accompanied by severe pain referred to a small spot under the ribs in the region of the gall-bladder. On examination, the abdomen was found to be occupied by a large ovarian tumour, to which her attention had been first directed six months before. Up to the time of the appearance of the tumour her health had been good: she had worked hard all her life, and had brought up a large family. She remained under Dr Thomson's observation for the next three months, and under the remedies employed the pain diminished, but the tumour continued to increase. She lost flesh rapidly, and her general health gave way.

Dr Thomson asked me to see her in the beginning of December last. She was then very thin and very sallow, otherwise healthy enough looking. The tumour was multilocular, and no cyst of any size could be detected. It filled up the whole abdomen, pushing the ribs outwards, and projecting in a suspicious manner to the right side, bulging the flank outwards, suggesting adhesions. But at that time these were supposed not to exist, owing to the fact that the integuments over this side of the tumour could be moved as freely and easily as over any other part of the abdomen. In the right iliac region there was a peculiar feeling of crepitation perceptible when the skin was pinched up, and adhesions were accordingly supposed to exist at this point. On the left side the abdomen felt soft, except under the ribs, where there were several hard masses to be felt. There was no line of separation between the hepatic dulness and that of the tumour, and the pain formerly complained of in this region had now ceased. The uterus was pushed downwards, but was otherwise normal and movable. A large soft cyst filled the upper part of the pelvic cavity, but no impulse could be communicated to it through the abdominal wall.

For a case such as this, there was evidently no middle course between letting the patient alone, and removing the tumour. And it seemed to me that she had now arrived at that stage of her malady, when I believe we are not only fully justified in proposing ovariotomy, but culpably negligent if we do not do so. For her health was rapidly giving way; she was able no longer to procure her livelihood; a feverish state of the system was being set up; her stomach was irritable; she suffered greatly from distention; her nights were wearisome and restless; she walked with difficulty; and her breathing was becoming more and more impeded.

Having made up her mind to have the tumour removed, she was brought to town in the end of December. Meanwhile, in the course of three weeks, her girth had increased from 39¾ inches to 43½ inches, the distance between the ensiform cartilage and pubis being 23 inches. For several days preceding the operation she was so uneasy that she could neither sit nor lie in any position for more than a few minutes at a time.

The tumour was removed on the 7th of January last. There were present Dr Thomson of Dalkeith, Dr Thomson of Inveresk, Dr Graham Weir, Dr Keiller, and Dr Keith. The day was cold, and the temperature of the room could not be raised above 65°, and owing to a dense fog there was so little light that it was a question whether the operation should be gone on with or not; but as the patient had spent the preceding nights in great distress, leaning over the edge of the bed, it was thought advisable not to delay. The tumour was accordingly exposed by an incision about ten inches in length, and some slight adhesions along the line of incision were broken down. The hand passed freely to the left of the incision, but adhesions were encountered over the whole extent of the right side. In the iliac region these gave way readily to the hand, up to a line leading from the spine of the ilium to the umbilicus; but over the whole space above this the adhesions were very startling, and of the most firm description. Before attempting to separate these, the tumour was reduced as much as possible by tapping and breaking down the various cysts, all of which were of small size, and all filled with a thick dark grumous fluid. The openings into these were then tied, in the hope of preventing the escape of any of their contents into the cavity of the abdomen; but the cyst walls were so very thin that on the least pressure they burst in all directions, and some at least of this grumous fluid must have found its way over the intestines. The incision was then extended slightly above the umbilicus, the line of demarcation of the cystic and parietal layers of peritoneum brought into view, and the separation of the adhesions proceeded with. This was a long and tedious business, for there was at least a square foot and a half of densely organized adhesion, against which the hand made no impression. The dissection was accomplished chiefly with the point of the scissors, sometimes using the edge freely to divide any bands firmer than the rest. As the separation reached the edges of the ribs a large piece of bowel, strongly adherent, was separated from the upper part of the tumour. The adhesions then became less firm; some of the delicate texture of the mesentery and omentum were slightly adherent here, till, close to the margin of the liver, the free edge was reached. Some other adhesions were then detached, the flaccid cysts drawn out, the calliper clamp applied, and the tumour cut away. The pedicle was formed by the left-broad ligament. It was of fair length and thickness, and was secured without much dragging upon the uterus. The wound was then closed by ten long needles, after all bleeding had been carefully stopped. The needles were passed through the whole thickness of the abdominal wall, including about half an inch of peritoneum. The patient was then placed in a warm bed, and in a wonderfully good state, notwithstanding that the operation had extended over two hours, and the exposure of the parts had of necessity been great.

The weight of the cyst walls and the fluid contents collected was upwards of forty pounds; but a good deal had been lost from the bursting of several of the cysts during the operation, so that the actual weight of the tumour must have been at least forty-five pounds.

Everything went well with this case. The wound healed without the discharge of a single drop of matter; quiet and refreshing sleep took the place of the restless and weary nights of the previous months; in ten days she was thoroughly convalescent; she was walking through the room when I called on the eighteenth day; and a month after the operation she went home to Dalkeith.

Mrs G., aged twenty-four, was brought to my house in a cab, one evening in November 1861. She was a very little woman, delicate and anæmic-looking, with an unhealthy, broken-down appearance, and even then was much emaciated. She mentioned to me that for some years past her usual monthly periods had been irregular, and that nearly a year ago she had become sensible of a fulness in the abdomen. This, however, gave rise to no discomfort till the preceding April, when she had a severe attack of pain in the tumour, which confined her to bed for a fortnight. After this her size increased with great rapidity. She was, however, allowed to marry in June, and, strange to say, she did not think of seeking for any advice till six months after her marriage, by which time her general health had completely given way, and slight exertion brought on breathlessness.

The abdomen was occupied by a large multilocular ovarian tumour, extending to the ensiform cartilage. The cysts were apparently small, the largest being in front below the umbilicus. On the left side, close to the ribs, there was a huge solid mass. Along the upper edge of the tumour she had had from time to time a good deal of pain, and occasionally felt there a creaking sensation. The uterus was retroverted, and movable in all directions.

The case was manifestly one in which, owing to the smallness of the cysts, no benefit, except of the most temporary nature, was reasonably to be expected from tapping. But at this time her general health was so bad that extirpation was not to be thought of. I resolved, however, to recommend the operation should she be brought into a sufficiently good state of health. Under the use of iron, she improved so much in the course of a month, that I did not feel justified at that time in pushing the question of ovariotomy, but kept the case under observation for the next six months. For some months after this she was able to go about with wonderfully little inconvenience, and she could walk to my house and back—a distance of about a mile—without fatigue. The tumour, however, steadily increased, and I proposed to remove it as soon as it should appear that her life was seriously threatened. Her condition at this time was most favourable for ovariotomy; and with this end in view she was repeatedly examined by my brother and myself. Adhesions were supposed to exist along the upper part of the tumour. This conclusion was arrived at from the pain and creaking sensations which at one time she had felt in that quarter.

At this stage of her illness, on calling on her one day, I found she had just been tapped by another medical man, and finding that he had thus assumed the responsibilities of the case, I ceased my attendance. I was thus relieved of not a little anxiety, for at that time there had been no successful case of ovariotomy in Scotland since 1825, when the operation was successfully performed, for the first time in Europe, by the late Mr Lizars.

After an interval of nearly eight months, this patient was taken to Professor Simpson about the end of January last, and I then saw her again, at his request. During this interval she had had no attendance. The circumstances of the case were now much changed: her emaciation was extreme, for the circumference of the arm over the prominence of the biceps measured barely four inches. When laid down in bed she could rarely get up without assistance; she could scarcely drag herself along, and every day she felt the little strength she had leaving her. In her present state she felt life not desirable, and was now very urgent to have the tumour removed, and quite prepared to incur the risks attendant upon the operation. She was the more anxious that her request should be complied with, on account of the fate of a friend of her own, likewise affected with ovarian dropsy, who had died after much suffering in the Infirmary, after repeated tappings and injections, a short time before.

The appearance of the tumour was now most startling. It had increased immensely, and the abdominal walls were very œdematous. The ribs were pushed far outwards, and the ensiform cartilage pointed upwards; and, as she lay in bed, the tumour hung down over the pubis, concealing the upper part of the thighs. Her girth round the umbilicus was upwards of four feet, and the distance between the ensiform cartilage and pubis was fully two feet and a half. The tumour had by this time encroached so much upon the cavity of the chest, that the heart was displaced, and its apex was beating against the third rib. Relatively to the size of the patient, who was a little spare woman, it was the largest ovarian tumour I had ever seen.

She told me that after the tapping, from which she had been led to expect great things, she measured herself with a string, and was disappointed to find that her girth was exactly the same as before. The amount of fluid removed was little more than a gallon; and, though her breathing was at once relieved by the tapping, in three days she was as uncomfortable as before. But after this she first noticed a thickened state of the parts round the opening. This slowly increased till the whole abdominal wall became œdematous, and took on a hard brawny feeling, as if affected with erysipelas.

Under these circumstances, favourable now no longer, I was asked to remove the tumour; and, at first sight of her, I must say, that, from the large size which the tumour had attained, from her extreme emaciation, from the unfavourable state of the abdominal wall, and especially from the great displacement of the heart, my impression was, that the time for interference was past, and that the disease should be allowed to run its natural course.

But, however anxious I might be at the time not to operate in desperate cases, I found I could not refuse; for the patient was young, with the prospect of a long life before her, free of suffering, in the event of a favourable issue. She had quite made up her mind, and though she was made fully aware that the chances were against her, she was sanguine and hopeful as to the result.

She was again examined by Professor Simpson and other friends, and the important question as to the amount of adhesion was carefully considered. From my previous examinations of the case, I believed that more or less adhesion existed along the upper edge of the tumour about a year before. But now there was distinct evidence of a layer of ascitic fluid, which did not exist even when Professor Simpson first saw her about ten days previously. The wave thrill was most distinct over every part of the abdomen, and this seemed to indicate that the adhesions could not be very extensive. The fact, too, that her girth had remained unaffected by the tapping, appeared to show that the upper part of the tumour had at that time considerably subsided. It was therefore hoped that no difficulty would be met with in removing the tumour.

After remaining quietly in bed for a few days, for the circumstances of the case forbade any farther delay, the operation was performed on the 6th of February. The distance between the umbilicus and pubis being sixteen inches, the tumour was exposed by an incision of nearly that length. The tissues were hard and brawny, and, considering their great distention by the tumour, of great thickness, especially near the pubis. The amount of ascitic fluid was much smaller than previous examination had led one to expect. The hand was at once passed to the left under the ribs, where a large mass of solid matter was known to exist. There, I was glad to find no adhesions. But to the right, above the umbilicus, the adhesions were felt to be extensive. The incision was extended upwards three or four inches, and the size of the tumour reduced as rapidly as possible, by emptying the various cysts. The anterior cyst, with walls of great thickness, contained upwards of a gallon of thick dark grumous bloody fluid. The contents of the other cysts were more or less clear. The adhesions first brought into view were near the umbilicus. They consisted of numerous thin fascia-like prolongations, several inches in length, dipping down from above into the tumour. These were not at any one point of great extent; often single thin cords; but very numerous. The first of these bands, being very much upon the stretch, seemed to contain no bloodvessels and was mostly torn through, and what resisted tearing was divided; but as the separation went on, I was soon made aware, by the amount of hæmorrhage,—most of which, however, seemed to come from the surface of the tumour,—that the adhesions must have contained numerous large vessels. Near the epigastrium the adhesions became closer, but gave way readily to the hand. Some bands along the upper surface of the tumour adhered firmly to the margins of the ribs, and from the posterior aspect of the tumour, there were numerous bands which connected it to the ribs, close to the posterior edge of the liver, which was very much displaced upwards. All these were broken down, some slighter adhesions about the head of the colon gave way as the tumour was withdrawn, as well as two pieces of adherent omentum, which, as they contained vessels, were tied with silver wire. After securing the pedicle, the mass of the tumour was cut away.

Thus far there had been no delay or difficulty, and it was hoped that, from the contraction of the abdominal walls upon the thin vessels that ran into the tumour, the bleeding would soon cease, as it had done in the preceding case, where the adhesions had been much more extensive and formidable. This was not the case, however, for on holding aside the wall, there was free oozing from a vast number of points, and what, during the separation of the adhesions, seemed to be nothing but thin fibrous cords, stretched and drawn downwards by the weight of the tumour, were now found to be numerous long thin wormy-like vessels, some several inches in length and of great size, hanging in great stringy bunches from the inner surface of the abdominal wall. These began to bleed freely, showing at once their real nature. Many of them were secured singly by silver wire, and the ends cut off close; others were tied with one ligature, four or five together. At the upper end of the incision the bleeding was troublesome, and there several oozing points were secured by silk ligatures, and the ends brought outside. After a great number of points had been thus secured, and when the hæmorrhage appeared almost to have ceased, a rather free use of the sponge, to wipe off some small adherent clots, removed likewise nearly the whole of the loops of silver-wire sutures; and the greater part of these had to be secured again, —this time with silk, or ligatures of horse-hair, which answered extremely well. Still there was some blood coming down from behind the liver, and on gently pushing the liver upwards against a soft sponge, two bleeding points were brought into view far up near the diaphragm. These were secured with some difficulty. It was now observed that when the wall was put on the stretch, numerous fresh points gave forth a little blood, not in any quantity, but just sufficient to prevent one risking the closing of the wound; and it was also observed that when the wall was pressed downwards and let alone, this oozing ceased. The wound was at length closed to my satisfaction in the usual way, and the patient put to bed in a better state than could have been expected; for the exposure of the abdominal cavity and its contents—especially the liver and diaphragm—had unavoidably extended to nearly two hours. At the very most, not more than twenty ounces of blood had been lost during the whole operation.

The weight of the tumour was 63 lbs. 9 oz.

As the effects of the chloroform passed off, the patient vomited freely, but soon rallied, and a small opiate was given to relieve pain. By the evening she was very comfortable, and passed water quite freely. She was left at half-past one in the morning, sleeping and breathing quietly, and in a most promising state. I was asked to see her about five o'clock, and found her very nervous, with somewhat accelerated breathing, but with a quiet pulse. She had got, against instructions, an opiate, besides a half-grain morphia suppository; and then it became necessary, for other reasons, to remove her attendant. By nine o'clock this unfavourable condition had passed off, and she said she felt much better. About eleven, she asked to be raised up in bed, saying she wanted more air; and as this was being done, she suddenly died.

The edges of the wound were found, on post-mortem examination, to be closely glued together, and there was no adhesion to the intestines along the peritoneal line of incision. Slight adhesion existed between the bowels and wall, where adhesions had been separated, but beyond the edges of these patches the exudation had not spread. There was not the slightest trace of general peritonitis, and there was no clot, even of the smallest size, anywhere in the abdominal cavity. There was, however, some slightly bloody fluid in the pelvis.

In this case there can, I think, be no doubt that death was caused by syncope, easily accounted for by the great displacement of the heart, for its apex was beating against the third rib. Had the tumour been removed at an earlier period of its progress, before it had attained such dimensions as to interfere so seriously with the position of the heart and bloodvessels, and before the vital powers of the patient had become so feeble, it is but reasonable to suppose that the chances of success would have been infinitely greater. And had the abdominal wall been in a healthy state, I cannot help thinking that the hæmorrhage would have given little trouble, and the operation would, in consequence, have been much safer, from the smaller amount of exposure of the abdominal cavity and handling of its contents that would necessarily have been needed. As it was, judging from the post-mortem appearances, the patient was not far from getting well.

## ARTICLE III.—*Case of Gangrene of the Foot and Leg, depending upon Obstruction of the Femoral Artery at the Groin.* By THOMAS ANNANDALE, F.R.C.S.E., M.R.C.S.(Eng.).

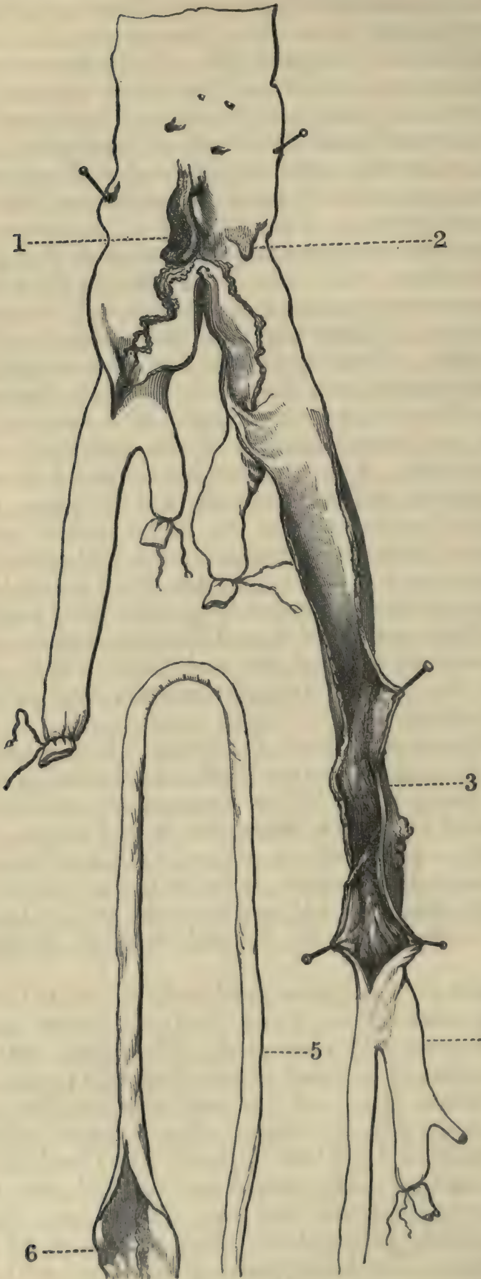
(*Read before the Medico-Chirurgical Society of Edinburgh, March* 4, 1863.)

MRS H., æt. 70, for the last twenty years had been supposed to labour under cardiac disease, but, during this time, with the exception of occasional palpitation of the heart, had enjoyed good health. On the 30th of July 1862, while walking in the street with a friend, she suddenly felt a pain in her left leg, which became so severe that she was taken into a neighbouring house, and from thence conveyed home in a cab. On arriving at home, it was found that the left foot was quite cold. Dr Dickson saw the patient the same evening, and discovered that there was no pulsation in the femoral artery at the groin of the affected limb; the corresponding foot was quite cold, and devoid of all sensibility. During the following night she was very restless, and suffered much from a severe burning pain in the foot and leg, as if a hot iron were being applied to these parts. Two days after, numerous vesications formed on the foot and leg, and it was evident that the foot was dead. At this stage of the case Mr Syme saw the patient. The leg and foot presented a coloured appearance, red, yellow, and black tints being mixed. There was still no pulsation in the femoral artery at the groin. Non-interference was recommended.

The gangrene spread very slowly up the limb, leaving the foot dry, and resembling in appearance that of a mummy. About a fortnight after the commencement of the attack, a large slough formed on the thigh. A second consultation was held on the case, and non-interference still advised. In the beginning of October, the gangrene had reached a point two inches below the knee-joint, and here a line of demarcation began to form, and gradually severed the soft textures down to the bone. On the 11th of October, the slough having separated from the thigh, Mr Syme sawed through the tibia and fibula at this point, and thus removed the gangrenous limb. On examining the limb removed, there were no appearances to account for the disease: both the anterior and posterior tibial arteries were pervious.

After the removal of the limb, the patient's health improved, the sore in the thigh began to cicatrize, and granulations to form on the ends of the divided bones. This improvement continued till the commencement of December, when she turned worse, complained of pain in the knee-joint of the affected limb, and had also some difficulty in her breathing, with expectoration of purulent matter; her appetite also began to fail, and she sank and died on the 18th of December. Pulsation in the femoral artery at the groin never returned.

In the presence of Dr Dickson, and assisted by Dr Pow, I made a post-mortem examination, about thirty-six hours after death. The body was extremely emaciated. The stump was partially healed, but a portion of the end of the tibia still protruded. The knee-joint contained pus; and the cartilages between the outer condyle of the femur and the corresponding articular surface of the tibia were ulcerated, and the bone exposed; the remaining articular surfaces were healthy. The wound in the thigh was cicatrized. The right lung was infiltrated with pus; the left lung was healthy. The liver was slightly enlarged, and there was one small gall-stone in the gall-bladder. The kidneys and spleen were healthy. Liga-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tures having been placed around the innominate, left carotid, and left subclavian arteries, the heart, aorta in its whole course, both iliacs, and the femoral artery of the affected limb, were exposed. These vessels presented no peculiarity externally, with the exception of about two inches of the left femoral artery, extending from the groin to the origin of the profunda. This portion of the artery was slightly enlarged, felt hard to the touch, and was of a bluish colour. Below this enlargement the femoral artery was shrunk, and felt like a cord [*see* woodcut (5) ]. Its profunda branch was natural in size, but looked much larger than usual, in comparison with the contracted femoral (4). The heart being opened, was found perfectly healthy. The aorta was now slit up to within two inches of its bifurcation into the iliacs, and, with the exception of a few small patches of atheromatous deposit, which had not as yet affected the internal coat of the vessel, was healthy. The remaining portion of the vessels, previously exposed, was removed for careful examination.

The arteries removed having been opened, the following appearances presented themselves. The lower part of the aorta was extensively affected with atheromatous disease: about three-quarters of an inch above its bifurcation there was a clot,—evidently of some standing, from its mottled appearance,—firmly adhering by its extremities to a patch of diseased vessel. This clot was about one inch in length and half an inch in breadth, its centre being unattached to the walls of the vessel,—resembling in this respect some of the muscular bands seen in the interior of the heart (1). From this clot there descended on each side into either common iliac artery, and down into their corresponding internal and external iliac branches, a thin narrow clot, of a light red tint, and of more recent formation. At the commencement of the left common iliac artery, there was a patch of atheroma, which had become calcified; and immediately below it was another small clot, of a pyramidal shape, adhering by its base to the wall of the artery, its apex being free, and projecting, nipple-like, into the canal of the vessel (2). The right and left common, internal, and external iliac arteries, all showed here and there deposits of atheroma underneath their internal coats. The left internal iliac artery was slightly larger than its fellow on the opposite side.

The enlargement of the femoral artery, before noticed, began immediately at the commencement of the vessel, and terminated at the origin of the profunda. This portion of the artery was quite impervious, its canal being occupied by a dense clot of a bluish tint, firmly adherent for the upper two-thirds of its extent, the lower third being free and of more recent origin (3). The upper part of the clot had a tendency to lamination, so that the outer layer could be separated from the remaining portion; and at first sight this appeared to be the inner coat of the artery detached, but, on careful examination, it was found that the internal coat of the vessel was quite entire and normal in its position. The coats of the artery at the seat of obstruction were perfectly healthy. The canal of the femoral artery below the origin of the profunda was obliterated, being occupied by a firm clot of a whitish colour, streaked with red. The termination of the popliteal artery, as dissected from the stump, was also quite obstructed, and its canal filled with a clot of a beautiful red tint (6). The profunda branch of the femoral artery was impervious for the first half-inch from its origin, but after this its course became natural.

*Remarks.*—The principal point of practical interest in the history of this case is, the sudden attack of pain in the affected limb, followed by the instantaneous obstruction of the femoral artery, and the immediate death of the foot. These symptoms, taken in connexion with the supposed existence of heart-disease, caused some of the gentlemen who saw the case to consider it one of obstruction of the artery, depending upon the passing down from the heart of a fibrinous clot. The post-mortem appearances, as already detailed, certainly showed that the artery was obstructed from the impaction of a clot, but, owing to the absence of any disease of the heart, we must look for the origin of this clot elsewhere. The causes usually given for the spontaneous obstruction of the arteries of the extremities are,—

1. The rupture of one or more of the inner coats of the vessel.
2. The narrowing in the canal of the vessel, produced by atheromatous disease.
3. Arteritis and its effects.

The *first* of these is the only one which could have produced sudden symptoms, but that it was not the cause of the blocking-up of the artery in the present case was proved by the dissection, as all the coats of the vessel were perfectly entire; and I do not think that even this cause would effect such an immediate obstruction of an artery as in the case of the sudden impaction of a clot.

Dissection also proved that the *second* of these causes did not exist at the seat of the obstruction.

The *third* cause mentioned, namely, arteritis, is the one which is most frequently given to explain this condition.

Hodgson[1] thinks that in arteritis there is inflammation of the internal coat, with exudation of lymph from this membrane, and a consequent blocking-up of the artery.

Dr Thompson,[2] in an interesting paper on arteritis, says, "When healthy inflammation attacks the lining membrane of an artery, the result is an effusion of plastic lymph, which may accumulate to such an extent as to obliterate the canal of the vessel."

Crisp[3] says, "One of the more common effects of inflammation of the arteries of the extremities is coagulation of the blood, fibrinous deposits adherent to the sides of the vessel, producing obstruction and often entire obliteration of the tube."

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[1] Diseases of the Arteries and Veins, by Joseph Hodgson.

[2] Thompson on Arteritis, Dublin Jour. of Med. and Chem. Science, vol. xxx.

[3] Crisp on Diseases of the Bloodvessels, p. 33.

Sir B. Brodie also thought that spontaneous obstruction of arteries depended on arteritis, and, in his Lectures,[1] relates the history of a lady whose case exactly resembles the one I have described. Sir B. Brodie divided the tibia and fibula at the line of demarcation, and the patient made a good recovery. There was no opportunity in this case of ascertaining the exact cause of the disease, and Sir B. Brodie refers it to the effects of arteritis.

Tiedemann,[2] in his work on the Closure of Arteries in Disease, has collected many cases of gangrene, and refers them to four heads :— 1. Those depending on inflammation of the inner coat of the artery; 2. Those depending on the growth and morbid excrescences of the inner coat; 3. On deposits of solid or earthy concretions, or purulent matter between the coats; 4. On clots of blood, which, like plugs, close the sides of the arteries.

There is no doubt that arteritis and the other causes mentioned may produce gangrene of the extremities, but they are not sufficient to explain many of those sudden cases of spontaneous gangrene of the limbs, like the one just recorded, which are by no means uncommon.

Professor Virchow,[3] in 1847, called attention to the fact, that fibrinous clots formed in the heart might become detached, and, passing along the circulation, become impacted in some vessel or vessels, and so give rise to gangrene.

Dr Kirkes,[4] in 1852, wrote a valuable paper on this subject, and relates several cases in which the cerebral arteries became thus obstructed.

Professor Simpson, in 1854, read before this Society a paper on Puerperal Arterial Obstruction and Inflammation. In the same paper, afterwards published in his Obstetric Works,[5] he gives an account of many interesting cases of gangrene of the extremities depending on this and other causes.

Dr H. Wallman, in his "Contributions to the Doctrine of Embolism," relates several cases of this disease, one of which resembled the case under consideration as regards the clot found in the aorta. It was the case of a woman æt. 66, who died of pneumonia. At the post-mortem examination, irregular vegetations were found on the aortic valves of the heart, and there was atheromatous degeneration of the descending aorta, iliac, and carotid arteries. About half an inch above the bifurcation of the abdominal aorta, and somewhat at its posterior part, there was seated on the left side of the internal wall, on an atheromatous patch, a firmly-adhering clot, which enclosed a small cavity filled with white blood-corpuscles.[1]

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[1] Brodie's Lectures on Pathology and Surgery, p. 353.

[2] Tiedemann on the Closure of Arteries in Disease.

[3] Virchow's Archiv, vol. i.

[4] Med.-Chir. Trans., vol. xxxv. p. 281.

[5] Simpson's Obstetric Works, edited by Priestley and Storrer, p. 34.

Dr Goodfellow relates two cases of gangrene of the extremities depending upon obstruction of the arteries, caused by separated cardiac vegetations. In both cases extensive vegetations were found to exist on the valves of the heart.[2]

Since then cases of this disease have been recorded by various authors.

The case related appears to have been one depending upon the sudden impaction of a clot in the femoral artery; and I think the appearances found on dissection, namely, the absence of all heart-disease,—the healthy state of the upper portion of the aorta and of the femoral artery at the seat of obstruction,—the diseased condition of the lining membrane at the lower part of the aorta, and the presence there of two firmly-adhering clots, not of recent origin,—warrant us in coming to the conclusion that the clot, in this instance, did not proceed from the heart, but was formed on the diseased portion of the aorta.

In proof of this I would refer to the following conclusions, which Professor Virchow has come to, from his experiments on the subject:—1. An exudation on the free surface of the inner coat of the arteries was never met with. 2. Necrosis of the arterial coats occasions a coagulation of the blood in the necrosed portion. 3. Every chemical or mechanical irritation applied to the inner or outer surface of the vessels occasions inflammatory appearances only in the outer and middle layers of the vascular coats; any changes of the inner coat are only of a secondary and passive description. 4. The acute inflammatory appearances of the outer and middle coats of the arteries are perfectly analogous to the ordinary appearances of parenchymatous inflammation.[3]

Mr H. Lee, in a paper on Senile Gangrene, says:—"Fibrinous deposits not unfrequently form in diseased arteries, and may here as elsewhere undergo the process of softening. The products of this softening, together with the *débris* of the internal coat of the artery and the softened atheromatous deposit, are carried along the course of the circulation, until arrested in the smaller tubes, or in the actual substance of organs."[4]

Mr Lister, in his interesting experiments in regard to the coagulation of the blood, has shown that, as long as the coats of the vessel remain healthy, coagulation is prevented; but when anything happens to impair the vitality of the coats of the vessels, coagulation takes place.[5]

Professor Virchow, also, says:—"The coagula connected with the wall of the vessel are produced by local retardations of the stream of blood, or through changes of the surfaces which are in contact with the stream of blood."[1]

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[1] Virchow's Archiv, vol. xiii. p. 550.

[2] Med.-Chir. Trans., vol. xlv.

[3] Loc. cit., p. 272.

[4] Brit. and For. Med.-Chir. Review, vol. xx.

[5] See paper in Edinburgh Medical Journal for 1858.

Dr Richardson believes that there are three conditions under which the fibrin may be separated from the blood:—1. Where the amount of fibrin is relatively increased. 2. Where the fibrin is present in the blood in quantities abnormally great. 3. Where some obstruction is thrown in the course of the circulating current, and interferes with or impedes the stream.[2]

Dupuytren thought that coagulation of the blood in arteritis might arise either by contact of the blood with the inflamed membrane, or by mixing with coagulable lymph, or by ulceration of the artery.[3]

The discovery of a clot adhering to the artery in the present case and the observations of all these authors prove that deposits of fibrin may take place on an atheromatous patch in the coats of an artery, or in any other diseased condition of the vessel, and it is not unreasonable to suppose that these deposits may become detached from their place of origin, and, passing down, cause obstruction of some vessel or vessels. It may be asked, Why should an obstruction confined to the femoral artery at the groin, the veins being unaffected, cause immediate gangrene of the limb, when a ligature placed round the external or even the common iliac artery for an aneurismal tumour very rarely produces immediate death of the corresponding extremity?

The circumstances are different in the two cases: in the one, the blocking up of the vessel is so instantaneous that the anastomosing circulation has not time to come into operation; whilst in the other, the presence of an aneurism, by causing a gradual obstruction in the flow of blood through the affected artery, directs the circulation into the anastomosing branches, and so preparation is made for a complete stoppage in the main vessel.

In cases where the inferior extremity is affected, much will depend upon the portion of the femoral artery which is obstructed; if the impaction of the clot should take place below the origin of the profunda, the anastomosing circulation through this branch will probably be sufficient to nourish the parts below. A few days ago, I heard of the case of a gentleman who was suddenly seized with a pain in the leg, which was followed by obstruction of the femoral artery below the origin of the profunda: he had numbness and coldness in the affected limb for some time, but gradually recovered, without any gangrene taking place. In the case just related, however, the advanced age of the patient and the diseased state of the arterial coats would also assist in preventing the circulation accommodating itself so readily to the new circumstances induced by the sudden blocking up of the main artery.

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[1] Loc. cit.

[2] The Coagulation of the Blood, by Dr Richardson, p. 64.

[3] Dupuytren on Lesions of the Vascular System.

*Treatment.*—In regard to the treatment of this case, I may mention that Mr Syme delayed operating for the following reasons :—

1. The absence of all pulsation in the femoral artery at the groin.
2. The presence of a large slough in the thigh, showing the weak state of the circulation even high up in the limb.

In conclusion, I would notice the advantages derived in this case from the use of Condy's fluid, not only as a disinfectant, but also as a lotion applied to the sores. Cloths dipped in this fluid were kept applied to the stump and ulcer on the thigh, and seemed to have a most beneficial effect in causing cicatrization.

## ARTICLE IV.—*Remarks on the recent Trial for Murder of Alexander Milne.* By D. YELLOWLEES, M.D., Assistant Physician, Royal Edinburgh Asylum.

THE legal doctrine of criminal responsibility, as it was laid down by the judge when charging the jury in this trial, is simple and definite: every person who knows the nature of the action he performs, and can understand its consequences, is responsible for that action. This inexorable doctrine admits no degrees of responsibility, and but one ground of exemption from it, namely, insanity.

The question of criminality, therefore, frequently involves a prior question as to the existence of insanity; if found insane, a prisoner is legally quite irresponsible; if not found insane, he is responsible like other men.

My first remark is, that this absolute mathematical line which the law would draw between sanity and insanity, between perfect health and fully developed disease, is an impossible one. Such an absolute division exists nowhere in nature, and certainly nowhere in medicine. Disease is essentially a gradual thing, both in its progress and its effects. No one ever thinks of drawing such a line in any other disease. In consumption, for example, we find all the successive stages from the scarcely altered respiratory murmur, and the flush which enhances the beauty of perfect health, to the sunken eyes and hurried breathing which tell that death is near. There is no line, on the one side of which we find absolute health, and on the other complete disease. This is true of all diseases, yet the law endeavours, in the case of insanity,—the subtlest and often most insidious of them all,—to draw this impossible line.

Again, to continue the illustration, the detection of the earliest traces of lung-disease, even in apparent health, is a matter of the greatest delicacy and difficulty, and one on which the most skilful physicians might easily form different opinions, according to the state of the patient when the lung was examined. The existence of brain-disease in its earlier stages is probably the most difficult question the physician ever attempts to determine; but what a clamour is immediately raised by the public and by the press if doctors should differ in regard to it!

And yet, again, it would not be true to say that the function of the lungs was destroyed, because the physician had detected proofs of consumption. Neither is it right to suppose that the function of the brain is destroyed, because the physician has detected proofs of insanity. As the progress of disease is gradual, so its effects are necessarily gradually developed. Hence it is that a large proportion of the insane can think and reason correctly, are conscious of right and wrong, and understand fully the nature and consequences of their actions; but these are the legal tests of responsibility, and a large proportion of the insane ought therefore to be responsible. So, we believe, they are. They are not, indeed, fully responsible like other men, because, along with comparatively unimpaired intellects, these patients have perverted emotions which instigate, or weakened wills which cannot always prevent, conduct which they often subsequently disapprove and bitterly regret; but yet they are so far responsible as not to be guiltless, if that conduct be contrary to law.

If the majority of the insane were not to a certain extent responsible, conscious of right and wrong, and amenable to moral government, how could any of our public asylums be conducted with a staff of attendants which does not number more than a twelfth or a fifteenth part of its population? The patients, too, are conscious of responsibility, and this consciousness is a chief agent in their management. Its existence is proved not only by the regret often expressed for misconduct, when, as they say, they "forgot themselves," but also by the frequency with which violent behaviour is justified, or by the ready cunning with which others are blamed. It is further shown by the universal intolerance of mere vice or depravity in a fellow-patient, which is not more remarkable than the patience and sympathy usually exhibited by the others towards one whose noise and violence are obviously the result of disease.

It would be easy, but it is unnecessary, to illustrate all these traits of asylum life. One of them may be noticed more particularly. That an insane person should perform an action under the influence of insanity, and then deny or explain it away through fear of blame, was regarded in the recent trial of Milne for murder as unprecedented or impossible. Happily murders are not frequently committed by the insane, and very seldom by an insane person whose sense of responsibility would lead him to invent such an explanation; but Dr Smith was certainly correct in stating that, in minor or less rare offences, such false excuses are frequently made. It is easy to illustrate this in the wards of any large asylum. One of the worst and most dangerous patients in Morningside Asylum is a perfect adept at excusing her own misdeeds, or throwing the blame on others who are perfectly innocent. If, for example, she strikes a fellow-patient, and thinks the attendant has not been looking at the moment, she says, sympathizingly, that "the poor woman fell down in a fit and hurt herself," or unblushingly accuses some other patient of having done it. Another patient steals every article, useful or useless, that he can lay hands on, and can hide about his person, such as caps, spoons, stockings, handkerchiefs, etc. etc., and when anything goes amissing in his ward, every one knows where it will be found; yet he always denies he has taken anything, and when the articles are found secreted about his person, he says some one must have put them there,—that the people in the laundry must have left them there when his trousers were washed, or, alluding to a boy who is in the same ward, "it maun hae been yon laddie; he's a wild mischeevous deevil!"

The degree of criminal responsibility among the insane varies in different cases according to the mental condition of the patient, and imperfect responsibility may extenuate crime without necessarily requiring exemption from punishment; where the insanity amounts only to an extenuation, its cause, and the circumstances in which the particular act was committed, may justly influence the sentence.

The non-recognition of the partial responsibility of the insane has led to strange inconsistencies in the administration of justice where the plea of insanity has been urged. The law has laboured so to define insanity as to make it correspond exactly and justly with the total irresponsibility which it legally involves, but is continually, most justly, violating its own definition; for the "total alienation of reason" which it demands is almost unknown, and was never seen at the bar of a court of justice.

The ignoring of this principle is the source, too, of the distrust and doubt—to use the mildest terms—with which medical testimony is so often received in cases of insanity, both by the legal profession and by the public. Counsel refuse to believe scientific truth unless it corresponds with erroneous legal definitions, and fearlessly pronounce opinions on questions beyond the range of their experience, and of which they are necessarily ignorant. The public, knowing that insanity legally confers complete irresponsibility, and recognising crime as an offence against itself, readily follows a bad example, thinks itself perfectly capable of determining the existence of insanity, and declares that doctors—especially asylum doctors—are not to be listened to, for they regard all criminals as insane, and, ignoring justice, are anxious only to "get them off."

Another consequence of the non-recognition of the partial responsibility of the insane is seen in the extraordinary explanations which are given of the behaviour of those insane criminals who are unfortunately held to be sane by these self-approved judges. The exalted christian charity manifested in these explanations towards the weaknesses of poor humanity is very remarkable, but not more so than the singular ingenuity with which the suspicions, excitement, and delusions of insanity are softened down into mere eccentricities.

The recent trial of Milne for murder has afforded a perfect illustration of these statements, and the remarks on this trial in the last number of the Edinburgh Medical Journal furnish an excellent example of the way in which legal ingenuity and ability are often sorely taxed to explain, on the supposition of sanity, behaviour which only an insane man could have exhibited. As its legal aspects have been so well presented by Mr Cowan, we propose to look very cursorily, and by way of contrast, at the medical side of the question.

Milne was stated to have been a respectable and well-disposed man until he became bankrupt in 1860. After that event there was a marked change upon him; he used to say he had been "crushed," his manner became dull and reserved, and he sometimes did very extraordinary things, such as presenting a revolver at his friend, and brandishing a carving-knife so as to alarm every one near him, and require the interference of the police, but these were ascribed apparently to the influence of drink and to his habitually tragic and theatrical manner.

On Christmas 1862, he had a supper party, at which his behaviour was wild and extraordinary, and greatly alarmed his guests; with an old sword in his hand he marshalled them like soldiers, made them march to supper to the tune of the "Merry Masons," and then said grace in a strange and incoherent manner. "The company were alarmed at his conduct," said the witness Moodie, "and his looks were very peculiar all night; he made contortions with his face, just like a madman." His friends became now convinced for the first time that intemperance alone could not explain his behaviour, for although he had been drinking a little, yet the witnesses agreed in saying that his conduct on this occasion was very different from that of a man excited merely by drink. Paterson the murdered man was one of the guests, and next day he remarked about Milne, "He's a strange man that; he looks dangerous."

On the 26th and 29th December, and on the 3d and 5th January, Paterson was proved to have been calling, with most friendly frequency, at Milne's house; and on the 3d, Milne had actually said to one of the witnesses that there was "no one he thought more of than Paterson." During this period Milne was in a state of great nervous excitement and agitation, aggravated, as well as primarily induced, by intemperance; his intemperance did not consist in bouts of drinking with intervals of sobriety and remorse, for he was very seldom quite drunk, but was of the gradual and most pernicious kind, so that he seems to have been constantly, although very slightly, under the influence of intoxicants.

On the 5th January, this intemperance and the nervous disturbance induced by it produced at length their frequent result—delusions. Milne sent for the doctor, under the conviction that poison had been put into the water; he made the statement to that effect calmly and intelligently; he was not in delirium tremens, and the doctor saw no appearance of insanity. When his daughter brought the medicine from the chemist's shop, soon after the doctor's visit, his friend Paterson was sitting with him and smelt without opening the bottle she had brought. No sooner did Paterson leave than Milne declared that he has poisoned the medicine, and another bottle must be brought from another chemist before he would taste it. On the same day, he declared that Paterson had poisoned both himself and his daughter with quicksilver. To the porter who was shutting his shop he manifested other delusions; he came to the door bareheaded and said, "Whisht, don't you see the robbers? they're planning to break into the shop." He looked wild and strange, and seemed to see the robbers in the street. He also told the porter that they were trying to poison his wife, his family, and himself, and bade him "watch them." When the porter came in the morning to open the shop, there was a great fumbling inside, and Mr and Mrs Milne were at the door when it was opened. Milne was only partially dressed and said, "They've been here, but they've not got in." There had been a noise at the door of his shop about nine o'clock on the previous evening, which he declared was caused by the robbers, and he went into the front shop shouting "thieves." This noise caused no uneasiness to any one else who heard it, and Milne's standing partially dressed behind the door to keep out the robbers is most characteristic of his mental condition.

On this day, January 6th, the day preceding the murder, his insanity becomes yet more evident. He ordered from a blacksmith a strong iron bar for the door of his shop lest the thieves should return; when Paterson chanced to call while the bar was being measured, Milne forbade him to enter his door, and said to the blacksmith, "I am suspicious of him and another." He said again, subsequently, "I am suspicious, and" (pointing upwards as he spoke) "there is a spirit I use which enables me to see what other people can't." When the blacksmith mentioned Milne's suspicions to Paterson, his explanation of them was, "The man has been in delirium tremens since Christmas." On the same day, Milne repeated the delusions about robbers to one of his neighbours, who had heard the noise of the previous night, and had thought nothing of it. When she asked him why he thought his shop was to be broken into, he answered, striking his hand against his forehead, "The spirit told me so." On this day also, Milne placed the most valuable part of his stock in Smith and Philpot's safe, because the robbers had been at his door the night before, and they were coming back again, and he knew he would be both robbed and murdered. The witnesses who saw him there were quite satisfied that these statements were delusions, and tried in vain to reason him out of them.

Next morning, within an hour of the murder, he went again to the auctioneers' rooms and told Mr George Smith that his wife and also a man had been attempting to poison him all the night before, and that he had caught the man shaking a powder into a tumbler of water which he was about to drink; he also left a message for Mr John Smith, asking him to come over and value the rest of his stock, as he wished to dispose of it.

The most important evidence of Mr John Smith is by some strange oversight entirely omitted in the report of the trial given in the last number of this Journal. It is of the greatest value, as showing Milne's condition and behaviour before the perpetration of the deed. As the report of the trial is very imperfect without it, I transcribe the evidence from the *Scotsman* of 11th February :—

> *John Smith*, examined by Mr Thomson—I remember Milne coming to our saloon a little after twelve on the 6th January. He had a pretty large parcel with him. He said that this was a part of his stock that he wished us to lock up in our safe. He wished me to put it in the safe, and keep it for a day or two. He said that robbers had been at his shop-door during the night previous, wishing to break in. I said I thought that was a strange thing in Edinburgh, and I asked him why he did not cry to the police for protection. He said he could not get out, because if he had come out he would have been killed. He said he knew they would be back that night again, and he was going away to get a smith to get the shop-door strongly barred with iron. He said that he knew he would be both robbed and murdered. I tried to persuade him past the idea of it, but I did not succeed. I thought there was something very strange in his appearance. He was excited, and the look of his eyes was strange. I was under the impression that he was in *delirium tremens*, or something similar. His whole conduct led me to infer that he was wrong in his mind. On the Wednesday morning I got to our place of business about eleven o'clock. There was a statement made to me by one of our clerks, in consequence of which I gave a direction to the clerks and apprentices as to their conduct in reference to Milne when they saw him. I got a message from the prisoner from my son, who is one of our clerks. The message was that he had been there inquiring for me, and that he wished me to go over to his shop. *I did not go, because I did not consider it was safe to go near him. Cross-examined*—I have known Milne five or six years by sight. I can scarcely say he has been a sober man during that time. I have seen him several times intoxicated, and I should say he was a man addicted to drinking. He had at all times a peculiar, excited look about him, drunk or sober, since ever I remember. I thought he was under the influence of some excitement on the 6th January, and I thought drink was the cause of a good portion of it. He looked very knocked-up like—more weak and feeble—just like a man who had not been in bed for two or three nights. He was worse than I had seen him any time previous. I could not say that Milne had been drinking on the 6th January. He had not the appearance of having been drinking recently. He had the articles he brought with him rolled up in parcels neatly enough. I don't think drinking would account for the absurdity of his conversation that morning.

On the same morning, Milne bought the dagger, and, within an hour and a half after the purchase, Paterson, who called in obedience to Milne's message, was stabbed to the heart with it.

Such are the facts up to the time of the murder. We believe that every medical man will regard them as conclusively proving that Milne was of unsound mind; and yet these are the facts which

Mr Cowan speaks of in the following terms:—"The general import of the evidence as to the conduct of the prisoner prior to the act is, that there was no undue excitement about him—that he was quiet and self-possessed, acting in the way in which a sane man usually does." This amazing conclusion will be found at page 840 of the March number of this Journal; how Mr Cowan can be of this opinion, even supposing him not to have heard, as he has not reported, Mr John Smith's evidence, I am at a loss to understand.

The examination of the facts on which this startling conclusion is based begins by the very misleading statement that Milne had *two* delusions. It is true that his delusions refer to two subjects, but did he not believe that the water had been poisoned; that Paterson had poisoned his medicine by smelling at the unopened bottle; that Paterson had poisoned his daughter and himself with quicksilver; that he had seen the robbers in the street planning to break into his shop; that Paterson was one of the robbers; that the spirit he used had told him so; that they had come to the shop but had not got in; that they were coming back again; and that he would be robbed and murdered? And did he not prove how real these delusions were to him by sending for the doctor, by getting the iron bar, and by removing the most valuable part of his stock?

But all this resolves itself to the legal mind into two delusions. Let us see how these two delusions are explained, in order to justify the conclusion of Mr Cowan. To the delusions about the robbing of the shop, Dr Christison is said to attach "little importance," as a noise had been actually heard at the door, which, however, no one else thought of any consequence.

The evidence of the porter presents more difficulty, but it is first slighted as being unsupported, and then is thus disposed of (p. 838):—"This evidence standing thus by itself is readily explained, when we consider, *first*, the suspicions which he seems to have taken up that an attack was to be made on his shop; and, *second*, the strange theatrical manner of the man, which led him to invest with lifelike reality the unreal fancies of his brain. The description which he gave Laing of the robbers on the Monday was mere acting; and while it seemed to Laing to be a serious performance, to Milne it was but a stage rehearsal." Truly there are few people insane if this explanation be sufficient; for example, we know a woman whose insanity has taken the form of pride instead of suspicion, who thinks she is a lady of rank, and whose aristocratic airs and pretensions make her both dangerous and troublesome; but all this is readily explained—she merely "seems to have taken up" the impression that she is a more influential and important person than she actually is, and in her manner and bearing tries to act the lady.

The proof that Milne was not merely "acting," but that he really believed what he said about robbers, is furnished unequivocally by his removing part of his stock, and placing it in the safe. This proceeding Mr Cowan characterizes by a delicate euphuism as "perhaps somewhat of overcautiousness," and ingeniously regards it as the consequence simply of the noise at the shop-door.

The delusions about poisoning are thus explained:—"That Milne refused to take medicine out of a bottle which Paterson had touched, saying that Paterson had poisoned it; that he complained of Paterson trying to poison him, to get his business and his wife, is true, but it does not follow that these were delusions. An unfounded suspicion is not a delusion." To wound is not to kill, is a statement precisely analogous to that contained in the last sentence, and equally inconclusive. It depends on the nature and extent of a wound whether it amounts to killing or not; and, similarly, it depends on the nature and extent of an unfounded suspicion whether or not it amounts to insanity. A person is not insane who, on a winter night, suspects and avoids a man whom he sees at a dark corner of the road, and whom he discovers in a little to be the policeman; but if a man suspects, like a patient I know, that Government has long been employing agents to watch all his movements, that Lord Palmerston especially has some design against him, and has for some political reason placed him in an asylum, will any one say that these unfounded suspicions do not constitute insanity? Milne entertained the suspicion that his most intimate friend was attempting to poison him and his family, was planning and attempting to break into his shop, and was certain on the following night to rob and murder him. These suspicions were without the slightest foundation, and yet we are told that there was no insanity about him—that he was "acting in the way which a sane man usually does."

The warning given to Paterson by his landlady scarcely seems to deserve the importance attached to it by Mr Cowan; it seems, on the other hand, rather to corroborate the idea of Milne's insanity, for she at least thought him very excitable and dangerous, if not actually insane.

The purchase of the dagger was quite consistent with the other precautions he took against the robbery and murder which he believed were to be perpetrated that very night, and the deed was done, we cannot doubt, under the influence of these delusions, probably after he had declared his suspicions and Paterson had indignantly denied them.

His calmness after the deed, his not attempting to escape, and his justification of the act on the ground of these erroneous beliefs, are all strongly corroborative of insanity. Mr Cowan's ingenuity makes him give four explanations of the deed, but there were substantially only two,—that Paterson's conduct deserved and justified it, and, subsequently, that it was entirely accidental.

The hesitation with which these delusions were expressed at the first examination is very frequent when a patient is expressing beliefs which he knows other people may not share, and is very far from proving that he was feigning the delusions. The subsequent explanation of it as an accident was made on the following day, after he had time to think over what had happened, and, as I have already shown, is not inconsistent with the existence of insanity. It is, however, *entirely inconsistent* with the idea that the insanity was *feigned.* This is a very important point. Had a sane man deliberately murdered his most intimate friend, and then feigned insanity to escape punishment, he would never have been such a fool as, at the same time, elaborately to explain that the deed was an accident. Milne's behaviour does not show the deceitful cunning of an impostor, but he feels that his beliefs will not justify his deeds in the eyes of other men, and that he needs to invent some other explanation.

There is no sufficient reason for believing that any of the delusions expressed in prison were feigned, although it is quite possible, nor does even that disprove insanity. I have known an insane patient to feign belief in her delusions which she had long ceased to entertain.

To alienists Milne's insanity is of a familiar type,—that, viz., which may be called, from its cause, the insanity of intemperance. Its most prominent features are hallucinations of the special senses, with liability to sudden and impulsive outbursts of violent temper or noisy rage. Every asylum contains illustrations of this form of mental disease.

As to the verdict, I think it was wrong and not according to evidence; I am inclined to believe that it was arrived at from a feeling that it would be unjust that such a man should escape punishment rather than from a conviction that his mind was unimpaired. Had a verdict of insanity been given, the jury and the public too would have felt that a criminal worthy of punishment had escaped, and that, by default of justice to the individual, injustice had been done to society. This desire for practical justice is the source, too, of the ingenious and far-fetched explanations which are given of the conduct of insane criminals, for insanity implies acquittal, and insanity must, therefore, never be admitted unless where *it ought to exculpate.* This is surely a very circuitous and uncertain endeavour after justice, for the prisoner, supposing him to be insane, but partially responsible, will get either too much or too little of it,—either complete exemption from punishment, or the same sentence as an ordinary offender.

It seems to us, but we would speak with deference on this legal point, that culpable homicide would be the righteous verdict in such cases, allowing the judge to determine the form and degree of punishment according to the degree of responsibility and the other circumstances of the case. The difficulty can never be solved until the doctrine of partial responsibility be recognised.

Milne should never have been in danger of capital punishment; but perpetual imprisonment seems both a righteous award to him and a most needful precaution for society. If further developments of insanity should render the prison an unfit place for his detention, the law provides for his transference to an asylum as a criminal lunatic.

In conclusion, we find in this case another proof of the extremely unsatisfactory way in which all medical questions are at present decided in courts of justice. Counsel necessarily know very little about the tests for poisons or the forms of mania, just as doctors know nothing about the forms and processes of law, and the jury are in absolute darkness, or, yet worse, know just enough to be unaware of their ignorance. Yet counsel must cram from the works of Taylor or Christison, and must at least pretend to know, while the jury must be made to know, or at least to think they know, for it is by them the question must be determined. We believe such questions will never be satisfactorily determined until they are referred by the Court to a council of experts, disinterested, unbiassed, and appointed by the Court, whose reports shall be held as final in reference to the scientific question involved. This measure is ably advocated by a writer in the *Cornhill Magazine* for March 1863, especially in reference to cases of poisoning; and it is, if possible, still more necessary in cases of insanity.

## ARTICLE V.—*Case of Retroversion of the Uterus, causing Retention of Urine during Pregnancy.* By JOHN DICKIE, M.D., L.R.C.S.E., Alloa.

THE following case occurred in my practice some time ago, and as it is a characteristic example of retroversion of the uterus in a first pregnancy, it may not be uninteresting to some of the readers of the Journal.

On the 24th April 1862, I was consulted by a young unmarried woman from the country, æt. 22, about a difficulty she had experienced for some time in passing urine. She had a constant desire to micturate, and after much straining she could pass only a very little at a time, and that with a great amount of pain. She had no other complaint. On inquiry, however, I found that she had ceased to menstruate for three months, and that her stomach had been rather irritable for some weeks. Suspecting some uterine affection, I proposed a vaginal examination; but she objected to this for the present; and as she believed cold damp feet had given rise to the attack, she was anxious to try the effect of some medicine in the first place. I therefore recommended her to foment the lower portion of the abdomen with flannel wrung out of boiling water, and to take a mixture containing some tincture of hyoscyamus and spirit of nitrous ether; but cautioned her about the danger of neglecting her complaint, and advised her, if no improvement took place, to let me know in a day or two.

A week afterwards, I was summoned to visit her at her home, being informed that she had passed no water for two days, and was in great distress.

On arriving at her house, I found her in bed, her abdomen distended to an enormous extent, and exquisitely painful to the touch. She suffered from constant vomiting, general fever, and restlessness; the bowels had not been moved for three days, and a distressing downbearing feeling was complained of. On examination per vaginam, I found that canal completely blocked up by a large round body, which filled the hollow of the sacrum, and prevented the finger from passing backwards. This was evidently the fundus of an enlarged uterus. On introducing the finger in the direction it would go, it passed upwards and forwards to behind the os pubis, where I thought I could detect the os uteri. I then introduced an elastic male catheter into the bladder, and drew off a large wash-hand-basinful of ammoniacal urine, to the patient's inexpressible relief.

Having diagnosed the case as one of retroversion of the uterus, I now proceeded to effect reduction. With my two fingers in the vagina, I found I could exert very little pressure on the fundus, and after persevering in this way for some time, I found reduction impracticable; I therefore placed the patient on her knees and elbows, and inserting my two forefingers into the rectum, I pressed downwards, and at the same time endeavoured to give the fundus a lateral inclination, so that it might escape the promontory of the sacrum. My efforts succeeded in a short time in replacing the fundus in its natural position. On examining the os uteri I found it projecting backwards, proving that reduction was complete. I then administered an opiate, and gave the patient strict injunctions not to leave her bed for a fortnight.

Before leaving the house, I was anxious to discover the cause of this accident. The patient denied that she was with child, and could give no other explanation but that of cold damp feet; however, after much cross-questioning, I elicited from the patient the confession that she believed herself pregnant, and that, for some weeks previous to her present attack, she had been attempting to procure a miscarriage by taking large doses of croton oil. This had produced severe and protracted hypercatharsis, and I have no doubt was the principal cause of the retroversion. The desired abortion was not obtained; and although, after the rough handling the womb had sustained, I could scarcely have anticipated such a result, there was no return of retention, and she carried her child to the full term. I was engaged to attend her during her confinement, but being in attendance upon another case when sent for, she had to call in the assistance of a midwife, who waited a day and a night upon the case, when I was sent for again. On examination, I found the passages hot, dry, and swollen; the head presenting; pulse 130; abdomen swollen and tender; an elastic catheter was introduced, and a large quantity of urine drawn off. I then waited for some time expecting the pains to regain their strength and expel the child; but this not taking place, and the mother getting exhausted, I delivered with the forceps. The child was apparently dead; but by bleeding it at the cord, and thereafter resorting to Dr Marshall Hall's "ready method," I had the satisfaction, after five minutes' perseverance, of hearing it begin to gasp and cry,—much to the astonishment of all present.

The placenta came away in a few minutes, the womb contracted well, and an hour afterwards the mother expressed herself as feeling very comfortable; she continued to improve, and made a good recovery; at the present time,—three months since the confinement,—mother and child enjoy good health.

The above case is interesting from the fact of its occurring in a primiparous subject, retroversion being an exceedingly rare occurrence in first pregnancies. It is also interesting from its cause: doubts have been expressed as to whether retroversion is the cause of retention or *vice versa*. The latter opinion was held by Denman and Blundel, and has many supporters; but from the above case, and from others I have seen in multiparous subjects, I incline to the former opinion, that the retention is the effect and not the cause.

Croton oil, as already stated, had been repeatedly taken; so that the exciting cause was evidently the continued tenesmus forcing the contents of the abdomen down upon the fundus. The predisposing cause was the enlargement of the uterus: the tenesmus most likely produced a prolapse of the posterior wall of the vagina, and this affecting the perpendicularity of the uterus, a descent was begun which continued until the fundus got below the promontory of the sacrum, and then complete retention took place from the mechanical pressure of the os uteri on the neck of the bladder.

Another point in the case worth noting is the attempt on the part of the patient to conceal her pregnancy. Had I not been on my guard, a serious mistake might have been committed; had the womb continued much longer retroverted reduction of it would have been impossible, and the case might have terminated fatally; hence the importance of the rule, in all cases of retention of urine during the early months of pregnancy, to make a vaginal examination to ascertain the position of the uterus.

As to the treatment, nothing could have been more satisfactory; and I would take this opportunity of strongly recommending the prone position, such as I adopted during attempts at reduction; I found it to facilitate my efforts very much.

The subsequent retention during labour arose from pressure on the neck of the bladder by the child's head,—a frequent occurrence during protracted labour, but which should never be allowed to occur in a case attended from the beginning by a qualified practitioner.

## ARTICLE VI.—*Obstruction of the Air-passages a Cause of Death of the Child after Birth.* By DR L. BUHL.[1] Translated from the German by J. MATTHEWS DUNCAN, M.D., Lecturer on Midwifery.

AMONG twenty-seven children dying from mechanical causes, in labour or shortly after birth, eleven were destroyed by obstruction of the air-passages with foreign matters, or presented this condition.

Most of the children were born dead [eight in number], and of those which were alive at birth none survived the first day of life. One died twelve hours before birth.

Ten of the children were boys and five were girls. They had an average weight of 5·6 pounds.

In ten of the cases the obstruction was produced by a greenish or greenish-brown slimy mass, which often copiously filled the cavity of the larynx and of the trachea as far as its bifurcation, or even completely filled its branches, or it was in less quantity and permitted the passage of air and of fluids to an insignificant extent. In the former kind of cases, the lungs had their fœtal position and texture unaltered (six cases); in the latter class, more or less numerous small portions of lung were filled with air; and this, as might be expected, was particularly observed in the three children which survived birth some hours. Only in two who died during parturition could air be shown to be present in the lungs; but in only one of these two was the distention of the air-cells the result of instinctive respiratory movements; in the other, air had been artificially injected.

The parenchyma of the lungs, in the cases where they contained air, had a brownish-yellowish coloration, and from the surface of a section there could be expressed a similarly coloured finely spumous fluid.

Microscopical examination of the matters found in the air-passages always discovered the presence of yellow golden reddish nuclei of colouring matter of different sizes and irregular form, very frequently of tabulæ of cholesterine, fat drops, and cylindrical cells,—circumstances which left no doubt that the substance examined was meconium. The superficial inspection of the larger tracheal plugs, and their comparison with the contents of the colon, in the first instance, removed any doubt on the point, especially as the throat, mouth, and nose were filled with the same, and the whole body and umbilical cord were smeared with it, and had from it a greenish tint. This condition of the skin almost establishes the diagnosis of the cause of the death before the autopsy is made.

To complete the proof that the greenish coloration of the skin and of the contents of the air-passages is produced by meconium, it is only necessary to add that this ordinary content of the child's large bowel was found in its natural situation either not at all or only in the rectum.

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[1] Klinik der Geburtskunde. Von Dr C. Hecker und Dr L. Buhl. Leipzig, 1861.

It is not without interest to observe that, in cases where repeated attempts at respiration were made, particles of meconium were always found even in the parenchyma of the lungs, having been drawn as far as the air-cells themselves.

In the pleural covering there were found small hæmorrhages corresponding to the larger accumulations of these foreign matters; but lungs that remained in their entirely fœtal condition showed only punctiform hæmorrhages.

In two cases both cavities of the chest were filled with yellowish-red serum.

Considering the admirable researches of Hecker, Schwartz, and others, on premature respiratory movements, there cannot be a doubt that meconium reaches the air-passages only by inspiration, —an instinctive intra-uterine act; and it is supererogatory to make any further remarks on the subject.

In one case, the unfortunate inspiration was not during or before birth, and meconium was not the foreign body which demonstrated the respiratory imperfection. It occurred in a boy born with split palate, harelip, and other deformities in the same situation, to whom, seven hours after birth, the nurse wished to give some milk to drink to stop its crying. It died immediately. Larynx, trachea, and bronchi contained milk, and even in the parenchyma of the lungs milk was recognised by its fat drops; and to the parts containing these there corresponded hæmorrhagic spots in the pleural covering.

# Part Second.

## REVIEWS.

*A Treatise on the Continued Fevers of Great Britain.* By CHARLES MURCHISON, M.D., F.R.C.P., Senior Physician to the London Fever Hospital, etc. etc. London: Parker, Son, and Bourn: 1862.

*Lectures on the Distinctive Characters, Pathology, and Treatment of Continued Fevers, delivered at the Royal College of Physicians of London.* By ALEXANDER TWEEDIE, M.D., F.R.C.P., Consulting Physician to the London Fever Hospital, etc. etc. London: John Churchill: 1862.

VOLUMINOUS as is the literature of fever, we are always glad to see additions made to it by competent observers. To the pathologist, the subject is one of extreme interest, as lying at the very root of medicine, and embracing questions still involved in obscurity; while, to the practical physician, a disease which causes a large share of the mortality of the country, cannot but be regarded as in the highest degree important. Without farther preface we have to introduce to our readers the most recent works on this subject: the authors are London physicians, graduates of the University of Edinburgh, and each has derived his experience, partly in this city, but chiefly in the wards of the London Fever Hospital.

Dr Murchison's book is a complete treatise on the fevers of Great Britain. It has been prepared with great care, embodies the results of extensive research, contains the fruits of much observation and reflection, and is not only honourable to himself, but creditable to the medical literature of this country. The subject is too extensive to permit us to analyze the work completely, but we shall endeavour to lay before our readers the chief points in regard to which Dr Murchison may be regarded as an original investigator.

Dr Murchison's first chapter treats of the theory of fever, and classifies the various forms of the disease. It had long been recognised that an elevated temperature was an essential phenomenon of fever, but the cause of the increased heat was till lately involved in obscurity. Much light has been thrown on this subject by the researches of physiologists, especially of Claude Bernard. It has been shown that section of the sympathetic in the neck produces dilatation of the bloodvessels, and increased temperature of the side operated upon. The dilatation of the bloodvessels is the result of paralysis, due to the withdrawal of the nervous influence supplied by the sympathetic, more blood flows through the enlarged canals, more change of tissue in consequence takes place, and the result is an elevated temperature of the parts affected. In all probability the increased heat in fever is to be explained in the same way. Various phenomena, such as the preliminary rigors, the feeling of languor, the tendency to sickness, and the anorexia, seem to indicate that the fever-poison acts first upon the nervous system, especially upon the medulla oblongata, and leads to a partially paralyzed condition of the sympathetic, the vagus, and possibly of other nerves. One of the results of this paralysis is a dilatation of the bloodvessels supplied from these sources; this leads to a more abundant flow of blood, which determines a more rapid change of tissue, and hence occasions an elevation of temperature. That the tissues are destroyed more rapidly than natural is easily proved. Emaciation goes on far more rapidly than could be accounted for by the mere deprivation of food; the elimination of urea and uric acid (the measures of destruction of the nitrogenous tissues) is largely increased; and there is in general a direct relation between the temperature of the body and the amount of urea given off. The urea resulting from this destructive metamorphosis of the tissues may not, however, all be got rid off; it may be to a greater or less extent retained in the blood, and may be the cause of those typhoid symptoms which modern observers have recognised as essentially identical with those of uræmic poisoning. The pathology of fever, thus briefly stated, is in all probability correct; no other theory is so consistent with itself, or affords so satisfactory an explanation of the facts involved.

The classification of fevers adopted by Dr Murchison is that which has of late been received in this country: it is thus expressed in a tabular form :—

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| " A.—NON-SPECIFIC . | I. Simple Fever, caused by . . | | Exposure to sun, fatigue, surfeit, etc. |
| B.—SPECIFIC. | II. ENDEMIC (Pythogenic, Enteric, or Typhoid) . . . . . | | Poison contained in emanations from sewers, &c. |
| | III. & IV. EPIDEMIC | Typhus caused by . . | The concentrated exhalations from squalid human beings. |
| | | Relapsing Fever . | Famine." |

The second chapter of Dr Murchison's work treats of typhus fever. It commences with an able historical account of the disease, in which it is shown that, although some of the cases described by Hippocrates were probably examples of it, no unquestionable proof can be adduced that previous to the sixteenth century any of the epidemics which were the scourge of the Middle Ages, can be clearly identified as typhus. The descriptions, however, of Frascatorius of Verona, and Cardanus of Pavia, leave no doubt that the epidemics in their time were identical with the typhus of the present day; and Dr Murchison shows, in his very interesting sketch, that the circumstances under which the disease has occurred have been almost invariably the same; that it has generally corresponded with periods of scarcity or want; that it has usually been associated with crowding together of the population; and that, although the disease has varied in severity at different times, there is no evidence of any change in its type or its essential characters.

The etiology of typhus, both theoretically and practically, is a subject of much interest, and is very ably treated by Dr Murchison. It is first shown that the predisposing causes are such as produce a depressing effect upon the system, and particularly such as lead to defective nutrition; and the exciting causes of the disease are then considered. That typhus fever is contagious, has been for long generally admitted by the profession; in fact, no one who has had even moderate experience of the disease can entertain any doubt upon the subject. But while this is the case, there has been much difference of opinion with regard to the mode of generation of the poison. Some, as the late Dr Alison, considered that the poison was specific in the fullest sense; that the disease, like small-pox, was never generated *de novo*, but that it was always communicated directly or indirectly from the sick to the healthy. Others, while admitting the contagious characters of typhus, have maintained that the virus was non-specific, but that it was generated under various unfavourable hygienic influences, especially starvation and vitiated conditions of the atmosphere. Dr Murchison's conclusion is, that although the disease is in general propagated by contagion, it may be produced *de novo*, when a large number of human beings are crowded into a small unventilated space, the ammoniacal exhalations being increased and concentrated, and generating by their putrefaction the poison of typhus. Numerous facts, some observed by himself, others recorded by previous observers, are adduced by Dr Murchison in support of this doctrine, and it is shown that the disease often arises in our great cities independent either of importation or contagion. But lest such cases should be objected to as admitting of another explanation, our author strengthens his position by a reference to circumstances where typhus has shown itself in its most virulent form, and where its origin by contagion was impossible, as in the case of the fevers which have devastated our jails, our armies, and our fleets. The section devoted to this subject is altogether one of great interest, and Dr Murchison has brought forward an amount of evidence in support of his conclusions, which warrants, or indeed compels us to accept them. These conclusions are the following:—

"1. Typhus is due to a specific poison.

"2. This poison is communicated from the sick to the healthy, through the atmosphere or by fomites, but is rendered inert by free ventilation.

"3. The poison is also generated *de novo*, by overcrowding and bad ventilation.

"4. The great predisposing cause of typhus is defective nutrition."

The next section, on the symptoms of typhus, is very fully and carefully worked out. Dr Murchison first gives a clinical account of the disease, and then considers the more important symptoms in detail. We extract the following excellent description of the typhus eruption:—

"Between the fourth and the seventh days, usually on the fourth or fifth, an eruption makes its appearance on the skin. It is composed of numerous spots of irregular form, varying in diameter from three or four lines to a mere speck, which are either isolated or grouped together in patches presenting a serpigenous or very irregular outline, and often closely resembling the eruption of measles. At first, these spots are of a dirty-pink or florid colour, and very slightly elevated above the skin, and they disappear upon pressure; but, after the first or second day, they usually become darker and more dingy, they resemble reddish-brown stains, are no longer elevated above the skin, and do not disappear, but only become a little paler, on pressure. They have no defined margin, but merge insensibly into the colour of the surrounding skin. These spots usually come out first over the abdomen, and thence they spread to the chest, back, shoulders, thighs, and arms; in some cases they are first seen on the backs of the hands; they are most common on the trunk and arms, and are rarely observed on the neck or face. Along with these superficial spots, there are others which are paler, less distinct, and which, from their apparent situation, beneath the cuticle, have been designated 'subcuticular.' When abundant, this subcuticular rash imparts to the skin a mottled or marbled aspect, which contrasts with the darker more defined spots before described, although sometimes the two appear to pass into one another. The eruption of typhus varies greatly in its appearance, according to the relative abundance of the mottling and more distinct spots. Sometimes both are plentiful; sometimes there are only a few of the more distinct spots; and at other times, there is nothing but a faint subcuticular mottling, which is apt to be overlooked. Its appearance also varies according to the degree of isolation or confluence of the distinct spots. The spots and mottling together constitute an eruption which Jenner has described as the 'mulberry rash' of typhus, but which other writers have designated measly, morbilliform, or rubeoloid."—P. 127.

This description is illustrated by beautifully executed lithographs of the eruption.

General convulsions, as is well-known, constitute one of the most formidable symptoms of typhus, and in reference to the pathology of the disease, it is interesting to find that in the great majority of cases, convulsions are associated with an albuminous condition of the urine: it is therefore nearly certain that convulsions in typhus have a uræmic origin; and this opinion is still farther confirmed by the circumstance, that Dr Christison and other observers have detected abundance of urea in the blood of patients labouring under this manifestation.

A full account is given of the complications and sequelæ of typhus. We can only direct attention to Dr Murchison's account of the inflammatory swellings or buboes which are not uncommonly met with. These generally occur towards the termination of the primary fever, and though most frequently affecting the parotid and submaxillary regions, are also met with in the axilla, the groin, the mamma, the arms, the thighs, and legs. These swellings are always considered as a formidable complication of the fever. Buboes, it is well known, constitute one of the most characteristic symptoms of the oriental plague, and Dr Murchison, in connexion with this subject, brings forward various arguments to prove that there is a strong analogy between typhus and the plague in their causes and their symptoms, and that, in fact, typhus is probably the plague of modern times. Plague, in early times a frequent scourge of this country, has been for long unknown in London, and is now far less frequent than formerly in the East; and there can be no doubt that, in this country at least, improved hygienic arrangements in regard to dwelling-houses have led to its disappearance. The great fire of 1666 did more in banishing plague from London, than could have been effected by any of our modern legislative enactments.

The treatment of typhus recommended by Dr Murchison is based on a careful consideration of the pathology of the disease. "Our objects," he says, "in the treatment of typhus, should be,—1. To neutralize the poison, and to correct the morbid state of the blood; 2. To eliminate the poison and the products of the destructive metamorphosis of tissue; 3. To reduce the temperature; 4. To sustain the vital powers, and to obviate the tendency to death; 5. To relieve the distressing symptoms; and, 6. To avert and attack local complications. By such means nature is assisted in effecting a cure; for it is wrong to imagine that it is a matter of indifference what plan of treatment is adopted." The first of these indications is to be fulfilled by the administration of the mineral acids, especially the nitro-muriatic; the second, by the addition of five-grain doses of the nitrate of potash to the acid drink, by the use of the bitartrate of potash and gentle laxatives; the third, by sponging the surface, or the cold affusion; the fourth, by means of nutritive and easily digestible food, and the administration of stimulants; while the fifth and sixth are to be carried out according as the circumstances of particular cases demand. In speaking of the treatment by bloodletting, at one time a favourite remedy, but now most properly discarded, Dr Murchison takes occasion to refer to the change in type which is supposed to have taken place in various acute diseases, especially in fever. To this doctrine Dr Murchison demurs, and in regard to fever he believes and proves that the change has been one, not of type, but of disease. The explanation consists in this, that the epidemics in which bloodletting seemed to be attended with success, were examples, not of typhus, but of relapsing fever, in which the mortality is naturally small, although it was probably increased by this very agent. The same subject is alluded to on several other occasions, and the observations regarding it (originally published in this Journal nearly five years ago) are highly creditable to the sagacity of the author.

The third chapter treats of relapsing fever in the same able and exhaustive manner as was displayed in the consideration of typhus. Our limits, however, prevent us from doing more than refer very briefly to the etiology of this disease. That relapsing fever is contagious, probably to much the same degree as typhus, is proved by abundant evidence. That it is susceptible of spontaneous generation seems equally to admit of no doubt; it disappears for prolonged periods, and manifests itself under circumstances where its outbreaks cannot be explained by importation, but are evidently connected with certain external conditions. These conditions consist in the existence of great destitution, whether caused by the failure of harvests or by want of employment. It does not seem due, like typhus, to overcrowding of the population, though the action of the poison whence once generated is strongly favoured by deficient ventilation; for it has frequently shown itself in small villages, well-aired country districts, and even in the most isolated situations. That the generation of the poison is closely connected with destitution is sufficiently proved by the names given to the disease by various writers; it is the famine fever of the Irish, the Armentyphus and Hungerpest of the German writers. Dr Murchison's opinion is strongly in favour of this view of its generation; but though there can be do doubt that this form of fever and destitution are closely connected, we question whether the disease can be produced by famine alone; it does not occur in isolated instances of starvation, and in all probability some other, and as yet unknown, agency is at work. The following passage contains a resumé of Dr Murchison's opinion on the subject.—"Of all the causes that can be assigned for the origin of relapsing fever, it seems to me that destitution is the most tenable. 'We give the name,' says Brown, 'of cause to the object, which we believe to be the invariable antecedent of a particular change;'[1] and such appears to me to be the relation of destitution to relapsing fever. But whether or not they stand in the relation to one another of cause and effect, the intimate connexion between them is indisputable. The facts bearing on this point are most important, even if the theory founded on them be not accepted."

The third variety of fever treated of is the "pythogenic or enteric." This in some respects is the most interesting of all, as it has only lately been distinguished as a special form of disease, and as its exact position in a pathological classification is by many held to be not yet decided. We are sorry to have to differ from Dr Murchison in the very beginning of this section, for we cannot express our approval of the nomenclature proposed. Dr Murchison very properly objects to the use of the term *typhoid*, as leading to an erroneous notion of the nature of the disease. Modern research has shown that there are most important differences between this fever and typhus; and it is certainly at utter variance with scientific precision to designate one disease by a term intended to show its similarity to another from which, however, it is essentially distinct. We are, therefore, quite willing to drop the term typhoid as applied to this fever. We cannot, however, consent to accept the substitute proposed by Dr Murchison. Believing that this fever is generated by decomposition, Dr Murchison, some years ago, proposed to designate it as "Pythogenic" (*πύθογενης* from *πύθων* (*πύθομαι*, putresco) and *γεννάω*), and employs the appellation in his present work. For several reasons we object to this term; in the first place, we do not think it desirable in the present state of our knowledge to attempt to introduce a strictly scientific nomenclature of disease, and even if it were possible, etiological considerations would not form the best foundation; in the second, it is by no means universally admitted that putrefaction is the cause of the disease in question, and it is not permissible to endeavour by a new name to support a still doubtful theory; and, in the third place, we already possess in the term *enteric* an appellation nearly unexceptionable, which is simply suggestive of one of the chief characters of the disease, and which we cannot agree with Dr Murchison in considering as calculated to convey the erroneous impression, that the fever is the result of the intestinal lesion. We cannot, therefore, consent that the new, and by no means euphonious term, shall take the place of the already familiar designation.

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[1] Inquiry into the Relation of Cause and Effect, 3d ed., Edin. 1818.

On looking back to the older authors, it seems certain that some of their descriptions apply to this form of fever, but it was not till our own day that it was accurately differentiated from typhus. Many, indeed, still maintain that typhus and enteric fever are mere varieties of the same disease, and this is an example of the difficulty of seizing the points of difference between somewhat allied diseases. In the present day it seems strange to us that small-pox, measles, and scarlatina should ever have been confounded together; but we have no doubt that it will be equally surprising to our successors, that careful observers should ever have doubted of the non-identity of typhus and enteric fever. Dr Murchison has furnished us with an exceedingly careful and discriminating history of the progress of opinion on this subject; he has shown how, between 1734 and 1738, a distinction was drawn by Gilchrist and Huxham between the slow nervous fever and the malignant fever of the hospital; how the intestinal lesions of the former had been noticed by John Hunter; how Bretonneau proved that enteric fever was always localized in the solitary and agminated glands of the ileum; how finally, mainly owing to the researches of Dr A. P. Stewart and Dr Jenner, the specific characters of the two diseases were established.

The etiology of enteric fever is interesting, and there is still some difference of opinion in regard to it. There have been disputes as to whether or not it is contagious; Andral, Chomel, Dr Stewart, and many others have expressed doubts as to its ever being communicated in this way; others have believed it to be communicable, though in but a limited degree; while Dr W. Budd maintains that the contagious nature of enteric fever is the "master-truth" in its history. We agree with Dr Murchison in believing that while it cannot be denied that the disease is communicable by some poison emanating from the bodies of the sick, the diffusion of the disease is not to be accounted for in this way. A more difficult question is that regarding the mode of generation of the poison. So far as we know, no attempt was made to explain its origin, which was merely alluded to as obscure and unknown, until in 1858 Dr Murchison presented to the Royal Medical and Chirurgical Society an essay in which he endeavoured "to prove that fever arising from sewer emanations was always enteric fever, and never typhus or relapsing fever; and to show that this mode of origin explained why enteric fever was endemic in many places, but often epidemic in circumscribed localities; why it attacked the rich as well as the poor; why it occurred in isolated country-houses as well as in large towns, and why it was most prevalent in autumn and in warm seasons." In his present work Dr Murchison repeats a portion of the evidence he formerly adduced, and brings forward additional facts in favour of his opinion. To our mind Dr Murchison has made out his case;

there is an amount of evidence arising from the appearance of the fever in circumstances where contagion, the possibility of importation, and all other agencies with a single exception were excluded, that we feel bound to believe that sewer emanations, or drinking water contaminated by sewage, are the ordinary exciting causes of the disease. The only other theory as to the origin of enteric fever to which we need advert, is that advanced by Dr W. Budd, and supported by Mr Simon. According to Dr Budd, the poison of enteric fever, although contained in sewage, is always derived from the alvine evacuations of an individual already labouring under the disease, and drains are merely the vehicles of its propagation. While we fully admit, as Dr Murchison and most others do, that the poison is contained in the stools, and that emanations from them are the chief mode of communication from the sick to the healthy, we cannot agree with Dr Budd in believing that the existence of such stools is essential to the production of the disease. In fact, this theory denies the spontaneous origin of enteric fever; for, if the alvine evacuations of an enteric patient are necessary for the production of the disease, it is evident that in the absence of such evacuations the disease cannot show itself. But as there is evidence that enteric fever does arise *de novo*, and as Dr Budd's theory does not explain how this takes place, we are forced to seek for a more general mode of causation, and this we think we find in the action of the gases emanating from sewers. Dr Murchison's conclusions we give in his own words,—" The etiology of pythogenic fever may be thus summed up:—

" 1. Pythogenic fever is either an endemic disease, or its epidemics are circumscribed.

" 2. It is most prevalent in autumn and after hot weather.

" 3. It is independent of overcrowding, and attacks the rich and poor indiscriminately.

" 4. It is often generated spontaneously by fæcal fermentation.

" 5. It is occasionally communicated by the sick to persons in health.

" 6. This transmission is probably effected by the stools, which are usually prone to decomposition."

The symptoms of enteric fever are described with great accuracy and minuteness, and, as in the case of typhus, the appearance of the eruption is illustrated by admirably executed lithographs. Great stress is laid upon the eruption as a diagnostic character of the disease, and as we have already quoted Dr Murchison's account of the typhus rash, we may, by way of contrast, extract a portion of the description of the appearance of the skin in this disease:—

> " The eruption of pythogenic fever consists of isolated lenticular spots—the '*tâches roses lenticulaires*' of Louis. Their colour is rose or pink, but varies slightly in tint, according to that of the patient's skin. Their form is rounded and regular; their margin is well-defined; and they measure from half a line to two lines in diameter. When the point of the finger is passed gently along the skin, each spot can in most cases be felt slightly elevated above the surface.

Their outline is rounded and convex, but not acuminated. They are never indurated; but, in very rare cases, a minute vesicle may be discovered at their apex. They are never converted into petechiæ; but during the whole period of their existence they disappear completely on pressure, and return when the pressure is removed. They are never observed on the dead body. They are developed in successive crops, each spot lasting three, four, or five days, and then fading, while fresh spots continue to appear. I have verified this observation hundreds of times, by surrounding daily every fresh spot with a circle of ink, and writing the date of each on the skin. The number of these spots is usually small; and hence they are often overlooked. In most cases, the number present at one time does not exceed twenty or thirty; and sometimes three or four spots are all that can be discovered. Occasionally, however, the spots are very numerous, and then the edges of two contiguous spots may cohere. Still this is a rare circumstance, and the spots are never seen to merge into irregular patches, as occurs in typhus."—P. 468.

So much stress is laid upon the characters of the eruptions in the two diseases, that at a later part of his book Dr Murchison lays down the two following propositions:—

"1. When lenticular rose-spots, as described at page 468, appear in successive crops in the course of continued fever, the abdominal lesions of enteric fever are invariably present.

"2. When the eruption of typhus, described at pages 117 and 127, shows itself in the course of continued fever, the abdominal lesions of enteric fever are absent."—P. 581.

The treatment of enteric fever is founded upon the same principles as those which should guide us in the management of typhus; it is modified, however, by the circumstance, that in the former the state of the intestines constitutes a formidable local complication, and that diarrhœa is the principal symptom demanding interference. In the treatment of diarrhœa, Dr Murchison trusts chiefly to acetate of lead in three-grain doses, with or without the eighth of a grain of acetate of morphia, although other remedies may sometimes be resorted to with advantage.

A separate chapter is devoted to a consideration of the specific distinctions of typhus and enteric fever. In it, Dr Murchison sums up the evidence derived from symptoms and post-mortem appearances, as well as from etiology, and arrives at the conclusion, in which we thoroughly concur, that the two diseases are essentially distinct, are characterized by different symptoms, are accompanied by different morbid appearances, and are generated by different poisons. In regard to this question it is very important to know that while an attack of one fever generally confers an immunity from a subsequent attack of itself, it is not protective against the other.

In taking leave of this work it is scarcely necessary to express in formal terms our high sense of its value; it is a very important addition to medical literature, founded upon extensive experience, illustrated by unwearied research, and characterized by clearness and accuracy of reasoning; while its author has on all occasions been careful to assign to his predecessors in the inquiry their full share of merit. A subordinate, though by no means a trifling recommendation, is the presence of a copious bibliography, and a full general index.

Dr Tweedie's book is of a different character from Dr Murchison's. It consists of a republication of his lectures delivered before the Royal College of Physicians: before such an audience a consideration of the more obscure points of etiology and pathology would manifestly have been out of place, and the work is rather a good practical account of what has been well ascertained regarding the different forms of fever, than an attempt to extend our knowledge of their causation or essential nature. Though the work fully carries out the object proposed by its author, our remarks regarding it must necessarily be very brief.

Dr Tweedie is a believer in the non-identity of typhus and enteric fever; and his testimony on this point is the more valuable, because he was for many years a warm supporter of the opposite doctrine. A careful consideration of the writings of modern authors, but more particularly the investigations carried on by Dr Jenner under his own observation in the London Fever Hospital, convinced him that these are distinct diseases, and he now unhesitatingly maintains "that the two forms, though having a close resemblance in the general symptoms, and having many features in common, are as distinct from each other as they are from periodic or from exanthematous fevers." The arguments brought forward in favour of this view are founded upon the symptoms and mode of access of the two diseases, the duration of the symptoms, the character of the respective eruptions, the different susceptibility to the two forms at different periods of life, the causes which produce them, their different degrees of contagiousness, and the results of treatment.

After having discussed the general question, Dr Tweedie proceeds to consider the different forms of fever in detail, and gives a very good account of their symptoms, morbid anatomy, and the treatment required. Four coloured plates give a very fair idea of the characters of the eruptions in typhus and enteric fevers, and of the appearances of the intestinal ulcers in the latter disease. The lectures altogether are evidently the production of a man who has had an extensive experience in the treatment of fever, and who has turned his experience to a good account. In one respect Dr Tweedie has left his work imperfect; he has furnished no index to his book, and the table of contents at the beginning of the volume is of the most meagre possible description. In the present day, when every one endeavours to live as *fast* as possible, there is really no time to be spared for hunting through three hundred pages, in order to discover the opinions of an author on a particular point. Dr Tweedie also has not been sufficiently careful in quoting his authorities; no doubt the fact of the form of lectures having been adopted, was the cause of this; but when he decided upon publication, he would have done more justice, both to himself and others, had he been more particular in this respect. As a specimen of Dr Tweedie's style, we may quote the following observations regarding the use of wine in enteric fever:—

> "To sum up the question of the administration of wine in enteric fever, I would observe,—1*st*, that it should not be prescribed indiscriminately, but as an occasional remedy in special cases; and, 2*d*, when there is doubt as to the propriety of giving or withholding wine, if the case be one of well-marked enteric fever, especially in the early stage, it will be safer to withhold it, at all events for a time; and, 3*d*, in every case, but particularly when wine is not clearly indicated, the effect of the first few doses should be closely watched, and if the general complexion of the symptoms be improved, it may be continued with care; if, however, it excite the patient, if restlessness and delirium be induced, if the tongue become more and more dry, the pulse more rapid and wiry, you may infer that wine is not suited to the case—at all events, at its present stage—and it should therefore be withdrawn."—P. 243.

And, again, in speaking of the excessive amount of stimulants which have lately been recommended in London, he says,—

> "The enormous quantities of wine and brandy recommended in even the early stage of fevers, whatever be the form, the individual circumstances, or whether there be local affections present, have often surprised me, and inclined me to doubt the accuracy of the statements. I have certainly seen intercurrent inflammation materially aggravated by the injudicious stimulation adopted, and on more than one occasion all the ordinary characters of acute delirium tremens supervene, when the unlimited administration of brandy had been left to the discretion of a nurse, who fancied that she was only obeying instructions when she poured down dose after dose. There is surely no practical philosophy in such indiscriminate abuse of a really valuable remedy when given on rational principles; and I deem it the duty of every physician who is convinced of the dangerous tendency of the Brownonian doctrine applied indiscriminately in the treatment of diseases, acute as well as chronic, to express his opinion boldly and decidedly, that the young and inexperienced practitioner may be warned of the dangerous consequences of this recently revived doctrine."—Pp. 265–66.

## *On Long, Short, and Weak Sight, and their Treatment by the Scientific Use of Spectacles.* By J. SOELBERG WELLS, M.R.C.S., M.D., etc. London: John Churchill: 1862.

ALL surgeons are well aware of the very great harm often done in the cases of patients labouring under affections either of the accommodation or refraction of the eye, by the hurried, indiscriminate, and unscientific manner in which the spectacles are selected, by which they expect to benefit.

A shortsighted child finds the glasses of some older friend lying about the house, and though they make his eyes ache he finds they increase his powers of vision. He continues to use them, though far stronger than he really requires. The consequence is a permanent increase in his myopic condition, and the necessity for the constant use of lenses the strength of which requires to be occasionally increased.

Some patients, too, have the idea that one pair of glasses is as good as another, or a deal better, and thus purchase new ones, or obtain loans from their friends, with an utter disregard of the nature or extent of the defect which requires the instrumental aid.

The treatise before us is an account of the different theories regarding the affections of accommodation and refraction, which constitute long, short, and weak sight respectively, and is meant as a guide for the surgeon in their differential diagnosis, and also in the selection of lenses which shall *exactly* suit each case.

Chapter I. contains a brief but accurate account of the mechanism of accommodation of the eye, and a short notice of the various theories regarding it. The idea that an alteration in the refractive power of the cornea is the cause, is disproved by Helmholtz's ophthalmometer, which shows that no change in its curvature takes place. Arlt's view that the muscles of the eyeball assist the internal structures is disproved by Von Graefe's case, in which all the recti and obliqui muscles of both eyes were paralyzed, and yet the power of accommodation was perfect.

Dr Wells adopts the theory of Cramer and Helmholtz (regarding the correctness of which few anatomists now entertain any doubts), that the necessary change in the refraction of the eye during accommodation is due to an alteration in *form* of the crystalline lens. This change is in the curvature of the anterior and posterior surfaces, and not due (as some wrongly interpreting the Germans have supposed) to any alteration in the *position* of the lens.

How is this alteration in form caused? Cramer ascribes it to simultaneous tension of the radiating and circular fibres of the iris. To this Donders adds the ciliary muscle, which, by its contraction, drawing the peripheral edge of the iris backwards against the wall of the canal of Schlemm, forms a fixed point for the action of the radiating fibres of the iris. Helmholtz and Heinrich Müller both give special prominence to the action of the ciliary muscle combined with that of the iris, and Mr Wells gives a brief summary of their views, especially the observations of the latter on the two different sets of fibres in the ciliary muscle.

Dr Wells is disposed, however (on the strength of a single case of complete removal of the iris in Von Graefe's clinique, in which the power of accommodation was still normal), entirely to dispense with the action of the iris in his explanation of the process of accommodation, and to give the whole credit to the ciliary muscle.

We cannot on this single evidence consent to deprive the iris of its share in the work, especially when we remember the power nature has to supply deficiencies in one part by giving added function to another. We have no more right, on the evidence of Von Graefe's case, to exclude the iris from participation in the changes for accommodation, than we would have to say that the crystalline lens has nothing to do with vision, because we occasionally find patients who can see tolerably well after its removal, or where it is congenitally absent.

An interesting chapter on the range of accommodation introduces the subject of myopia, in which condition the range of accommodation is diminished, unless the eye be assisted by a concave lens. After speaking of the ordinary causes of shortsight, such as hereditary predisposition, excessive use of the eyes upon small objects, as in fine needlework or engraving, Dr Wells directs attention to an alleged cause of shortsight hitherto little understood—sclerotico-choroiditis posterior. The textural change, the result of this local inflammation, is said to cause a lengthening of the antero-posterior diameter of the eyeball, as a result of which parallel rays meet in focus *in front* of the retina. But we cannot, without some caution, accept this as an explanation or cause of myopia, for it implies the existence of a stage in this sclerotico-choroiditis posterior in which there is a textural change sufficient by its mere mechanical thickening to alter the *focus* of vision, while at the same time it has caused no change in the *distinctness* of vision, if the focal error be corrected by the use of a lens.

Besides this, neither the ophthalmoscopic appearances, nor the usually well-marked subjective signs of choroidal inflammation are present in the sclerotico-choroiditis.

The section in this chapter, which explains Donder's method of estimating the degree of myopia, will prove very useful to any one who has not access to the original.

A short account of presbyopia is followed by an interesting chapter on hypermetropia,—"that peculiar condition of the eye in which its refractive power is too low, or its optic axis (antero-posterior) too short," and to which many cases of that peculiar weakness of sight asthenopia may be referred. In hypermetropia, parallel rays are united not *on* the retina but *behind* it, and thus we are enabled to explain those curious exceptional cases in which *convex* glasses are found to assist in distant vision, while similar, or even stronger lenses are required for near objects.

A notice (necessarily very brief from our at present limited knowledge) of paralysis, spasm, and atony of the ciliary muscle concludes this very practical yet scientific treatise, in which the latest views of the German ophthalmologists are presented to English surgeons in a compact and readable form.

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## *The Spas of Europe.* By JULIUS ALTHAUS, M.D. 8vo, pp. 494. London: Trübner and Co.: 1862.

THE interest attaching to mineral waters is most naturally developed to its fullest extent in those countries which abound in the commodity in question. Mineral waters are not only valuable medicinally, but are, moreover, of vast commercial importance. Strip the continent of Europe of its medicinal springs, and at once a large source of its income perishes, just as, in kind, though in a much higher degree, would result from a failure of the coal and iron royalties of Great Britain. This country has at no period been very famous for its mineral waters; still less so now, when fashion and facility both lead to the crowded resorts of Germany and the Pyrenees. Probably it is due to the questionable merits of most of the medicinal waters of this country that our valetudinarians are compelled to seek in foreign lands that healthy influence which is denied to them at home. At least, in part, this must be the cause, for wherever there is unmistakable merit, thither will the tide of health-seekers flow. Undeniably, however, fashion, guided frequently in these matters by attendant frivolity, too often steers from our shores the bark freighted with *soi-disant* invalids,—for they are not all *invalids* who journey to Baden. But, it may be asked, why may not they who are really sick have their necessities supplied by the importation of foreign mineral waters, or, knowing their chemical constitution, by factitious drinks, and so be spared the expense and tedium of a long journey, and the too often injurious results of the exciting whirl of a continental watering-place? Dr Althaus tells us that one reason why medical practitioners ought to be acquainted with the action of mineral waters, is because of their more common use in the present day, and that partly on account of "the high degree of perfection attained in the manufacture of artificial mineral waters." We confess to a want of faith both in factitious and bottled mineral waters. If kept too long the virtues of the water must be impaired; its salts change their character more or less; its gaseous elements are either altered or lost, and if originally of high temperature, no subsequent application of artificial heat can restore it to its former state. In many cases, no doubt, in which the water is used within a short time of its importation, and in which the disease and the application are well adapted, beneficial results will follow; but to the crowd of health-seekers making its annual rush to the Continent, more than the mere mineral water is essential, fresh though it be from the bosom of its parent earth. Like the motley assemblage gathered in St Paul's Churchyard on a foggy 9th of November, it is not the chief magistrate, but the Lord Mayor's *show* it goes to see.

According to our author, every country in the world is possessed of mineral waters—from the torrid to the frigid zones, from Siberia to the Cape of Good Hope. To follow him through his wanderings from place to place is beyond our province; but there are many of the shorter passages of the work from which an occasional quotation may be given to show the general scope of Dr Althaus's remarks. We are glad to find that charlatanism is on the wane, so far as mineral waters served its purpose; but we fear, even with our author's judicious reserve, that there is still a great deal more of knavery practised at many of the continental spas than he allows.

"The mode of using mineral waters is now widely different from what it was in former times. The principle, that the benefit is more considerable the more water is drunk, and that the 'crisis' and 'critical excretions' are necessary if the treatment is to be successful, is now rejected by all enlightened members of the medical profession. Many cases have occurred in which large quantities of water drunk in rapid succession have not only greatly disturbed digestion, but have caused dropsy and general prostration; and a moderate use of the springs is, at the present day, justly believed to be the only safe plan. The rules to be observed for the use of mineral waters have also been greatly changed. He no longer advises 'preparatory cures,' 'great cures,' 'little cures,' 'prophylactic cures,' and 'after cures;' but the mode of treatment is made to suit the requirements of each individual case."

Describing the class of patients to which mineral waters are suitable, Dr Althaus says:—

"Spas are only suitable for patients suffering from chronic diseases, and in those only if the composition of the blood has not become too much altered, and in the absence of considerable structural changes of important organs."

That the author himself believes in the determining influence of surrounding objects at the several spas, is shown in the following remark:—

"The pleasant neighbourhood and sublime scenery which surrounds many spas greatly aid the curative effects of the waters; while the gloomy and wild aspect of others may, at least in a certain number of cases, retard the benefit which would otherwise accrue from the peculiar virtues of the springs."

In the several chapters into which the work is divided, the history, physical properties, chemical composition, geographical distribution, physiological action, and therapeutic uses of mineral waters are treated of; and a general description, with chemical analysis, of each of the spas mentioned is given. "The numerous questions connected with the nature and action of mineral waters," from the preface, "have been the subject of long and earnest study on my part; and having been often asked for information respecting them by my professional brethren and others, I now venture to submit to their notice this treatise, which, it is hoped, may fill up a hiatus that has hitherto existed in British medical literature." We cordially commend Dr Althaus's book to the consideration of our readers, believing that he has done good service in placing before the profession so much information in so agreeable a form.

With a view to the convenience, and as an aid to the memory of the reader, Dr Althaus has constituted the following classification of mineral waters in accordance with their chemical composition:—

1. ALKALINE SPRINGS: (*a*) *Alkaline acidulous springs.* Chief contents: carbonic acid and bicarbonate of soda; representative, Vichy. (*b*) *Alkaline muriated acidulous springs.* Chief contents: carbonic acid, chloride of sodium, and bicarbonate of soda; representative, Ems. (*c*) *Alkaline saline springs.* Chief contents: bicarbonate and sulphate of soda; representative, Carlsbad.

2. BITTER WATERS. Chief contents: sulphates of soda and magnesia; representative, Friedrichshall.

3. MURIATED WATERS: (*a*) *Simple muriated waters.* Chief contents: a moderate amount of chloride of sodium; representative, Wiesbaden. (*b*) *Muriated lithia waters.* Chief contents: chlorides of sodium and lithium; representative, Baden-Baden (Murquelle). (*c*) *Brines.* Chief contents: a large amount of chloride of sodium; representative, Rehme. (*d*) *Iodated muriated springs.* Chief contents: iodide of sodium; representative, Castrocaro. (*e*) *Bromated*

*muriated springs.* Chief contents: bromide of magnesium; representative, Kreuznach (Oranienquelle).

4. EARTHY SPRINGS. Chief contents: Carbonate and sulphate of lime; representative, Leuk (Louèche).

5. INDIFFERENT THERMAL SPRINGS. Chief contents: a very small amount of salines; representative, Gastein.

6. CHALYBEATES: (*a*) *Acidulous chalybeates.* Chief contents: carbonic acid and bicarbonate of protoxide of iron; representative, Schwalbach. (*b*) *Saline acidulous chalybeates.* Chief contents: sulphate of soda and bicarbonate of protoxide of iron; representative, Franzensbad.

7. SULPHUROUS SPRINGS: (*a*) *Springs containing sulphuretted hydrogen;* representative, Eilsen. (*b*) *Springs containing sulphurets of metals;* representative, Bagnères de Luchon.

*Die Musculatur am Boden des Weiblichen Beckens.* Von Dr HUBERT LUSCHKA, Professor der Anatomie in Tübingen. [*The Muscular System of the Floor of the Female Pelvis.* By Dr HUBERT LUSCHKA, Professor of Anatomy in Tübingen.]

*Illustrations of the Surgery of the Female Pelvic Organs, in a Series of Plates taken from Nature; with Physiological and Pathological References.* By HENRY SAVAGE, M.D. Lond., F.R.C.S., Physician to the Samaritan Hospital for Women. Churchill: London: 1863.

BOTH of these excellent works are illustrated by beautiful plates. Those of Dr Savage are got up in the style of the magnificent anatomical works of Bourgery and of Hirschfeld, and a coincidence in the name of M. Leveille, who is the draughtsman, leads us to think there is even a closer connexion between them than mere resemblance of style. Dr Savage, indeed, acknowledges, in regard to some of them, that they are copied from Hirschfeld (and Leveille's) work. These plates are said to be drawn from nature; but this gives a very erroneous idea, if it is intended to imply that they are drawings of dissections. They are a refined ideal view of what a dissection might be, just as a Venus in marble might be said to be "from nature," without a wrinkle or spot or any such thing. Let any one inspect, first, a dissection of the female perineum, and then these plates, and he will appreciate what an anatomist may mean by "drawn from nature." We do not make these remarks with any wish to depreciate the great beauty and value of Dr Savage's work, which we recommend to the profession. At the same time we wish he had entered upon the subject of the relations of the female pelvic organs to one another in states of repletion and vacuity in the living female, and availed himself of the homalographic sections made by aid of congelation and otherwise. His Plate VI. might have been made greatly more correct, more from nature, and more valuable by this means. We find that Dr Savage adopts, in the main, the usual descriptions of the muscles of the female pelvis. In our heading to this article we have placed an excellent, laborious, and exact work, by Dr Luschka, to which Dr

Savage might be expected to have referred. We cannot pretend to say that we have verified Dr Luschka's observations, but they have every appearance of being substantial anatomical discoveries, or improvements in anatomical description, and they are endorsed by an author of high reputation. When we tell British readers that Dr Luschka describes a retractor ani, a compressor bulborum, a constrictor vestibuli, a levator vaginæ, and gives minute and new descriptions of portions of the well-known muscles of the part, it will be evident that a deal of new work has been done in this region; and while Dr Savage's work is of this year, Dr Luschka's was published in 1861.

Dr Luschka simply and barely describes the muscles of the region embraced in his monograph. Dr Savage intersperses his anatomical descriptions with practical reflections and advices, and we have been highly gratified by their soundness and philosophical spirit.

## *Clinical Observations in Surgery. Part II.* By DR FAYRER, Professor of Surgery and Surgeon, Med. Col. Hospital. Calcutta: 1863.

THAT Dr Fayrer is an enterprising and successful surgeon is proved both by his former writings and by the present publication. This little work of fifty-six pages contains an account of many interesting cases and important operations. As elephantiasis genitalis, or hypertrophy of the coverings of the genital organs, is of rare occurrence in this country, we may briefly give Dr Fayrer's experience in regard to it. During the three years from May 1859 to July 1862, he operated on account of this disease twenty-eight times; twenty-two of the patients recovered, and in all these, the functions of the organs appear to have been restored; six died, one from shock and slight hæmorrhage supervening three hours after the operation, the others from pyæmia. The disease, from Dr Fayrer's account, seems to be far more common among the Hindoos than among the Mahometans. Age appears to have an important influence in determining its occurrence; only four of the patients were below thirty, eighteen were between thirty and forty, four were between forty and fifty, and one was fifty years of age; the youngest patient was aged twenty-one. In reference to this fact, it may be interesting to mention that Professor Syme lately operated successfully on account of this disease on a boy of only thirteen. Dr Abbaté of Alexandria, who has had much experience in this disease, saw the patient with Mr Syme, and recognised the case as one of true oriental elephantiasis; he was present at the operation, and stated that he had never seen the disease in so young a subject. The patient, it may be added, was a native of Scotland, and no cause whatever could be assigned for the disease.

Dr Fayrer's pamphlet contains cases of various other forms of disease, and is altogether creditable to the medical school of Calcutta.

# Part Third.

# PERISCOPE.

## SURGERY.

### PARACENTESIS THORACIS. BY HENRY I. BOWDITCH, M.D., BOSTON.

In the American Journal of the Medical Sciences, April 1852, I published an article on Paracentesis Thoracis, with cases. In January 1854, I gave in the American Medical Monthly an analysis of twenty-five cases of the same. Still later, I made further statements on the subject in the Boston Medical and Surgical Journal, 1857, and now I wish to give to the Society a brief *résumé* of the principal results of the operation, as performed 150 times on 75 persons during a period of nearly twelve years, viz., from 17th April 1850, to 17th December 1861.

During the above period I have operated 150 times on 75 persons, and have seen other gentlemen operate on 10 more, making 160 operations on 85 persons.

*Innocuousness of the operation.*—I have never seen the least *permanent* evil resulting from any operation, and *only the slighest temporary* difficulties, such as pain, slight dyspnœa, stricture, or cough, etc. This, I think, shows the innocuousness of the operation by means of the exploring trocar and suction pump, as suggested by Dr Wyman.

*Frequency of the operation.*—One lady (case formerly reported) I tapped 9 times during 8½ months; commencing when she was 4½ months pregnant, and in whom the orthopnœa was, several times, so great that death, I have no doubt, would have supervened within twenty-four hours, if the operation had not been performed. She is now tolerably well, but with a contracted chest, as is usual in chronic pleurisy.

In striking contrast with this case, as it regards the *frequency* of the operation, while resembling it in the number of times it was performed, was the unique case of an elderly man, very recently under my care, and in which I tapped eight times in six weeks! The patient himself, a physician, earnestly and even solemnly demanded of me the operation as a mere means of relief to intense distress. As he jocosely remarked on one occasion, he considered it one of his "luxuries."

*Cases in which the patient recovered wholly.*—Twenty-nine out of the seventy-five patients got wholly well, apparently in consequence, chiefly, of the operation. The operation was performed generally when severe symptoms were manifest, and I was called in consultation. In a few, a great quantity of fluid was recognised by the physical signs alone, the rational having been slight; but, as the disease was chronic, an operation was deemed necessary. In all these cases, the operations seemed the *first step* towards a cure.

*The character of the fluid and its influence on the prognosis.*—In 26 out of the 75 *serum* was found; and 21 of these patients got wholly well. If after the first operation the fluid becomes purulent, an almost certain fatal prognosis should be made. I have seen six such cases. Four of the patients died, two were lost sight of, but, when last seen, were failing.

*Pus* was found at the *first* operation in twenty-four cases. Once, it was of the consistence of honey, but I easily drew it through the exploring tube. Seven of these patients recovered wholly; seven died; nine were relieved one or many times; but they had either a long and tedious illness, terminating usually in phthisis, or a fistulous opening, or a still doubtful result.

A *sanguinolent fluid* at the first puncture (and by that I mean a dark red thin fluid, evidently stained with blood, though not coagulating) I consider almost certainly fatal, and a consequence of some malignant disease of the lung or pleura.

There were seven of such cases. In six the patients died. In one there was a doubtful result, but apparently fatal tendencies were commencing. If the fluid is found sanguinolent at the second or any subsequent puncture, I deem it of comparatively *little* importance towards the prognosis.

A mixture of *bloody purulent fluid* at the first operation is usually fatal. Three cases, all fatal, occurred.

A *fetid gangrenous* fluid is very rare, only one case having occurred, and that fatal; but in this case infinite relief from horrible orthopnœa was obtained, and it never returned, though the patient sunk and died in a few days. Gangrenous pleura was found.

I have operated once in *pneumo-hydrothorax* with temporary relief and comparative ease for several days. Many theoretical objections may be urged against the operation in such a case. To such objections I have simply to answer that, as the operation can do no harm and may give much relief, I shall operate again in any case where the dyspnœa may be so great as to require it.

*Cases where no fluid is obtained.*—Finally, in seven cases I got no fluid. These cases occurred usually in the earlier operations, and the failure was often owing, I have doubt, to the cautious and slow manner with which I plunged the trocar between the ribs, carrying thus the false membrane of the pleura costalis before the instrument instead of piercing it; so that a valve was really formed over the end of the canula. At other times I have little doubt that an error of diagnosis was made, and that instead of a fluid there was smply an unexpanded lung and thick false membranes on the pleura, causing as much dulness on percussion and absence of respiration as a fluid would have done. The differential diagnosis of the two was not, at first, quite so easy as it is now. Inspection usually is the test between the two conditions; the intercostals being distinct, and depressed when a membrane exists; but very indistinct and level with the ribs, or, possibly prominent, when a fluid occupies the chest.

Once an immense tumour occupied and uniformly distended one pleural cavity, and in its course presented all the phenomena, natural and physical, of simple pleurisy. I tapped three times, viz., at the back, side, and front, at the same visit. No evil followed.

A member of this Society asked me my experience as to the good results or otherwise in operations on the right side; observing, at the same time, that one European writer (Trousseau?) contended that pleurisy of the right side is often or always tuberculous. I could not answer definitely, although my impressions were against the truth of the assertion. On referring to the brief summaries, and not to the original notes of my cases, where I find the sides named in 25 cases, I find that in these the operation was performed with the following results:—

| | PLEURISY OF | |
|---|---|---|
| | Right side. | Left side. |
| Death . . . . . . . . . . | 4 times. | 5 times. |
| Cure entire, without symptoms of phthisis, except in one, but pleurisy was cured in that . . . . | 9 ,, | 4 ,, |
| Doubtful result . . . . . . . . | 1 ,, | 2 ,, |
| | 14 | 11 |

These data do not exactly answer the question proposed; but if tubercles always or more frequently exist in pleurisy of the right side, we should, *à priori*, anticipate more unfortunate terminations of the operation of paracentesis of the right than of the left side. My experience proves exactly the reverse, and may be expressed, if deduced from the above table, as follows:—

Of 25 cases, 14 were of the right side, 11 of the left. Of the 14 of the right side, only one person is mentioned as having tubercles, and in that the pleurisy was cured and the pulmonary symptoms mitigated.

Of the 14 persons tapped in the right side 28·57 per cent. died; 64·28 per cent. were cured, and 7·14 per cent. remained doubtful. Whereas, of the 11 cases of the left side, 45·45 per cent. died, 36·36 got well, 18·18 were doubtful.

In other words, twice as many have got well from tapping the right as the left; and only half as many have had doubtful results from operations on the right, as in those where the left side has been tapped.

Hereafter, if my cases are any criterion wherefrom to judge, I shall regard an operation on the right side as much more favourable than one on the left; which I can hardly think would be the case were all right side pleurisies tuberculous.

*When shall we operate?*—Experience teaches me to operate in every case, however recent or chronic may be the attack, provided there is permanent or occasional dyspnœa of a severe character, evidently due to the fluid. I have of course more hope of doing good where the disease has not been of too long duration; is uncomplicated with phthisis, or any other disease, and where, moreover, the amount of fluid seems directly the cause of the trouble. I also deem it best to operate in *any*, even latent cases, where the pleural cavity gets full of fluid; and if, after a reasonable amount of treatment, the fluid does not diminish.

*Where shall we operate?*—The point originally chosen by Dr Wyman and myself, viz., in a line let fall from the lower angle of the scapula, and between the 9th and 10th ribs, I deem the most appropriate point at which to make a puncture. I have, however, tapped under the axilla, or in the breast, where the case seemed to require it. In selecting the precise intercostal space, on the back, I usually choose one about an inch and a half higher than the line on a level with the lowest point at which respiratory murmur can be heard in the healthy lung of the other pleural cavity.

I never wait until *pointing* commences; for then I am sure that pus will be found. If *pointing* without opening has commenced, I do not necessarily tap in that place, as recommended by the older surgeons, but seek the most depending point in the chest. While thus desiring to operate before a *local* distention shows itself, I dislike or refuse to tap where there is contraction of the intercostal muscles; and I am certain of getting fluid only where there is distention or flattening of the same.

*Objections to the operation.*—One word on the objections to the operation: "We may puncture the lung." "We may let air into the pleura." "We may by our strong suction injure the lung." "The instrument often fails to operate." "The connecting tube between the pump and the trocar collapses, and thus checks the flow of liquid." "The liquid may be too thick, and cannot be drawn through the small canula." "We may excite inflammation of the pleura." "The operation is useless, because non-tubercular cases will get well without; tubercular will die in spite of it," etc. etc.

One and all of these objections are to me, with the experience I have had, simply absurd. Let any man have good instruments and manage them skilfully on proper cases, and he will agree to the truth of what I state.

The operation, like everything else in all the departments of human life, is imperfect. It cannot cure all. But it has relieved many, and will continue to do so, if surgeons will use it; it has been the prominent cause of relief in many more, and will be so hereafter, if men will theorize less and act more. It has been the sole means of saving life, I am sure, in a few of my cases; and I know some patients have died within the last few years in New England, as I believe, for want of it, under the care of others.

It is certainly innocuous, and gives so little pain, compared with the relief that it affords, that patients have begged for it to be repeated again and again, as a mere matter of relief. In my opinion it ought never again to be allowed to fall into disuse by the profession. I regard any man who allows a patient to die of dyspnœa from pleuritic effusion, however great may be the complications with other diseases of head, chest, or abdomen, as in the dilemma of him who is either wilfully neglectful of some of the means of relief or cure, now by experience proved to be always at hand, or ignorant of the simple and beautiful operation suggested by Dr Wyman. To a certain extent I deem my connexion with the operation somewhat providential. I had seen, in the earlier years of my practice, men die with sudden dyspnœa, or, after months of obscure disease, die with one pleural cavity filled with serum, and not a particle of other disease; and, finally, I have seen tubercular phthisis follow, after months of debility, from what was simple pleurisy at first.

Having no surgical tastes myself, shrinking from the simplest operations, and doing nothing of the kind save when compelled to do so, I at times urged surgeons to operate. They declined, and men died. Finally, in cases where I had control, I took the responsibility, and asked the surgeons to do the manual they were more accustomed to than I was. Their plan was incision and dissection down to the pleura, and a suppurating wound as a consequence, a long, painful operation. At last Dr Wyman's instrument and method came to my notice. I seized upon them as those I had long sought for. As Dr Wyman and I were the only believers in the operation, it devolved often upon me. The result is the experience which I have given above. And now, as I have often said, I would as readily puncture the chest as I would draw a tooth or vaccinate a child.

### NOTE ON THE PATHOLOGY OF TERTIARY SYPHILIS. BY DR DIDAY, LYONS.

TERTIARY SYPHILIS should not be considered as one of the periods,—as one of the stages of evolution of a disease. It is a distinct pathological stage, which, by its clinical characters, its prognosis, and its treatment, may be sufficiently differentiated from the morbid condition, with which there would be the greatest probability of confounding it,—secondary syphilis. Thus, secondary syphilis can be produced at pleasure on any individual (supposing he have not attained immunity by a previous attack of the disease); its radical cure is the ordinary termination; finally, its lesions are contagious. With tertiary syphilis it is different; it is impossible to produce it at pleasure, or even to foresee the cases in which it will occur; in general it continues and perpetuates itself in spite of every remedy; finally, its lesions are not contagious. If we compare the two groups, the characters of which I have enumerated, it will be seen that the characters of secondary syphilis belong to the class of virulent affections, or innoculations by morbid secretions (such as vaccinia and the eruptive fevers), while those of tertiary syphilis belong to the class of diatheses (such as some skin diseases and rheumatism). Tertiary syphilis, in fact, is only syphilis which, from the condition of an intoxication, has become a diathesis,—which, from being essentially transitory, has become permanent. But as this transformation is very far from taking place in all syphilitic patients, the pathologist ought to inquire under what circumstances it takes place. Is it due to an excessive duration of the secondary period? By no means; syphilis sometimes pre-exists for three or four years under the form of simple secondary lesions, without on that account necessarily passing into the tertiary form. Is it due to the absence of specific treatment? Not at all. Independent observation has shown that tertiary syphilis occurs as often on omission as after the employment of specifics during the secondary stage. The true cause of the occurrence of tertiary syphilis,—the cause which entails it upon three out of twenty affected with the secondary disease,—is undoubtedly the variable strength of the original virus, but especially the variable degree of resistance which the organism of different patients opposes to the action of this virus. Individuals originally endowed with little vital resistance, or in whom this power of resistance has been enfeebled, whether by age or dyscrasiæ,—whether by privation of pure air, of proper food, of sleep, —whether by the effects of errors of regimen or the depressing passions,— these are the persons in whom we most often see syphilis assume the permanent form, and take on the characters of a diathesis. Experience, in verification of these rational conclusions, confirms, by the failure of medicines, what I have said regarding the special obstinacy of this condition. Mercury, in spite of its title of specific, fails almost invariably. Iodine, an admirable palliative, is no more than a palliative. To cure, if it be possible, a diathesis, requires not less than the prolonged action of all reconstituent influences; requires not less than a radical and durable change of alimentation, of residence, sometimes of profession; in short, of all the habitudes, whether social or moral. In such a change alone can we look for the secret of getting rid of the disease. This is what few patients can be brought to understand, and what still fewer can be induced to put in execution: all which is an additional cause of the rarity of radical cures of tertiary syphilis.—*Gazette Médical de Lyon.*

# MATERIA MEDICA AND THERAPEUTICS.

## CLINICAL REMARKS ON THE RELATIVE VALUE OF ACONITE, OPIUM, AND VERATRIA IN RELIEVING PAIN—ILLUSTRATIVE CASES. BY DR HANDFIELD JONES.

In reference to the following cases, Dr Handfield Jones remarked that the view which he was inclined to take of the action of the three remedies mentioned above, when applied locally, was, that *aconite* is most suitable to acute superficial pain, itching, and hyperæsthesia of the skin; *opium*, to pain of a more wearying, aching kind, more connected with debility, and requiring stimulating or tonic more than sedative treatment; and *veratria* to intermediate states, where a peculiar excitation of the nerve extremities seems to alter the morbid action of the affected nerve. More shortly the same may be put thus,—aconite is a benumber, opium a stimulant, veratria an alterant of nervous power. It is quite permissible, as is often done, to join opium with ammonia, chloroform, or turpentine, as these stimulants, if not in excessive amount, will rather promote the action of the opium. The other drugs should certainly be used alone. Belladonna is highly praised by the admirable Trousseau for its pain-subduing efficacy, especially in the case of external and superficial pains. He (Dr Jones) had had more experience of its internal than of its external use, but was inclined to look upon it as a sedative akin to aconite.

He would not omit offering a caution as to the use of aconite, which is certainly a most potent drug. It should not be applied to weakly subjects, especially to those who had any tendency to syncope, and care should be taken that the liquid should not come in contact with any excoriated or mucous surface, unless, indeed, it be much diluted. In the case of ordinarily healthy persons it is quite safe, and may be used freely to the unbroken skin. It seems, also, pretty certain that the presence of acute pain, or severe itching, or any like affection, renders its use internally and *à fortiori* externally much more safe, even in weakly persons, than it would be if the nervous system was quite tranquil. He had then a patient under his care who had been taking twelve minims of the pharmacopœia tincture daily for several weeks, on account of an obstinate spasmodic action of the muscles of one side of the neck. This quantity the patient bore well, and it benefited the spasms, but sixteen minims in the day caused some faintness. In tetanus this tolerance is very remarkable.

CASE 1.—A man, 62 years of age. He had prurigo, for which he first used an ointment of veratria—twelve grains to the ounce. This, however, made him feel "as if," he said, "he was in a burning furnace." Dr Jones then prescribed a lotion of aconite—one ounce of the tincture to two ounces of water. Under this and bichloride of mercury—one-twelfth of a grain three times a-day —he was cured.

2. A man, 48 years of age, had a lichenous eruption ensuing on a state of neurolysis. It was cured by the lotion of aconite.

3. A female, 31 years of age. In this case the patient had suffered from gall-stone for nine months, and had severe pain in the right side. The pain distinctly relieved by the aconite lotion.

4. A man, aged 35. A case of cutaneous hyperæsthesia, great itching, cured by the local application of the aconite lotion and the administration of carbonate of iron.

5. A female, about 30 years of age. Severe rheumatic pleurodynia. She received but little benefit from poultices sprinkled with laudanum, but was very notably relieved by aconite lotion. She took, also, iodide of potassium from the first.

6. A man, 36 years of age, suffered from neuralgia in the head and in both infra-orbital regions. It was worse in damp and cold weather. It was greatly benefited by the veratria ointment.

7. A woman, aged 31, suffered from menorrhagia and neuralgic pains in the temples and left side of the abdomen. The pain in this case was more relieved by opium liniment than by the veratria ointment.

8. A woman, aged 47. She had been subject to tic douloureux for four years, on taking cold. She had the most pain in the left side of her face on

eating. Exposure to cold air aggravated the pain very much. She had several decayed teeth, some of which had been drawn, without relief. She took quinine and iron, and applied the ointment of veratria with much benefit. She said the ointment caused a pricking sensation and lulled the pain.

9. A woman, aged 55. For about eight months she had a remittent neuralgic pain of the lower and outer part of the right leg. The veratria ointment alone at first gave her much relief. Afterwards, she took quinine and iron, and used a liniment of opium. She then took iodide of potassium, still using the liniment. She thought the opiate liniment gave her more ease than the veratria ointment.

10. A man, aged 30. He had had severe pains in his head, probably rheumatic. A node formed. He found that a lotion of equal parts of laudanum and water aggravated the pain, but that the aconite relieved it. Under the use of the iodide of potassium he quite recovered.

Linimentum opii is so commonly beneficial in the various aches and pains of weakly people, that it is not worth while to adduce any instances of its action alone.

In several cases of intense photophobia, evidencing the extremest retinal hyperæsthesia, Dr Jones has found the application of aconite lotion very beneficial, at least apparently, though it is not always easy to say how much of the improvement is owing to the lotion and how much to the tonics given at the same time.

The strength of the aconite lotion has been one part of the tincture to two parts of water; of the veratria ointment, ten grains to an ounce.—*Medical Times and Gazette.*

## ON THE ASSIMILATION OF THE LACTATE OF IRON, AND ITS SUPERIORITY OVER THE OTHER CHALYBEATES, IN REFERENCE TO DIGESTION. BY A. CORDIER, M.D.

IN order to appreciate the different preparations of iron, their action on the gastric juice has of late been the subject of frequent considerations. And rightly so, as it was to be determined, whether the gastric juice, by dissolving the chalybeates or combining with them, would not be found wanting in its proper place and time, the act of digestion. Meanwhile we have, like the majority of the profession, continued to prescribe the lactate of iron as the safest preparation in point of digestion.

Indeed, since 1839, when Messrs Gelis and Conté presented their first paper to the Imperial Academy of Medicine, the lactate of iron has risen continually in the favour of the profession; and as clinical observation has more than sufficiently proved its eminent value, we feel much satisfaction in supporting this opinion by some new physiological facts and experiments.

That the lactate of iron is the only chalybeate which can be prepared in the human body is easily proved by digesting for twelve hours, at a temperature of 104°, some iron filings, with distilled water and calf's rennet. Hydrogen is disengaged and lactate of iron formed, because lactic acid is contained in the gastric juice, and, in fact, is the acid which imparts to it its acidity. This has been proved to demonstration by Messrs Claude Bernard, Barreswill, Chevreul, Leuret, Lassaigne, and others.

Besides, Mr Bernard has shown that the lactate of iron can be injected into the veins in large quantity without producing any accidents,—an experiment speaking highly in favour of this preparation (Archives de Médecine, vol. xvi. page 87).

The lactate of iron readily combines itself with the albuminous fluids, and loses the property of being precipitated (Mitscherlick, Bernard). These combinations are readily assimilated, without fatiguing the stomach. Unlike other chalybeates, the lactate of iron, far from impeding digestion and weakening the appetite, rather strengthens them, as has been stated also by M. Bouilleaud, in his report on the lactate of iron to the Imperial Academy of Medicine.

Messrs Fouquier and Hardy, in a note annexed to this report, say:—"We have several times prescribed the pastils of lactate of iron in cases of chlorosis with amenorrhœa, and after three or four days there was always such an increase of appetite that the patients complained of the insufficiency of their diet," etc. etc. Experience has now sufficiently proved the excellence of Messrs Gelis and Conté's pastils in all cases where ferrugineous preparations are required, and also their superiority over other preparations where a weak or diseased stomach has to be taken into consideration. The reason of this is partly intelligible from the above remarks; but there are other proofs which we will presently consider.

In experimenting on the digestibility of medicines, M. Quevenne has found that a dose of thirty grains of reduced iron, or of the sesquioxide, even when taken at meals, produces diarrhœa and vomiting; while the lactate, administered eight times, in a dose of fifteen to thirty grains, produces no inconvenience. Thirty grains only appeared to affect the animals experimented upon. This difference appears to be due to the stomach having in the first case to dissolve and transform into lactates the above-named preparations of iron, while in the latter case it receives the lactate already formed. If the stomach of healthy animals is differently affected, how much more will this be the case in a convalescent or a chlorotic patient?

Recent experiments have given new support to the view which we hold of the preference to be given to the lactate over other less soluble preparations.

M. Felix Boudet undertook a series of experiments to ascertain whether the salts of iron which are precipitated on contact with the gastric juice require for their absorption any considerable quantity of this valuable and important liquid (Report on the therapeutic Use of Pyrophosphate of Iron, read before the Imperial Academy of Medicine, July 13, 1858, etc. etc.). He says,—"My experiments were made with the assistance of Messrs Robiquet, Corvisart, and Boudault, who themselves are occupied with a general work on the relation and reaction between the gastric juice and a large number of drugs. One drachm of fibrin and two and a half of fresh gastric juice of a dog being mixed and kept for six hours at a temperature of 104°, the fibrin is dissolved and completely transformed into albuminose. But if one introduces at the same time any substance antagonistic to the action of the gastric juice, the fibrin is not, or is only partially, digested. In order to ascertain the degree of digestion, Messrs Boudault and Corvisart apply three consecutive tests: 1*st*, Boiling; 2*d*, Bareswill's liquor; 3*d*, Glucose added to Bareswill's liquor. If digestion has been completed, the obtained produce does not coagulate at 212°, turns into deep violet when boiled with Bareswill's liquor, and prevents this liquor from being reduced by glucose. But if digestion has not taken place, the obtained produce is not turned into violet by Bareswill's liquor, and glucose readily reduces the liquor. In case of incomplete digestion, the above tests are less marked one way or the other. These tests have been applied by me to different compounds of iron, of which in each case I employed so much as to represent $\frac{3}{4}$ths of a grain of metallic iron. The result has been,—with lactate of iron, digestion complete, consequently no modification of the action of the gastric juice by its presence; with tartrate of iron and potash, citrate of iron, pyrophosphate of ammonia, digestion null; with $\frac{3}{20}$ths of a grain of reduced iron, complete; with $\frac{6}{20}$ths, incomplete digestion. Experiments with the pyrophosphates of iron and soda appear to have an equally paralyzing effect on the gastric juice. The same results had been obtained by Messrs Boudault and Corvisart in previous experiments. Chemistry and physiology have thus, besides clinical observations, testified to the superiority of the lactate of iron, which has long ago, and mainly through the pastils of Messrs Gelis and Conté, proved itself to be the most digestible of all ferrugineous preparations."

### DIGITALIS IN DELIRIUM TREMENS. BY W. KESTEVEN, F.R.C.S.

In a case of delirium tremens, which occurred to me a short time since, I administered three separate doses, one drachm each, of tincture of opium, without the least benefit, stimulants being freely given at the same time. Finding that no good result followed, I gave four doses, of half-a-drachm each, of tincture of digitalis. The patient continued still excited, not having closed his eyes for ninety hours. The relatives then requested a consultation.

Two hours after the last dose of tincture of digitalis had been given, Dr Ballard met me at the patient's bedside, when we found that he had just fallen into a quiet sleep. This lasted more or less completely for thirty-six hours, and the patient was apparently safe. He was a man of middle age and an habitual toper. It was his first attack. He had, however, a relapse a few days afterwards. I at once gave him two drachms of tincture of digitalis, to be followed every two hours by half-drachm doses. In less than three hours he was soundly asleep. Several weeks have since passed, and there has been no return of the malady. I would add, that in the next case of delirium tremens that falls to my lot I shall be disposed to begin with a larger dose than I gave in this case.—*Medical Times and Gazette.*

### ON THE PREPARATION OF THE HYPOPHOSPHITES.

I RECEIVED a few days ago the following communication from the head of the house of Swann & Co., the manufacturers of hypophosphite salts, and I think that the observations which accompany this gentleman's letter embody a considerable amount of practical interest:—

SIR,—You applied to us some time since for information with reference to our method of manufacturing the hypophosphites, remarking at the same time that these salts had not proved so successful in the hands of many other practitioners as they appeared to have been in the hands of Dr Churchill. I enclose you a few remarks on the subject which may tend to explain chemically the difference in the results.—I am, Sir, your obedient servant,

Laboratory, 12 Rue Castiglione, Paris, 7th Feb. 1863. H. H. SWANN.

"Since the year 1855, when Dr Churchill first presented to the French Academy of Medicine his 'Memoirs on the Treatment of Consumption by the Hypophosphites,' we have applied ourselves specially to the preparation of these salts, and consequently our experience on this subject has been very extensive. We are enabled to state that in almost every instance in which we have examined specimens of the hypophosphite of lime and soda, as usually employed, we have found these salts to be impure. The hypophosphite of soda has been alkaline in reaction, containing sometimes free alkali, not unfrequently carbonate of soda.

"The salt of lime has contained smaller or larger quantities of free lime, phosphate of lime, and occasionally carbonate of lime. Now, the salt recommended is a *neutral* salt, and the fact has been particularly insisted on that the value of these drugs depends upon the hypophosphorous acid, and not upon the base with which it is combined. In the preparation of our salts we follow the plan indicated in Dr Churchill's work, taking care to pass a current of carbonic acid gas through the hypophosphite solution in order to dissolve any free lime that may remain, and then by boiling the liquid we get rid of the excess of carbonic acid, and precipitate the carbonate of lime. This solution filtered will, if prepared with *pure* lime and *pure* phosphorus, be found to constitute a 'liquor calcis hypophosphitis' perfectly neutral, and free from extraneous ingredient. By means of the areometer the quantity of salt in a given measure of this liquid can be readily ascertained, and the result verified by evaporation to dryness, and weighing of the residue. The above remarks apply equally to the hypophosphite of soda, which we prepare by double decomposition, the dosed solution of lime being added in excess to a solution of carbonate of soda, care being taken that the alkali, which appears to be prejudicial to the therapeutical action of the hypophosphites, be entirely neutralized.

"We should recommend chemists to dispense from the dosed solutions of the hypophosphites, and not from the dry salts, the solutions being kept in small bottles in order to avoid the risk of decomposition by exposure to the air. We have found that the addition of sugar, provided it be perfectly free from lime, by cold maceration, prevents decomposition, and have now by us syrups prepared five years ago which are in a perfect state of preservation. The hypophosphites of quinine and manganese we prepare by means of hypophosphorous acid, obtained by decomposing the hypophosphite of lime by sulphuric acid." The salt produced with the last-named base is much used in Paris at present as a substitute for the various ferrugineous preparations, and is highly spoken of.—*Parisian Correspondent of Lancet.*

# Part Fourth.

## MEDICAL NEWS.

### CONVERSAZIONE OF THE ROYAL COLLEGE OF SURGEONS.

On Friday, the 27th of February, a conversazione of the Royal College of Surgeons was held in their hall, Nicolson Street. Upwards of 200 gentlemen were present, embracing representatives of the medical department of the navy and army, the Royal College of Physicians, and the Universities, as well as medical practitioners from all parts of Scotland. Among the company present were,—Dr Craigie, President of the Royal College of Physicians; Dr Currie, C.B., Deputy-Inspector-General of the Army; Dr Anderson, Deputy-Inspector-General; Professors Christison, Syme, Thomson, Simpson, Lister, Laycock, Bennett, Balfour; Dr Grant, Surgeon-Major; Dr Macbean, R.N.; Dr Lockwood, Surgeon-Major; Dr Graham, H.M.S. Edinburgh; Dr Seivewright, Staff-Surgeon; Dr Veale, R.A.; Dr Bruce, R.N.; Dr Rudd, Scots Greys; Dr Markey, 25th Regiment; Dr Stevens, Staff-Surgeon; Dr Wood, R.N., Staff-Surgeon; Dr Fleming, Glasgow; Dr Lyon, Glasgow; Dr Guthrie, Brechin; Dr Gray, Dundee; Dr Paterson, Glasgow; Dr Bonnar, Cupar; Dr Hamilton, Kelso; etc. etc.

Dr Newbigging, the President of the College, received the visiters in the library; after which they adjourned to the hall, where Professor Syme related the history of a case of excision of the scapula, and a lecture, on the present aspect of cellular pathology, was delivered by Mr William Turner.

The President, in opening the proceedings, remarked that no name was better known or more honoured in the science and practice of surgery than that of Professor Syme; while Mr Turner had already attained a high position in the medical school of Edinburgh as an original observer, and had gained a well-merited reputation for his minute and accurate scientific knowledge.

Professor Syme then detailed a case of

#### Excision of the Scapula.

Mr Syme said that, before proceeding to communicate the case which he had undertaken to relate, he might explain the circumstances which had led him to do so on the present occasion. Mr Turner had happened to mention that, in his lecture on cellular pathology, he intended to notice particularly a tumour which had been removed by himself (Mr Syme) some time ago, and, as the procedure might be regarded as new in the practice of surgery, Mr Syme had thought it might prove not uninteresting to give some account of the operation, and to show the patient who had been the subject of it.

Between two and three years ago, G. C., a man 43 years of age, applied to Mr Syme on account of a tumour connected with the right humerus. It was seated under the deltoid, was of no great size, and evidently proceeded from the bone. From the consistence of the growth, Mr Syme concluded that, if allowed to go on, it would soon attain such a size as to require amputation at the shoulder-joint. Wishing to save the arm, and at the same time get rid of the disease, Mr Syme determined to remove the head of the bone. An incision was accordingly carried downwards from the coracoid process, the head of the humerus was turned out, and the bone was sawn through a little below the surgical neck. Mr Syme might here remark that this proceeding was a new one. Cutting out the head of the humerus for caries was an established proceeding in surgery, but this was not the case as to performing the operation on account of morbid growths springing from the bone; from his experience, Mr Syme considered that it would prove very useful under the latter circumstance. Large as these growths ultimately became, they generally arose from the head of the humerus or its vicinity. The cast now exhibited was taken from the largest tumour of the kind which had probably ever been removed: it weighed fourteen pounds, and was removed by Mr Syme, by amputation at the shoulder-joint, twenty-seven years ago, from a man forty-five years of age. The man was still alive and well, and Mr Syme had heard of him quite recently. Had the case been seen earlier, it would no doubt have admitted of the less formidable proceeding of excision of the head of the bone, for it could be seen that the growth had arisen from the neighbourhood of the joint.

To return, however, to the original case. G. C., after the operation, made a good recovery, the wound healed quickly, and he went back to the country with a very useful arm. For twelve months he performed the duties of a small shopkeeper, occasionally travelling about the country in the course of his business. At the end of this time he noticed an enlargement of the upper part of the scapula, and soon after he came back to Mr Syme, who, not liking the appearance of the growth, advised him to return home. G. C. did so, but again returned with a very considerable tumour. Mr Syme could only offer to remove the arm along with the tumour. To this the patient objected, and again went home, but returned with the tumour so large and oppressive, that he expressed himself as willing to submit to anything which might be recommended for his relief. Mr Syme having in the meanwhile reconsidered the case, had made up his mind that, instead of removing the arm with the scapula, it would be possible to remove the scapula and leave the arm. Such a proceeding, it might be remarked, was not altogether new in Mr Syme's practice. Five years ago, he had, in the case of an old woman seventy years of age, removed the scapula alone on account of a tumour. The growth was very vascular, and pulsated almost exactly like a true aneurism. In this case the difficulty was greater than usual, on account of the nature of the tumour. Still, the patient did not lose much blood, she made a good recovery, and seemed likely to do well, but died about two months afterwards, rather, it would appear, from the gradual failure of her powers, due to her advanced age, than from anything connected with the operation. Having this favourable result before him, Mr Syme did not think there would be much danger in performing the operation in the case of G. C. Accordingly, a longitudinal incision was carried from above downwards over the posterior surface of the scapula, and another incision was carried across. The clavicle was divided at about a third of its length from its acromial end, both in order to facilitate the operation, and because this portion of the bone would have been useless after the scapula was removed. A cord was passed round the scapular end of the divided clavicle, and was pulled upwards by an assistant, so as to facilitate the detachment of the connections of the scapula. The cast exhibited showed the large size of the tumour, which was found to weigh between four and five pounds. It had commenced in the glenoid cavity, and consisted in great part of masses which extended towards the axilla. The operation was performed with very little loss of blood, and next day the patient was noticed to be using the arm at breakfast. There was no constitutional disturbance; the cure went on in the most satisfactory manner; and, three weeks after the operation, the patient was found writing a letter with the hand of the side operated on. Three months had elapsed since the operation, and constant improvement had taken place during that time. (The patient was here introduced to the meeting, and presented all the appearances of a man in vigorous health. The arm was about the same length as the other, neither shorter nor longer. He easily raised his hand to his mouth, and lifted a heavy mahogany chair from the ground; and, in illustration of the increased flexibility of the limb, he placed the point of his elbow under his chin. Professor Syme pointed out that the trapezius, although its scapular and clavicular attachments were gone, still acted upon the arm through the medium of its connexion with the cellular substance.)

Mr Syme was inclined to entertain a very favourable opinion of the operation. It might be observed that the occasions on which it might be required were by no means rare, for the scapula was a bone where tumours were apt to form. Mr Syme remembered one case in particular, which had occurred at a very early period, not of his professional career, but of his professional life. The case came under the care of Mr Liston, and was that of a boy, who had been dismissed from the Royal Infirmary as incurable, on account of a tumour of the scapula, in all probability very like that in the case just related. Mr Liston proceeded to remove the growth, which was very vascular, and bled furiously. A small portion of the tumour was left attached to the supra-spinous portion of the scapula. From this point the disease started afresh, the tumour grew rapidly and began to bleed. Mr Liston was naturally very anxious about the case, and wished to remove the scapula along with the arm, but he could not obtain the sanction of any surgeon in Edinburgh, and did not at that time feel warranted in performing such a serious operation on his own responsibility. The boy died; but if it had occurred to Mr Liston to remove the affected portion of the scapula alone, the patient might perhaps have been saved. Mr Liston, in despair, had applied to Dr Barclay to sanction the operation, but he refused to do so, because the operation involved cutting through the great subclavian vein at a point where it was very near the heart, and would not bear a ligature. Had the operation of excision of the scapula been performed, cutting through this vein would not have been required.

Mr Turner then delivered a lecture on the Present Aspects of Cellular Pathology, which will be found at page 873 of this number of the Journal. On the motion of Dr Craigie, seconded by Dr Currie, the hearty thanks of the meeting were voted to Professor Syme and Mr Turner.

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## MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION XLII.—MEETING VI.

*4th March* 1863.—JAMES SPENCE, Esq., *President*, in the Chair.

### I. PRIMITIVE FORM OF RUPTURE-TRUSS.

*Mr Edwards* showed a very simple form of truss which had been worn by one of his patients. A labourer had had a rupture for some time without knowing what it was; but he found that the swelling would go back into his belly. Accordingly, with the assistance of the neighbouring carpenter, he contrived an apparatus which was intended to keep the swelling permanently up. It consisted of two pieces of wood attached together: one end terminated in a sort of nob, which was padded with flannel, and was applied over the external ring. The apparatus was placed under the ordinary belt worn by labouring men, and was thereby kept in position. It had been worn for some time, and had answered pretty well.

### II. TRACHEOTOMY IN CROUP.

*Mr Edwards* showed a preparation, for which he was indebted to his friend Mr Tait. A child, fourteen months old, presented the symptoms of croup. Under the ordinary treatment it appeared to improve, but was attacked with such a severe paroxysm of dyspnœa that Mr Tait was obliged to make an opening in its trachea, just below the isthmus of the thyroid. Little benefit was derived from the operation; and this was not surprising, as, on post-mortem examination, the exudation was found to have extended to the bronchi, and many of them were almost completely plugged by it.

### III. EXCISION OF THE KNEE-JOINT.

*Mr Edwards* showed a knee-joint which he had excised two months before. The patient was a boy, and the circumstances under which the operation had been performed were by no means favourable. Amputation was out of the question, as the boy was much exhausted; and, in consequence of the irritation occasioned by the diseased knee-joint, an abscess had formed, which extended nearly to Poupart's ligament. The abscess had been opened, and was discharging at the time Mr Edwards first saw the patient. The knee-joint was accordingly excised, the patella being at the same time removed. The case had gone on well up to this time; the abscess had healed, and the general health was much improved.

### IV. ACUPRESSURE IN AMPUTATION OF THE ARM.

*Mr Edwards* showed an arm which he had removed a few days ago. The subject of the observation, a girl between fifteen and sixteen years old, had suffered from disease of the elbow-joint for three years. An operation had previously been suggested, but had not been assented to. Mr Edwards had not seen the patient till ten days ago, when he was requested to interfere, not so much on account of the old-standing disease as of a fracture. The bone had become so much atrophied and so fragile that in dressing the arm the humerus had given way, and there had been a great aggravation of the patient's sufferings. Though the condition of the patient was unfavourable, and there was a swelling resembling a chronic abscess over the chest, Mr Edwards had removed the limb immediately below the shoulder. All the arteries were secured by sewing-needles, which were removed in twenty-two hours. Whether in consequence of this proceeding or not, the wound healed almost entirely by the first intention; and the patient was able to take the air before the end of a week.

### V. EXCISION OF THE ELBOW-JOINT.

*Mr Edwards* showed an elbow-joint which he had removed from a girl on the previous day. The patient had suffered from the disease for two years, and her health had been much affected by the abundant suppuration in the neighbourhood. Mr Edwards had removed the bones, as he generally did, by two applications of the saw; the olecranon and head of the radius being removed by the one, and the condyles of the humerus by the other. As a little disease was perceived in the end of the humerus, another thin slice of that bone was removed, two vessels were compressed by needles, one of which was removed within four hours.

### VI. FRACTURE OF THE BASE OF THE SKULL.

*Dr P. H. Watson* showed a specimen of fracture of the base of the cranium, not because examples of this form of injury were rare, but because in this case the membrana tympani had been ruptured in a peculiar manner. There had been copious bleeding from the left ear; and as it was found after death that the line of fracture had not involved the external meatus, Dr Watson was anxious to discover from whence the hæmorrhage had proceeded. On dissection the membrana tympani was found ruptured, and the handle of the malleus was broken off quite close to its origin; and it was this which had occasioned the rupture of the membrane.

### VII. AMPUTATION.

*Mr Spence* read a paper on amputation. After some preliminary observations, the author stated that the object of the surgeon in amputation was to obtain a good useful stump with the least risk to life, and the question to be decided was, by what methods of operating were these results to be most surely and readily attained. Having from an early period of his professional life paid much attention to the subject of amputation, and having had a large experience as an operator, Mr Spence's remarks would be founded on the results of personal investigation and experience. It was necessary, in the first place, to decide as to a common standard, namely, as to what constituted a "good stump." The requisites were briefly these:—the bone must be well covered with soft parts, and must not be adherent to, or bear directly upon, the cicatrix; the nerves must have been so cut as not to become involved in the cicatrix, or attached to the end of the bone, and so deeply covered as to obviate the bad effects produced on their cut extremities by pressure or atmospheric changes. A stump possessing these requisites would be able to bear a considerable amount of direct pressure without becoming painful, irritable, or liable to ulceration. In deciding the question as to how the different modes of operating attained or fell short of this standard, Mr Spence would refer principally to amputation of the lower third of the thigh as typical of the operation. When Mr Spence entered the profession the two great methods of operating were by transfixion, and by the old circular method as modified by Allanson. In Edinburgh the former method, characterized by great rapidity in its performance, and strongly advocated by Liston and Syme, had gained such an ascendancy that Mr Spence had never seen the circular operation performed till he visited London, where he found that many of the principal hospital surgeons strongly supported the circular method, and denounced that by flaps as more dangerous to life, and ultimately leading to bad stumps. Mr Spence's own observation led him to the conclusion that the method by transfixion was the better of the two, although it did not possess all the advantages which had been claimed for it. The chief objections urged against the flap operation were three:—1. That the redundancy of muscular tissue retained was an immediate cause of danger; 2. That this same redundancy was useless as a covering to the bone, as it atrophied and ultimately disappeared; and, 3. That the manner in which the nerves were cut favoured the formation of neuromata. Mr Spence's subsequent experience had convinced him that the first of these objections had some force; whilst as to the other two he had endeavoured to satisfy himself by an examination and dissection of stumps. From these investigations Mr Spence had found, that whilst the muscular substance retained in flap operations certainly diminished in bulk, it did not in many cases do so to such an extent as might have been expected from *à priori* reasoning, but that in almost every case the muscular fibres, though frequently altered and condensed into a fibroid substance, could be traced as a thick layer under the skin. With regard to neuromata, or enlargements of the extremities of the divided nerves, Mr Spence had found that they were equally present in all forms of stumps, and even when the nerves were deeply covered; that, in fact, a greater or less enlargement of the end of the nerve was a necessary result of its division. A careful examination of these neuromata showed that they consisted simply of consolidated lymph deposited amongst the fibrillæ of the nerve; it was probable that the sheath of the nerve retracted more than the contained fibrillæ, which were separated from one another and stretched by the effusion of lymph. When such swellings were subjected to irritation from pressure or from atmospheric influences, by being too near the surface of the stump, or from being involved in the cicatrix, they apparently increased in size, and occasioned excessively painful symptoms. But, whether the stump had been a painful one or not, neuromata were found invariably present.

The ends of the bones forming the skeleton of the stump had their sawn extremities rounded off, and their medullary canals closed by the production of new bone; in general the bones were atrophied so as to appear unnaturally pointed, but were sometimes found generally or partially enlarged by the deposit of new osseous matter. In stumps of the forearm and leg the ends of the two bones were usually approximated and connected by a strong ligamentous substance; but in some cases they had coalesced so as to form an osseous arch. Another point in reference to the state of the bones after amputation was worthy of being alluded to. It had long ago been stated that the development of the bone after amputation in young persons was apt to give rise to projection of the cicatrix and conical stumps. Though there might, doubtless, be many other reasons for this as the persons grew up, Mr Spence's observations had satisfied him that the bone may continue to grow, and lead to such results. One very remarkable example of this was found in the case of an amputation of the upper arm of a child, performed by Mr Liston. Mr Spence examined the stump a number of years afterwards, and not only did the bone project in an extreme degree, but terminated in a hook-like process. The practical deduction was, to leave a redundancy of soft parts when amputating in the young.

Examination of stumps formed by the circular method had satisfied Mr Spence of its inferiority to the flap; for almost all the circular stumps were extremely conical and tender, the bone adhering to the cicatrix, which was not unfrequently ulcerated. At the same time many of the stumps formed by flap amputations were very defective, especially those where the operation had been performed by lateral flaps in the lower third of the thigh. In these the bone projected towards, and generally adhered to, the cicatrix at the anterior point of transfixion, whilst the soft parts, forming the flaps, hung back from it. In various cases the soft parts were so retracted as to cause the stumps to resemble those obtained by the circular method; whilst in others the flaps seemed to have turned in upon themselves, leaving a deep cleft corresponding to the cicatrix, which was closely attached to the bone. Still, on the whole, the stumps obtained by the flap were decidedly better than those which resulted from the circular method, and their defects did not appear due to causes inherent in the mode of operating.

In Mr Spence's earlier operations he adopted the method by anterior and posterior flaps formed by transfixion, as recommended by Mr Liston, the posterior one being considerably the larger, to allow for the greater retraction of the posterior muscles. The result was two rounded flaps of equal length, the end of the bone being a little higher than their bases. Nothing could look better at the end of the operation than a flap so formed,—the smoothly-cut flaps came neatly together, covered the bone thickly, and formed a well-rounded stump. Practical experience, however, soon showed that it would not do to trust entirely to the length of the flaps, as they had a tendency to retract and contract gradually to the point of transfixion, and if cut very long, to contract and turn in upon themselves, leaving a deep cleft in the cicatrix, so that the soft cushion did not correspond to or cover the end of the bone. To remedy this defect, Mr Spence's first modification consisted in transfixing the limb fully two inches lower than the point where he intended sawing through the bone; having cut and retracted the flaps, the point of the knife was passed round the bone so as to clean and expose it, the thigh was raised so as to cause the bone to project, and the soft parts were fully retracted before the saw was applied. The bone was thus cut through high above the point of transfixion, and even after permanent retraction of the flaps the bone remained deeply covered. A still further modification consisted in cutting the anterior flap longer, and the posterior shorter; and this Mr Spence was led to by a remark of Mr Liston's in speaking of the advantages of antero-posterior flaps in the thigh, "for then the more the stump is raised, the better is the end of the bone covered—the anterior flap folds over it." Mr Spence's dissections had shown him that the anterior flap never fully covered the end of the bone, but that the bone corresponded to the cicatrix, which was retracted in the centre of the stump; and it was obvious that, by cutting the anterior flap rather longer than the posterior, the former would "fold over" the end of the bone, and the cicatrix would be posterior to the centre of the stump. Another advantage anticipated was greater simplicity in the dressing and after-treatment, from the diminished bulk and weight of the posterior flap. Mr Spence continued to practise this modified operation, and to teach it in his surgical lectures, till a recent period, and the results he obtained from it were very satisfactory. This operation, however, required great care both at the time and during the after-treatment; and there were special difficulties under two circumstances of not unfrequent occurrence: first, in primary amputations; and, second, in amputating through condensed textures, as in the vicinity of necrosed bone. In the former, when amputation was performed in a powerful man, it was almost impossible to avoid such a redundancy of muscle as to make the approximation of the integuments difficult, and also to render the section of the bone high up not very easily accomplished. In the latter class of cases, the dense tissues forming the flaps could not be retracted so as to allow the bone to be cleared or sawn through high above the point of transfixion. To get rid of the redundant muscle in primary amputation, especially in amputation below the knee, Mr Spence used to slice off a portion of the muscle after the flap was formed, and thus, by a somewhat awkward-looking procedure, secured a better stump.

In 1846, Mr Syme, whose writings and practice had done so much to introduce and promote flap amputations, announced that his experience had led him to modify his opinion as to the advantages of flap amputation, and to recommend in the lower third of the thigh a modification of the circular method, which consisted in making two semilunar incisions through the integuments, which were dissected and drawn upwards for at least two inches, cutting the muscles on a level with the retracted skin obliquely up towards the bone, the muscles in the posterior aspect being divided somewhat lower down than those in front, all the soft textures being next forcibly retracted, and the bone cleaned and sawn through high up. This plan, when well carried out, gave excellent results, and was especially applicable to primary amputations. In such cases,—though at first rather prejudiced against the operation,—Mr Spence had performed it repeatedly, and in these, at least, he had no hesitation in stating that it was superior to the double flap operation by transfixion, as involving less danger to life, and leaving a better stump. In several other cases Mr Spence had also practised the ordinary circular operation. While thus recognising the advantages of the circular or modified circular method in primary and some other amputations, Mr Spence continued to employ the double flap operation, with the modifications already described, in ordinary cases of disease of the knee when the muscles were not redundant; because he found that, unless there was a considerable amount of subcutaneous fat, there was not by the circular method so good a covering of the bone; for, from the way in which the muscles were divided, they retracted, and did not heal over the bone, so that we were obliged to depend entirely upon the skin and subcutaneous tissue for the ultimate covering. Mr Syme, indeed, in the paper referred to, expressed the opinion that there was a fallacy in regard to preserving muscular tissue, and that the proper covering ought to be skin,—referring, in proof of this, to the excellent stump obtained in the ankle-joint operation, where the end of the bone was covered only with integument. In this, however, there was a fallacy; for the protective covering in the ankle-joint operation was altogether peculiar, consisting as it did, not only of skin, but of a peculiar elastic cushion of fat and fibrous tissue, specially adapted by nature for bearing direct pressure. Could we, indeed, obtain such a covering elsewhere, it would undoubtedly be superior to any other, but unfortunately in no other amputation could it be obtained; and as his dissections had shown Mr Spence that the muscular tissue preserved did form a more or less thick protective covering, he still thought that, if possible, it should be preserved in moderate quantity.

In 1858, Mr Teale of Leeds published an account of his method of amputating by a long and short rectangular flap. In contrasting his method with the others in use, Mr Teale said, in reference to stumps formed by other modes of procedure than his own, that it was extremely rare to find a soft movable mass of tissues over the end of the bone, that with few exceptions the cicatrix was adherent to the bone, and that such stumps could not bear pressure on their extremities. Such a sweeping condemnation (opposed as it was to the results of Mr Spence's experience) of other methods of amputating, had a tendency to raise a suspicion that the advantages of the new plan were exaggerated. According to Mr Teale's method, the size of the long flap was determined by the size of the limb at the place of amputation, its length and breadth being each equal to half the circumference of the limb; it was therefore a perfect square, and long enough to fall easily over the end of the bone. In selecting the structures for its formation, such parts must be taken as did not contain the larger bloodvessels and nerves. A flap so formed would generally be anterior in position as regarded the general direction of the body, but superior, when the patient was in the recumbent position, as during the after-treatment. The short flap, containing the chief vessels and nerves, was one-fourth the length of the other. The flaps having been formed, the bone sawn, and the arteries tied, the long flap was folded over the end of the bone, and the wound having been brought together by sutures, the transverse line of union was bounded at each end by a short lateral line at right angles to it. From this description it would be seen that the operation was essentially amputation by a long anterior flap, for the very short posterior flap was little more than equivalent to a circular sweep through the posterior tissues of the limb.

Amputation by a long anterior flap, and a short posterior flap formed by the circular division of the soft parts, could scarcely be called a new operation, for such a method of amputating the thigh was recommended and carefully described in 1765 by Mr O'Halloran of Limerick in the following words:—"With a straight knife commence your flap incision at about three inches more anteriorly (lower down ?) than where the bone is to be sawn through, supposing an adult, and so in proportion to the age and size of the limb. Let the incision be continued in an oblique direction till you reach the bone; then with the same or a common amputating knife make your circular sweep, commencing from the side of your flap directly to the bone." The after-treatment, however, was very peculiar. Mr O'Halloran inserted a piece of lint between the bone and flap, and dressed the surfaces separately for ten or twelve days before approximating them; and this doubtless led to the method falling into disuse. In 1848, this method was revived in France by M. Sedillot and M. Baudens. On comparing Mr Teale's plan with that of O'Halloran, it would be seen that the chief difference was the much greater length of the flap in the former than in the latter. For instance, in a thigh twelve inches in circumference, Mr Teale's anterior flap would be fully six inches long, whilst O'Halloran says that in operating on the adult the length of the anterior flap should be three inches. It was evident that a flap of the latter dimensions would be quite inadequate; for, taking the limb as nearly a circle, the flap would be one inch less than the diameter, or the surface it was required to cover, and this without allowing for vital retraction or contraction of the flaps. Mr Spence could not, therefore, help thinking that O'Halloran had not measured his flaps, but had judged of their proportion by the eye, otherwise the stumps would have been very imperfectly covered. On the other hand, the length of the anterior flap required in Mr Teale's method was extreme, for, to allow the flap to fold easily on itself where there was much tissue, its length positively required to be more than half the circumference of the limb. After performing several amputations of the thigh by Mr Teale's method, and obtaining excellent stumps, it seemed to Mr Spence that its chief advantage consisted in the end of the bone being completely covered by the sound tissue of the anterior flap, whilst the cicatrix was placed high up on the posterior part of the stump; but that the square form and the folding of the long flap were disadvantages, as leading to trouble in adjusting the flap and in the after-dressing, should union by the first intention fail, whilst the folding the flap on itself, and so retaining it by stitches, was likely to endanger its vitality to some extent. It seemed, therefore, to Mr Spence that he might obtain the advantages without the disadvantages of Mr Teale's method, by reverting to a modification of O'Halloran's operation, and simply cutting a large, slightly-rounded flap from the anterior aspect of the thigh, dividing the soft parts posteriorly by a circular sweep of the knife, so that, when the bone was sawn through, the anterior flap would fall upon and adjust itself to the section of the limb. This plan had accordingly been put in practice, and found to answer so fully as regarded the form of stump and the simplicity of the dressing and after-treatment, that Mr Spence now constantly adopted it as a general method in amputation of the lower part of the thigh. For the proper performance of this operation, some particulars required to be attended to as regarded the size, form, and textures to be preserved in the anterior flap, and the section of the bone. In order that the anterior flap might fall loosely over and cover the posterior segment of the stump, its breadth in its whole extent must be fully equal to one-half the circumference of the limb, and it must be gently rounded at its extremity, so as to adjust itself readily to the curve of the cut margins of the posterior half of the stump. In Mr Spence's own operations he judged of the proportions by the eye; but a flap of four inches would be found sufficient in a limb twelve inches in circumference, provided due attention were paid to preparing the bone for section, by elevating the thigh somewhat towards the pelvis, retracting the soft parts, and clearing the bone fully two inches higher up than the base of the flap. The bone was thus projected to the utmost, and if sawn close to the soft parts, when the limb was lowered it was deeply buried in the anterior flap, which hung loosely over it. As to the textures composing the flap, Mr Spence generally operated as low down as possible, so that the lower part of the flap was chiefly composed of skin, but he cut obliquely through the anterior muscles to the bone, so as to have a thicker cushion directly corresponding to the sawn end of the bone. The great vessels must be left in the posterior part of the stump, and not included in the flap.

This mode of operation, when applied in operations at the lower part of the thigh, seemed to Mr Spence preferable to any other. Mr Spence had observed that his former colleague, Professor Lister of Glasgow, in a paper on amputation in Holme's System of Surgery, in speaking of this plan of operating, whilst admitting some of its advantages, seemed to think that it involved the necessity of amputating higher up in the thigh, in order to procure a sufficiently long anterior flap. But what Mr Spence considered one of the great advantages of the operation was, that it enabled him to amputate lower in the thigh than by double flap, for as that part of the flap which was to be behind the bone did not require to contain muscular tissue, he could cut the flap as low as the upper margin of the patella, and, as the bone was projected and cleared for about two inches above the base of the flap before sawing, he seldom required in the adult a flap more than from four to five inches in length. The plan, recommended by Professor Lister, of an anterior flap, somewhat longer than usual, and a shorter posterior flap, was in fact the modification of Liston's double flap operation, which Mr Spence had long practised and taught, and which, as before stated, he still preferred in certain situations, but which, after considerable experience of both methods, he had abandoned in favour of the single anterior flap in amputating at the lower third of the thigh.

Though the single flap might be adopted in other amputations, as the arm and thigh, it lost some of its advantages. From the form of the limb, and its position when the patient was lying in bed, the flap did not adapt itself by its own form and weight to the cut surface, but required to be maintained in position.

In these situations, a modification of the operation answered sufficiently well. In the upper arm, a single flap was taken from the outer and anterior portion of the limb, the soft parts on the inner aspect being divided circularly, on a level with the base of the flap; a stump so formed possessed the advantage that the great nerves were divided high up, and were removed from the end of the bone which was still covered by soft parts, while the cicatrix was placed high upon the inner side beyond all risk of pressure. In amputation of the leg below the knee for disease, or where the muscles were atrophied, the long posterior flap brought forward over the ends of the bones formed an excellent stump, though care was required both in operating and in the after-dressing; but in primary amputations, the mass of muscular substance was a great objection to that method. Hitherto Mr Spence had principally used the modified circular method of Syme in this situation, and thought it on the whole the best; still the form of the tibia and the sharp edge it presented, however sawn, was apt to cause ulceration of the anterior flap, especially in muscular individuals, in whom there was no great amount of subcutaneous fatty tissue. The long anterior flap recommended by Teale was still more objectionable, as its weight dragged it over and pressed it against the sharp edge of the tibia; whilst the length of such a thin flap of skin created a danger of sloughing, and besides could seldom be obtained in cases of injury requiring this amputation. Mr Spence had lately conceived the plan of operating here by a long flap from the internal aspect of the leg, and letting the stump rest during the subsequent treatment on its outside, as in the case of fractures of the leg, so that the flap fell by its own weight over the face of the stump; he had recently tried this method in one case, but a part of the anterior edge of the flap sloughed, and although the result would probably be a very good stump, the method did not answer as in the thigh, and besides it was very rare to meet with a case which would admit of its performance. In Mr Spence's opinion, amputation of the leg in primary cases was of all others the most unsatisfactory, owing to the form of the tibia, and the unequal manner in which the muscular tissue was disposed in relation to the bones; and in Mr Spence's experience, the circular or modified circular operation was, as already stated, the most suitable.

Mr Spence would say in conclusion, that though convinced of the advantages of the method by anterior single flap, he would never amputate higher up the limb in order simply to obtain a single flap; indeed, in many cases it would be inadmissible, for example in amputating for malignant tumours, where we must avoid including the soft parts over the tumour in the flap, and where we should remove as much of the muscular tissue as possible so as to be wide of the disease. The modified circular method by two flaps, composed chiefly of integument, would, under these circumstances, be found preferable, and the flaps could be so cut that the anterior might be sufficiently long to allow the cicatrix to be behind the centre of the stump, as shown in one of the casts before the Society. Finally, whatever method of amputation might be adopted, the great point in order to secure a good stump, was to project and clear the bone, at least two inches above the point exposed by the incisions, so as to be deeply covered by parts not liable to further retraction. Mr Spence would take another opportunity of communicating to the Society his experience regarding the results and especially the mortality of amputations. (Mr Spence's paper was illustrated by numerous casts, preparations, and dissections of stumps).

*Dr Handyside* had listened with much interest to Mr Spence's paper. There was no subject on which it was more important for the practical surgeon to make up his mind, than as to the method of amputating both in primary cases, and in cases of long standing disease. Mr Spence's paper enabled us to proceed from certain data to a consideration of the principles on which the operation should be performed. There had been ample reasons for abandoning some of the older methods, still the history of amputation should be examined before changing from one plan to another, as some of the best surgeons had returned to methods they had formerly given up. When Dr Handyside entered upon the study of his profession, the flap operation was the chief one practised in Edinburgh; while, in the provincial hospitals of England, in London, and abroad, the circular method was almost universally practised. Dr H. felt that the grounds advanced by Mr Syme, in his paper of 1846, were such as to lead us to be slow to abandon absolutely a method which had been previously in repute. There were three points of special practical importance in Mr Spence's paper:—*First*, As regarded the stump; it was not only essential to have the bone well covered with soft parts, but it was very important to have an adequate covering of skin, and to have the cicatrix in such a situation that it should not be pressed upon when the patient was using the limb. Mr Spence's plan certainly seemed an improvement on those previously in use, and appeared to combine the advantages of Mr Teale's, the circular and the single and double flap operations. *Second*, As to the importance of preventing neuromata; Dr Handyside had been under the impression that these were generally ganglionic enlargements upon the ends of nerves. He remembered having had sent to him the re-united nerves from the fore-legs of a horse, which had been nerved for disease of the coffin bone. In one, there was a ganglionic enlargement at the union, with a few fibrils traversing it; in the other, there was an intertwining of nerve fibres with intermediate fibrous tissue. The practical point in Mr Spence's paper under this head, was the importance of cutting the nerves short. *Third*, As to the advantage of sawing through the bone high up by raising the limb.

There were, however, two points on which Dr Handyside desiderated information. The first of these was a statement of Mr Spence's experience as to the mortality after the operation; and, in the second place, he regretted that no allusion had been made to acupressure. Dr Handyside was surprised that acupressure, which presented so many advantages and so few disadvantages, had not been more extensively taken up, especially by hospital surgeons, who had such abundant opportunities of testing its merits.

*Professor Simpson* would ask one question. Mr Spence had stated that Mr Syme had in some cases reverted to the circular method, and, that since then, his results had been more fortunate than by the flap operation,—he would like to know on what that better success was founded? Dr Simpson also referred to Dr Peacock's statistics, which showed that, at the time they were published, the mortality after amputations in the Edinburgh Infirmary was very large.

*Mr Spence*, in speaking of Mr Syme's experience, had quoted his own words; but he believed that the less redundancy of muscle had had a considerable share in diminishing the mortality after amputation. Mr Spence had not yet made up his own results, but he had no doubt that the statistics he could refer to would bear this out. He had a very great objection to statistics brought together indiscriminately, because we could not know how the operation had been performed, nor the various circumstances attending the particular cases. In a future communication he would confine himself entirely to the results of his own observations.

*Professor Simpson* had no doubt there had been a great diminution in the mortality since the publication of Dr Peacock's statistics, and he had no doubt that one great cause of this had been the introduction of chloroform into surgical practice.

*Mr Edwards* said it was very interesting to receive the statements of surgeons who had themselves witnessed the changes in the methods of operating. It seemed to him that in former years the great object had been rapidity. We had heard of Liston, John Bell, and other great surgeons, whipping off legs while people stood by them with stop-watches in their hands; when the merit of the operator was judged by the number of seconds in which he performed an amputation. In this respect there had been a great change; the surgeon now proceeded leisurely, and carefully dressed the stump before the patient left the operating table. Was it not possible that the great rapidity aimed at in former times was dangerous to the patient? Might not the violent sawing of the bone have been dangerous by causing it to splinter? Then, as Mr Spence had mentioned in the flap operations, the flesh was apt to crop out in stout patients, and it was difficult to bring the two surfaces of the stump well together. Mr Edwards thought that Mr Spence's operation was an excellent one for the lower third of the thigh; his great objection to it was that it was not adapted for other situations, and that the circumstances under which it could be employed were exceptional, and required division of the bone higher up than in the ordinary method. Dr Handyside had alluded to various methods of stopping bleeding. Every one who had seen Mr Spence operate must have observed the great care which he took to stop the whole bleeding. Mr Edwards, till he came to Edinburgh, had not been accustomed to see so much pains taken in this respect, such lingering over the flaps. It was certainly common elsewhere to be obliged to take down stumps in order to remove clots, or tie arteries, which had been overlooked at the time of the operation. As Dr Handyside had alluded to arresting hæmorrhage by acupressure, Mr Edwards would take the opportunity of saying that the last method introduced by Dr Simpson, of passing a sewing needle below the artery and a loop of wire round it, was very easily applied and perfectly reliable. At first he had left his patients with some doubt, but now he had secured even large arteries by this means, and had found it perfectly safe. Mr Edwards had secured in this way, and with perfect success, the axillary, the tibials, and other large arteries. The other day, a patient came to Mr Edwards's own house to have his elbow-joint excised, in consequence of the limb being anchylosed in the straight position. The parts were a good deal altered in consequence of the original injury received, and in performing the operation, a large artery, probably the brachial, was wounded, and bled freely. A needle was placed under it, and the hæmorrhage was immediately and permanently checked. The other day, after amputating an arm, which was very vascular, Professor Simpson had put in needles and wire: one round the brachial artery, others around various smaller vessels; the needles were all removed in twenty-two hours, and there had not been the slightest hæmorrhage.

*Dr P. H. Watson* observed that the question before the Society was not as to the mortality after amputations, as to the effects of the inhalation of chloroform, nor as to the mode of stopping bleeding, but it was purely one of operative surgery, namely, as to how a stump could best be formed. In considering this subject Mr Spence had referred especially to amputation of the lower third of the thigh, and had brought the method he proposed to what might be called an *experimentum crucis:* And that this was the situation where the greatest difficulty had always occurred in performing amputation so as to obtain a good stump, and when therefore the decision as to the best mode of operating was most important was obvious from two facts; first, the frequency with which new methods of amputating in this region had been introduced, having as their object the removal of the limb as low as possible; and, second, that when the operation of amputation in the lower part of the thigh was performed by transfixion, it must have been observed, as it furthermore appeared from the drawings in the various text-books (including Mr Liston's), that it was in reality performed in the middle third, and often pretty high up in it, so as to admit of such retraction as would secure a good stump. The principle of operation by a single flap was not in itself new; amputation at the hip-joint was generally performed by a single anterior flap, if there was a posterior flap it was short. In the same way, in the case of the shoulder-joint, a single flap was generally preferred, obtained chiefly from the external surface of the limb. In some injuries also, where there was more interference with the soft parts on one side of the limb than on the other, and especially high up in the thigh, such an operation by single flap was frequent. But in Mr Spence's paper it was not so much the mode of amputating by a single flap which was new, as the principle on which the practice was founded. The principle seemed to be that while the bone could be divided lower than by any other method of operating on the lower third of the thigh, its covering should consist not merely of cutaneous but also of muscular tissue, and that the single flap by which this was effected should be long enough to go quite round the end of the stump, so as to have the cicatrix situated posteriorly. Mr Spence did not, in Dr Watson's opinion, do himself justice in calling his method of amputating a modification of Mr Teale's, it was in reality a new operation. No doubt the part best suited for this operation was the lower third of the thigh, but it was not on that account the less but rather the more satisfactory, because it was precisely here that the greatest difficulty had always been felt. Certainly, from what he had seen in Mr Spence's hands, as well as in his own cases, he was satisfied that by no other means could amputation be performed so low in the thigh and with such good results, immediate and ultimate. Mr Spence had spoken of the application of his own and of Mr Teale's method to amputations of the forearm and leg; but they were not so well suited for these limbs, which being conical, and so becoming smaller towards their lower extremity, did not admit of a flap of uniform width being formed from them with facility; and in the case of the fore-arm, as the operation was generally resorted to on account of injury, where we were limited as to the length of the flap, and where it was very desirable to have the stump as long as possible, a double cutaneous flap operation would generally be preferred.

*Mr Spence* had but little to say by way of reply; he could scarcely call his method new, for O'Halloran's, with the exception of his absurd plan of after-treatment, was almost identical with it. The principle had been long lost sight of, and it was through the medium of Mr Teale's operation that Mr Spence had been brought to it. It was quite true, in the case of the forearm, that we could not generally get the kind of flap required, but years ago he (Mr Spence) had obtained the same result in a case of injury. From the nature of the injury he had been obliged to take the flap from behind; the result looked bad at the time, but when the wound healed it made a good stump with the cicatrix in front. He thought at the time that he had made the best of a bad job; but he now saw that he had not at that time recognised the true principle. Since then he had performed his modified operation in the upper extremity several times, and the result had been satisfactory. Mr Edwards had objected that the operation must be of very limited application; but the fact was, that the cases in which it could be applied constituted the rule, those in which it could not being exceptional. It could be performed in almost all cases of disease of the knee-joint, for the presence of one or two sinuses was no objection; and one great advantage was that it could be performed lower down than Liston's method. With regard to the tying of vessels, Mr Spence could assume no greater credit to himself than was due to Edinburgh surgeons generally. No one had been more particular in this respect than Mr Liston, and though the flap amputation was performed very rapidly, the patient was actually longer on the table in consequence of the greater number of vessels which required to be tied after that operation,—in consequence of the greater amount of tissue saved. Mr Spence had seen as many as eighteen vessels tied after a flap amputation of the thigh, and had himself tied thirteen. Mr Spence did not attach much importance to its being occasionally necessary to open up stumps on account of bleeding, for, under any method, reactionary hæmorrhage might occur. Although the mode of amputating recommended by Mr Spence generally occupied somewhat longer time than the flap operation, it might, if necessary, be performed quite as rapidly. He had lately performed the primary operation on a boy who was in a state of extreme exhaustion from the effects of the injury and loss of blood, and where it had been of the greatest consequence to operate as rapidly as possible. Still it was better to operate rather slowly, as the flap was then better shaped, and fitted more accurately afterwards.

### VIII. GANGRENE OF THE LEG DEPENDENT UPON ARTERIAL OBSTRUCTION.

*Mr Annandale* read a paper on this subject, which will be found at page 904 of the present number of the Journal.

*Mr Spence* regretted that the report of so interesting a case should have been delayed till such a late hour; there were, however, some points which required to be carefully looked into. The preparation exhibited reminded him (Mr Spence) of one in his own possession, where the brachial artery was obstructed by a long fibrinous clot; the clot passed up into the axillary artery, but was not adherent to its walls, and in that situation was evidently secondary. Mr Spence was of opinion that in Mr Annandale's case the clot in the femoral artery was primary, while that in the aorta was secondary. With regard to the fact of blood coagulating where the vitality of a vessel was diminished, this had been long ago pointed out by Sir Charles Bell. He showed that the mere passing of a ligature loosely around a vessel delayed the current of blood in consequence of the vitality of the vessel having been lowered by having been separated from its attachments, and he explained in this way the occurrence of gangrene from injury where there was no rupture of the bloodvessels. With regard to the statement that inflammation of the inner coat of an artery could not take place, but must always be external to it, Mr Spence had a preparation, for which he was indebted to Dr James Duncan, where, in a puerperal case, gangrene of a limb took place, there was not only a fibrinous clot but an evident exudation from the interior of the vessel. In Mr Spence's own experiments he had shown that an exudation took place not only from the cut part, but from the inner coat of the vessel a quarter of an inch higher up, and here we must carefully distinguish the blood clot and the plastic clot. If the carotid was laid bare, and two ligatures were applied half an inch apart, and the intervening portion completely emptied of blood by squeezing from below upwards, it would be found twenty-four hours afterwards that in this situation the canal of the vessel was obstructed. Whence could the obstructing material have come but from the inner coat of the vessel? Certainly from what he (Mr S.) had himself seen he was compelled to differ from some of the pathological teaching of the present day. He saw nothing to make him doubt that there might be effusion from the inner coat of an artery as the result of inflammation. A peculiarity in Mr Annandale's case was the rapidity with which the symptoms had set in. Such, however, was possible, although the obstruction was due to arteritis, as the process might have been going on without being manifested by symptoms for some little time. Mr Spence would not deny that it might have been a case of embolism; at the same time, there were so many sources of possible fallacy that it was necessary to examine the case in every light.

*Dr P. H. Watson*, from an examination of the preparation exhibited, and from the account of the case given by Mr Annandale, thought it reasonable to believe that the clot across the bifurcation of the aorta was secondary, and was the result of the diminished force of the arterial current produced by the obstruction lower down. The absence of disease of the heart was no argument against the formation of the clot in that organ or some of the larger vessels, and he had little doubt from the history that the case had really been one of embolism. The occurrence of gangrene was probably owing rather to the age of the patient and the diseased condition of the collateral vessels which should become enlarged than to anything special in the situation of the clot, for embolism rarely produced gangrene when it occurred in young persons. A pupil of Dr Watson's, who had no cardiac disease, was recovering from enteric fever, and was doing well, when one afternoon he complained of intense pain in the groin; when seen by Dr Watson the foot was livid, and no pulse was perceptible between the groin and the foot. The anastomotic circulation was, however, gradually established, the patient recovered, and now no bad effects remained. The other day, he had seen a patient who had no cardiac disease suffering apparently from the effects of embolism of the innominate artery; the symptoms at the time of the obstruction were of an apoplectic character, and there was a resulting hemiplegia of the side opposite to that on which the artery was obstructed. The patient was now suffering from the symptoms of softening of the brain; and at present, on examining the vessels of the right side of the neck and arm, there was only faint pulsation perceptible in the axillary artery, none in the brachial or radial, and a faint undulation in the carotid, which contrasted in a marked degree with the forcible pulse upon the opposite side. Dr Watson had lately seen a case where without cardiac disease the axillary artery of an individual well up in years had been obstructed by an embolism; the limb had threatened to become gangrenous, but the unfavourable symptoms had disappeared, and the patient was now going on well. Dr Watson considered that when obstruction took place, the circumstances which determined the occurrence of gangrene were the general state of the patient, the condition of the collateral vessels, and the extent of the embolism, rather than its site in the vessel; at the same time gangrene was more liable to occur in cases of obstruction of the vessels of the lower than of the upper extremity.

*Dr Handyside* observed that a morbid state of the arterial system might exist without producing any bad effects, and referred to an interesting paper by the late Mr Turner,[1] on the production of gangrene by a rupture of the inner coat of the artery in middle and advanced life, and due to violence direct or indirect. Shortly before the death of the late Dr Abercrombie, Dr Handyside had examined with him the body of a woman who had died from gangrene of the arm. The woman had been wringing blankets, and in doing so had doubtless been twisting her arm, and in so doing had stretched and compressed the humeral artery. On examination, the brachial artery was found obstructed, owing to the curling up of its inner coat, and a clot of a pyramidal form was found with its base directed downwards; the remainder of the arterial system was not in a thoroughly healthy condition. The other organs were healthy, and it was possible that, but for the violence to which the brachial artery had been subjected, gangrene would not have occurred. These remarks, in connexion with the observations of the former speakers, would show how complicated was the subject of obstruction of the arteries, and to how many various causes in particular cases it might be due.

## THE ROYAL EDINBURGH HOSPITAL FOR SICK CHILDREN.

It is so short a time since this charity was established in Edinburgh, that the announcement of a new hospital is somewhat startling. In 1859, when the promoters of this now firmly-settled institution were labouring to convince the public of the present urgency for its commencement and to secure for it substantial pecuniary support, many objections were urged and somewhat extensively entertained against it. All these objectors have now disappeared with their objections. The latter were, indeed, not altogether groundless and trivial, but may be passed over with the remark that they were equally applicable to all hospitals; and, in these days, the objections to hospitals, good and true though the objections be, are well known, but are freely admitted to be far more than counterbalanced by the paramount and overpowering recommendations of their beneficence. The good they do immensely surpasses the little evil they may often produce. To many, the strongest reason for not then beginning a new hospital was the fear of injury to the funds of the Royal Infirmary, by the withdrawal or diminution of subscriptions to it caused by the new call for contributions to the Hospital for Sick Children. This fear has proved groundless, we rejoice to say, and we hope that the opponents to the Children's Hospital which it produced, rejoice with us. Among the supporters of the new charity were some who restrained their zeal and gave a languid co-operation to its promoters, because they were of opinion that funds could not be procured to set it agoing and carry it on. These lukewarm friends must be astonished to hear already of a new hospital, and we hope their want of reliance on the generosity of a benevolent public will never be justified.

Much enthusiasm was manifested at the establishment of the hospital in No. 7 Lauriston Lane. Accommodation was found there for above twenty patients, and for a time there was no need of more. But under the fostering care of the managing committee, of the physicians, and of the matron, the reputation of the house rapidly extended and increased, and the hospital became crammed, sometimes receiving nearly thirty patients. The dispensary for giving out-door relief became also oppressed with applicants for advice and medicine.

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[1] Transactions of the Medico-Chirurgical Society of Edinburgh, vol. iii. p. 105.

During 1862, the number of children resident in the hospital has been 265, and the number of out-door patients has been 3610. In order to allow of still further extension of the operations of the charity, the directors last year secured a locality, pleasantly situated on our southern park or Meadows, and on it have raised a new hospital.

A brief account of this building is given in the following extract from the Report of the Directors of the hospital for 1863:—"Hitherto the accommodation for the reception of patients in the hospital having been not only very limited, but, necessarily, defective in its construction and arrangement, the medical officers were precluded from admitting many of that class of cases whose removal from among other children would have afforded the greatest relief,—viz., fever patients. But much better accommodation will be very soon supplied. In their last Report the directors were enabled to congratulate the contributors on having permanently acquired very suitable premises for the hospital at Meadowside House. And these premises have now been successfully converted into a large and commodious hospital, capable of containing about 44 patients.

"In this new hospital—which will be ready for the reception of patients in May—there are three distinct departments, viz., the *hospital* department, the *dispensary* department, and the *household* department. The *hospital* department consists of two long wards, each 56 feet by 15 feet, and each suited for 13 or 14 patients; two fever wards, each 25 feet by 22 feet, and each suited for 6 patients; and one ward for special cases, 17 feet by 12 feet, suited for 3 or 4 patients. The large wards are on the first and second floors, and are each supplied with bathroom, nurse's room, and other conveniences. They are well lighted and ventilated,—the windows, from which a pleasant view of the grounds and Meadows is obtained, having a southern exposure. The fever wards are placed in the west end of the hospital, and are approached by a separate staircase. The *dispensary* department is situated on the ground floor, near the entrance gateway, and consists of a well-ventilated, large waiting-room, a prescribing room, and a commodious lecture-room. Those rooms are so arranged that patients pass from the waiting room to a hall adjoining the laboratory, and thence out by a separate door. The *household* accommodation consists of the house-surgeon's rooms (near the dispensary), matron's rooms, nurses' rooms, kitchen, and servants' rooms. All these are commodious and well ventilated, and every consideration has been bestowed on securing efficiency and economy in every department.

"Adjoining this commodious and well-arranged building, there is a large airy garden for convalescent patients. Thus, the object originally contemplated, has been most satisfactorily attained, as regards number of patients, situation, and accommodation, within a period, and in a form, in all respects so complete, that it may well be the subject of congratulation."

We are sure the medical profession will join with us in commending the zeal and admiring the success of the promoters of this institution. It recommends itself to medical men not only as a benevolent scheme, but also as a means of advancing our imperfect knowledge of the diseases of children, and of educating more completely than heretofore the numerous medical students who flock to this metropolis.

We are happy to hear that the Queen has been graciously pleased to become patroness of the charity, and has intimated, through her Secretary of State, her desire that it be henceforth entitled, "The Royal Edinburgh Hospital for Sick Children."

## ARMY MEDICAL SERVICE.

THE following were the questions proposed at the recent examination of candidates for admission into the Army Medical Service :—

### ANATOMY—10 A.M. to 1 P.M.

1. Describe the membranous portion of the urethra, its form, relations, and structure.

2. Describe the parts, as displayed on dissection, contained in the space bounded by four lines corresponding to the borders of the masseter muscle, proceeding from the surface inwards.

3. Point out the situations of the ganglia of the sympathetic in the head and neck, and describe their connexions with other nerves.

4. Describe the human blood, its physiological, physical, and chemical properties, the mode of development of the corpuscles, and mention the distinctive peculiarities of the blood in various other mammals, birds, fishes, and reptiles.

5. Describe the mode of development of a vertebra in general, noticing, also, those vertebræ (not including the sacrum and coccyx) which appear the most exceptional to the common plan.

6. Point out the homologies between the bones of the anterior extremity of a horse and of an ox, and those of the upper extremity of man.

### SURGERY—2 to 5 P.M.

1. Describe the ordinary characters of cancer of the tongue, and the means of distinguishing it from other growths and ulcers of the same organ.

2. State any rules that you would generally observe in regard to the opening of certain abscesses as soon as the presence of pus is ascertained, and the leaving of others till they point or spontaneously discharge.

3. State your opinion whether, in diseases of the knee-joint, excision of the articular parts of the bones is to be preferred to amputation; give reasons for your opinion, and indicate the cases, if any, in which you would prefer excision.

4. Name three of the best means (not including the ligature) for arresting hæmorrhage from small bloodvessels recently divided; state the means of employing them, and the cases to which they are severally best suited.

5. Describe the principal forms of fracture of the patella, their symptoms, and, very briefly, the processes for their repair.

6. What are the chief diagnostic symptoms of ovarian tumours, and what cases may be best selected for ovariotomy?

### NATURAL HISTORY—10 A.M. to 1 P.M.

1. What are the drugs obtained from the natural order leguminosæ, and what are the botanical names and native countries of the plants producing them?

2. Enumerate the natural orders, botanical names, and native countries of the principal diuretic plants.

3. Of what parts does the seed of a dicotyledonous plant consist?

4. In what part of the plant is the pollen grain developed? What is its function, and how performed?

5. In what part of the stem does liber occur; what are its characters and economical applications; from what natural orders do our manufacturers derive their greatest supply?

6. What are the chemical and physical properties of gutta-percha and india-rubber?

7. Describe the principal modifications of the vertebral column in the vertebrata.

8. How can you distinguish microscopically a piece of wood, bone and tooth of mammal, claw and bone of bird, horn of ruminant, and shell of a lamelli branch?

9. Upon what principles is the mercurial barometer constructed?

10. Why does the barometer tend to rise before or during dry weather, and to fall before or during rain?

11. In what form does iron ore chiefly occur in Britain, and what is the process of extracting the metal?

12. Define tersely the terms dialysis, induction, detonation, ebullition, evaporation, congelation, allotropic, oblique, horizontal, arithmetical and geometrical progression.

MEDICINE—2 to 5 P.M.

1. Enumerate the most common organic diseases of the cardiac valves. Describe the physical signs of a mixed case of obstructive and regurgitant aortic, and of mitral regurgitant disease, and state what would be the effect of such lesions on the heart itself and other organs.

2. Define the following stethoscopic terms: bronchial respiration, bronchophony, mucous râle, crepitation, amphoric respiration. In what diseases are humid râles heard in the lungs, and what would be the differential diagnosis of these diseases?

3. Describe a typical case of typhoid fever ending in perforation on the nineteenth day. What should be the treatment of the perforation, if recognised?

4. What are the symptoms and treatment of chronic ulcer of the stomach leading to perforation?

5. What are the chief causes, and what should be the treatment, of convulsions in children under two years old?

6. For what purposes are salts of lead used in pharmacy? What are the symptoms and treatment of chronic lead-poisoning?

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## ON DILATATION BY FLUID PRESSURE IN OBSTETRIC THERAPEUTICS.

(*To the Editor of the Edinburgh Medical Journal.*)

SIR,—Your Journal for the present month contains a paper by Dr Keiller, on the invention of fluid pressure as an instrument for dilatation in midwifery.

Many years elapsed from the adoption of this expedient in the treatment of stricture and stone, before the idea occurred to me that so excellent a mode of rapidly dilating the animal textures might, probably, be extended with great advantage to the numerous dilatations required in the practice of midwifery. Strongly impressed with the value of this idea, I sought every opportunity of obtaining experimental proof of its correctness, and on publishing a second edition of my treatise on "Stricture and Stone," in 1840, I made this new and most important use of fluid pressure generally known. In the fourth section of the appendix to that work, after some remarks on the inadequacy of the various measures hitherto employed for dilatation in midwifery, it is stated that, "Nature opens the passages, at least in great part, by mechanical means—by the soft wedge, as it has been called, of the distended membrane;" and the employment of an instrument for dilatation is recommended, constructed in imitation of this natural expedient. I conclude the dissertation by expressing my "conviction, from much observation and reflection on the subject, that obstetric surgery will receive a valuable acquisition in the application of this principle of dilatation by fluid pressure, in cases where, to prevent danger or suffering, not only in childbirth, but in the earlier stages of pregnancy, it may be advisable to open the passages with the least possible irritation. The construction of the apparatus may, on ordinary occasions, be exceedingly simple; and for using it, the best rule will be, to imitate as closely as possible the intermitting and gradual natural process of dilatation in parturition."

In an essay "On the Present State of Therapeutical Inquiry," which I published in 1845, the subject is again adverted to; and, amongst other remarks, it is stated that, "in the construction of the simple apparatus required, there is no necessity for attending to various minute points, essential to the utility of the fluid-pressure dilators of strictures in the narrower canals, unless, perhaps, where a greater nicety is desirable, as in opening the uterus for the extraction of the secondaries in hæmorrhage from abortion."—(Appendix on Midwifery, page 11).

Even in the preface to a work "On the Remedial and Anæsthetic Uses of Congelation," published in 1849, I sought an opportunity of returning to a subject which I deemed of so much importance; but it is unnecessary to add more quotations.

The second edition of my "Treatise on Cancer" (1854) contains incidental notices of obstetric fluid pressure as a means of suppressing hæmorrhage, of opening the vagina for the purpose of facilitating operations on the uterus, of obviating the necessity of using the forceps in certain cases, and of exciting uterine contraction.

Twenty years after the publication of the first of these works, and five years after the last, three claimants appear for the invention of the obstetric fluid-pressure dilator, Dr Keiller, Dr Storer of the United States, and M. St Tarnier of Paris. Dr Barnes merely claims some peculiarity in the shape of the instrument.

Dr Keiller first preferred his claim in 1859, in a letter addressed to the Medical Times and Gazette. On that occason, he expressed "his belief that he might fairly claim" the obstetric fluid dilator "as his own." In the present communication, his claim assumes the form of an accusation against Dr Barnes, that "he has been, to say the least of it, unfairly silent as to the claims of those who, prior to him, were engaged in developing and introducing into obstetric practice" the agent of fluid pressure. There is an essential difference between the two claims, one being for the invention, and the other for endeavours to introduce it; and it is to be regretted that Dr Keiller does not express this difference in unequivocal terms. No one will dispute the justice of the latter. Of course, every one who aids in the introduction of a valuable therapeutic agent is deserving of much credit, and this would be freely awarded if nothing more were demanded than what could be justly conceded. It must often happen that the originator of a medical improvement has not the opportunity which a public hospital affords of making it speedily known and appreciated; but as he knows that it is one of the principal purposes of hospitals to promote medical science, he naturally hopes for, and has a right to expect, the assistance, in establishing the improvement, of those in possession of such advantages. At all events, he has the right to expect, that the better opportunities which have fallen to the lot of others, shall not be so used by them as to deprive his discovery of the credit due to it.—I am, Sir, your most obedient servant, JAMES ARNOTT.

LONDON, 7*th March* 1863.

*Note.*—The remark in the concluding sentence, on the expectations which the originators of therapeutical improvements have, that their new methods shall be so employed as not to throw discredit on them, has especial reference to a paper on Local Anæsthesia, which appeared in the Edinburgh Medical Journal for July last. The authors of that interesting paper express a high opinion of intense cold as a substitute, in many cases, for chloroform, and support this opinion by a relation of cases in which they had witnessed its application in Guy's Hospital. I gladly embrace the present opportunity for stating, that although this anæsthetic answered well on these occasions, it is surprising that it did so, considering the very imperfect and almost reckless manner in which it appears to have been employed. Instead of the part being congealed for a minute or two, the mixture of ice and salt, we are told, was kept applied over a hernia in the groin for many hours. It probably happened that the congelation produced was not of long duration, and, possibly, actual congelation may not have been produced at all; but this ought not to be a matter of accident. A very short congelation will produce perfect insensibility, and at the same time obviate inflammation from the wound: congelation prolonged for hours will destroy the textures to a certain depth, and so completely as to constitute (when a powerful freezing mixture is frequently renewed in a deep gutta-percha cup fitted to the part, and a considerable degree of pressure is simultaneously applied) an excellent addition to caustic, or a substitute for it, in the removal of cancerous growths.

## ORIGINAL POETRY, FROM "THE DELHI GAZETTE."

### ACROSTIC.

By the Queen's Command, the
Royal Warrant gives
Each Assistant Surgeon
Allowances, Pay, and Rank of
Captain, when he
Has served six years.

"Oh!" says the Government,
"Fury take the Queen.

"Faith! we'll give just what we like;
And all the Warrants
In the world sha'n't make us
Tip rupees, or justice do, unless we
Have the will."

Un Pauvre Diable.

### ANOTHER AND BETTER.

By Royal Warrant, England's solemn law
Rules, that Assistant Surgeons now shall draw
Emoluments of Captains: just reward,
After a six years' service, spent to guard
(Careless of Indian heat, Canadian cold)
Health's blest continuance in her armies bold.

Oh! cries the Local Government, "What arrant
Fools they must be, who think a Royal Warrant

"Favour will find with us. Supreme ourselves!
Away with plaint of discontented elves!
India shall learn, that loud must be her cries,
To reach to Westminster; so let your sighs,
Humbugged Assistant Medicos! from her still
fruitlessly arise!"

## PUBLICATIONS RECEIVED.

Ballard,—Convulsive Diseases of Infants. By Thomas Ballard, M.D. London, 1863.
Bedford,—Principles and Practice of Obstetrics. By Gunning S. Bedford, M.D. Third Edition. New York, 1863.
Fownes,—Manual of Elementary Chemistry. By George Fownes, F.R.S. Ninth Edition. London, 1863.
Hunt,—Stammering and Stuttering. By James Hunt, Ph.D., etc. Fifth Edition. London, 1863.
M'Clintock,—Clinical Memoir on Diseases of Women. By A. H. M'Clintock, M.D., etc. Dublin, 1863.
Sutton,—Systematic Handbook of Volumetric Analysis. By Professor Sutton. London, 1863.
Sydenham Society (New),—Atlas of Portraits of Diseases of the Skin. Third Fasciculus. London, 1862.
Thudichum,—A Treatise on Gall-Stones: their Chemistry, Pathology, and Treatment. By J. L. W. Thudichum, M.D., etc. London, 1863.
Tilt,—Handbook of Uterine Therapeutics. By Ed. J. Tilt, M.D., etc. London, 1863.
Yearsley,—Deafness practically illustrated. By James Yearsley, M.D., etc. Sixth Edition. London, 1863.

## PERIODICALS RECEIVED.

Bibliothek for Læger, femte Række,—April, July, and Oct. Copenhagen, 1862.
Births, Deaths, and Marriages, Monthly Return of,—February 1863.
Boston Medical and Surgical Journal,—Feb. 5, 12, 19, 26, 1863.
British Medical Journal,—Feb. 28, March 7, 14, 21. London, 1863.
Canstatt's Jahresbericht,—Parts 3 to 6 for 1861. Wurzburg, 1862.
Dublin Medical Press,—Feb. 25, March 4, 11, 18, 25, 1863.
Gazette des Hôpitaux,—Feb. 19, 21, (24th not received), 26 (two copies), 28, March 3, 5, 7, 10, 12, 14, 17, 19, 21. Paris, 1863.
Gazette Hebdomadaire de Médecine, etc.—Feb. 27, March, 6, 13, 20. Paris, 1863.
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Medical Times and Gazette,—Feb. 28, March 7, 14, 21. London, 1863.
Medizinische Jahrbücher,—Parts 4 and 5. Vienna, 1862.
Norsk Magazin for Lægevidenskaben,—Nos. 11 and 12, vol. 16. Christiania, 1862.
Revue de Thérapeutique Medico-Chirurgicale,—March 1, 15. Paris, 1863.
Wochenblatt der Zeitschrift der Aerzte,—Nos. 2 to 5. Vienna, 1863.
Würzburger Medicinsche Zeitschrift,—Part 6, vol. 3, and Part 1, vol. 4. 1862–63.
Würzburger Naturwissenschaftliche Zeitschrift,—Part 2, vol. 3. 1862.

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# Part First.

## ORIGINAL COMMUNICATIONS.

### ARTICLE I.—*Report of Clinical Cases treated in the Surgical Wards of the Royal Infirmary, under the care of* Mr SPENCE, *during the Session* 1861-62. By FREDERICK STEELL, M.D., late Resident Surgeon.

### INJURIES OF THE HEAD.

A. B., æt. 21, was brought to the hospital on October 2d, at 9.30 P.M., with a severe compound comminuted fracture of the skull, which had been caused by the discharge of a fowling-piece. The fracture involved the left parietal and frontal bones, and was situated about midway between the anterior termination of the sagittal suture and the anterior inferior angle of the parietal bone. The wound of the integuments was much lacerated, and several pieces of the splintered bone were depressed. Mr Spence, who was at once summoned to the case, removed in all fifteen detached fragments of bone, and elevated any remaining portions which were depressed. The dura mater was found intact, except at one point where there was a small laceration. The patient, who was now placed in bed, after having had his head shaved and cold applied, seemed to be in some degree conscious of what was going on, and to understand questions addressed to him, though unable to answer them. The pulse, which, though weak, was about the natural standard after the operation, about three o'clock next morning became strong and full, and rose to 88; and as the patient was feverish and restless, he was ordered antimony in doses of a twentieth of a grain. During the day he vomited twice; the pulse varied from 80 to 54. He was ordered six leeches behind the left ear, and an enema, which moved his bowels freely. On the morning of the 4th, after a tolerably quiet night, the wound was looking well, but the pulse, which was about 66, was irregular, and there was slight paralysis of the right side. On the evening of the 5th, six leeches were again ordered to be applied to the mastoid region, in consequence of the pulse having fallen to about 50. *6th.*—Pulse 60; urine, hitherto passed in bed, drawn off by a catheter; ordered a castor-oil enema. *7th.*—Patient has passed a good night, and seems more intelligent, but is still unable to make water. Convulsions, however, came on in the afternoon, chiefly affecting the paralyzed side, and there was considerable hæmorrhage from the wound, through which the brain substance had begun to protrude. *8th.*—Has had two more convulsive seizures; pulse 67, regular; blister ordered to the nape of the neck. About 3 P.M., there was renewed hæmorrhage from the wound, which was arrested by transfixing the bleeding point and securing it by a ligature; pulse 86, regular, but weak and compressible. *9th.*—Hæmorrhage again occurred, and could not be stanched till a slough had been removed. *12th.*—For the last two days the fungus cerebri has been rapidly increasing, and is now about the size of the closed fist. Patient somewhat restless, but intelligence still improving; has passed water voluntarily for the last three days; pulse 100. *15th.*—Since last report the patient has been progressing favourably, but to-day he has had repeated rigors, and been very restless and noisy. Part of the fungus has separated; the bowels have been freely moved by an enema; pulse 106. *16th.*—Patient looks much worse; has passed a very bad night. Recurring hæmorrhage from the surface of the fungus and edges of the wound arrested by matico; pulse 100. Early next morning, symptoms of coma gradually set in, and patient died about 4 A.M. On post-mortem examination an abscess was found diffused through the left cerebral hemisphere, the surface of which was covered with a layer of pus most distinct towards the base.

J. T., while following a runaway horse, received a kick on the forehead. He had been drinking previously to the accident, and when admitted to the hospital half an hour afterwards, was in a somewhat excited state, and his pupils were dilated. There was a compound fracture, with depression a little above the right superciliary ridge. Mr Spence being sent for, immediately trephined, and found much splintering of the inner table, with slight abrasion of the dura mater. The head was ordered to be shaved and cold applied, while low diet and perfect quietness were enjoined. For six days after the operation the patient progressed most favourably. The wound looked well; there was no headache or flushing of the face, and his pulse averaged 72. On the seventh day, after a restless night, his pulse rose to 86. He was ordered six leeches behind the ears, antimony in doses of a twentieth of a grain, and a saline aperient. Feverish symptoms continued to increase in severity during the two following days. He complained of intense headache; his pupils were dilated, and there was a wild expression of countenance, with slight delirium. On the tenth day, the pulse fell to 40, and, after several violent convulsive seizures, he died comatose. After death there was found to be pus infiltrated into the meshes of the pia mater all over the surface of the brain.

J. R., who had been sent to the hospital from a distance of ten miles, was admitted on the evening of the 4th of April, suffering from severe injuries of the head, face, and perineum. The debilitated state of the patient almost amounted to collapse, but he was sensible, and after some time answered questions rationally by signs or writing,—the condition of an injury of the jaw preventing him from speaking intelligibly. Mr Spence, on examination about an hour after his admission, found,—1*st*, A lacerated and contused wound of a triangular form over the right temporal region, about an inch and three-quarters long, communicating with the bone, which was felt bare, and exhibited a fissured fracture over the superciliary ridge and external angular process of the frontal bone; 2*d*, A lacerated wound, one and a half inch long, passing obliquely from the root of the nose downwards and outwards below the lower right eyelid to the cheek; 3*d*, A lacerated and contused wound, an inch and a half in length, in front of the right ear, communicating with the zygomatic process of the temporal bone; 4*th*, A lacerated and contused wound of the scalp, one and a half inch long, over the upper part of the frontal region, corresponding to, but not communicating with a depressed fracture, the bone not being laid bare, but still covered with pericranium; 5*th*, A compound fracture of the lower jaw close to the symphisis, and great swelling and contusion over the right side of the face, the jaw being apparently fractured at another part near the angle; 6*th*, A lacerated wound of the scrotum, and swelling in the perineal region; and, on passing the finger into the wound, the deeper textures of the perineum were felt to be injured and infiltrated with blood. On passing a catheter along the urethra, it was felt to pass through a laceration of the canal about the bulb, apparently into a quantity of clotted blood, and, after some little care, it was lodged in the bladder and retained.

The wound over the right temporal region was slightly enlarged, in order to permit a more complete examination of the bone; the other wounds were lightly dressed, and the fractured jaw was supported by a bandage. 5*th*.—Passed a restless night, and pulled out the catheter. Pulse weak, 120. Ice ordered to be applied to the head; urine drawn off with the catheter. *Vespere*.—Urine again drawn off. Ordered to have beef-tea during the night. 6*th*.—Made water twice himself last night; was not quite so restless. Pulse 130. Warm-water dressing applied to all the wounds on face and head; fomentations to the scrotum. Ordered to have an enema, and cold to be applied to the head; also to have a little wine along with the beef-tea. 7*th*.—Pulse 120. 8*th*.—Warm-water dressing still applied to the wounds. Patient very restless during the night. Pulse 116. *Vespere*.—The swelling in the perineum being very tense, the opening was enlarged backwards, which allowed a quantity of fœtid fluid to escape. Ordered to have a poultice applied. 9*th*.—Pulse 118. No. 8 catheter again introduced. *Vespere*.—Very restless, and complains of great pain in the scrotum. Pulse 140. 10*th*.—Pulse 138. Gum-elastic catheter tied in the bladder. 11*th*.—Pulled out the catheter again during the night, but has passed his urine freely. Bowels loose. Pulse 128. The bone is bare for some little distance at wound over left side of head; hot-water dressing still continued. 12*th*.—To-day the urine has come away entirely from the wound in the perineum. Patient very much weaker. 13*th.*—Patient still weaker. Pulse scarcely perceptible at the wrist. Wounds are all looking glazed, and quite dry. Urine drawn off by a catheter. Patient has been and is perfectly sensible, and has not in the slightest degree suffered from any head symptom. Patient gradually sank, and died at 9.50 P.M.

J. R. fell from a window three storeys high, and was brought to the hospital immediately after in a state of profound insensibility. His pupils were dilated, his breathing stertorous, his pulse slow and feeble, and there was considerable hæmorrhage from both ears. There was considerable bruising and extravasation of blood over the vertex, but no fracture could be detected. Patient passed his urine and fæces involuntarily, and died five hours after admission, having never rallied from the state of coma. Post-mortem examination disclosed extensive fracture of the base.

D. L., æt. 38, about 3.30 A.M., on the 27th July, fell from a window into the street, alighting on his head. When admitted, immediately after the accident, he had a flushed face, was noisy, excited, and smelt strongly of spirits, but answered questions quite coherently. There was an extensive bruise over the right temporal region, where the patient complained of pain; there was great swelling and ecchymosis of the eyelids, especially the right. There was no external wound, and no depression of the skull. Slight vomiting, but no signs of compression. Pulse 96, and soft. The head was shaved and cold applied, and a cathartic enema was administered. Towards evening, as he was more restless and excited, and the pulse had risen to above 100, he was ordered to have three leeches behind each ear. About 5 o'clock on the following morning, I was summoned to the patient, and found him violently convulsed, with a slow and irregular pulse and dilated pupils. In the intervals of the convulsions he was evidently comatose. Mr Spence, who was now called, made an incision over the point where the patient had complained of pain, and where there was the greatest amount of extravasation, and, finding a fissure, immediately trephined. The inner table was found splintered at the edges of the fissure; there was some amount of blood between the bone and the dura mater, and the latter was tense and bulging, and, when divided, a considerable quantity of bloody serum escaped. The patient never rallied after the operation; convulsions recurred in the course of the day; coma gradually became more profound; and he died in the evening at 10.30 P.M.

W. K. fell from a scaffolding twenty feet high, and when admitted to the hospital, a short time afterwards, was found to have sustained a fracture of the right clavicle and severe bruising of the right side of the head. He was in a deep stupor, and when roused, which was accomplished with great difficulty, muttered shortly and incoherently; the pupils were dilated, but slightly sensitive; the breathing slow and laboured; there was hæmorrhage from both ears, and the pulse was about 60. The head was shaved and cold applied, and perfect quiet was enjoined. On the following day, the pulse was 54, and as the state of semi-consciousness continued, leeches were applied behind the ear, and a cathartic enema administered. Under this treatment sensibility gradually improved, and the patient, after two days, could answer questions quite intelligently. Pulse 74. After this the patient continued to progress favourably, no bad symptom occurring except double vision, which yielded to a seton introduced into the nape of the neck. At his own request, he was dismissed at the end of three weeks.

E. M., æt. 16, while cleaning a window, fell from a balcony three storeys high, and, besides much general bruising, received a lacerated contused wound, situated about the middle of the left parietal bone, which was partially denuded of its periosteum. The symptoms of concussion, which were well marked on admission, gradually passed off; but on the evening of the following day they were followed by flushing of the face, a quick, feverish pulse, and intense headache. These symptoms, however, yielded to leeching and the exhibition of antimony with saline aperients, and the patient seemed doing well until the fifth day, when the wound took on an erysipelatous action, which extended over the head and face. This soon abated under the use of tincture of the muriate of iron, with occasional purgatives, and the patient left cured at the end of four weeks.

J. G., æt. 16, lost his balance while in a railway waggon in motion, and his head fell before the wheel, which inflicted a severe scalp wound, extending from the anterior part of the temporal ridge upwards and backwards to the occiput, and extensively exposing the bone. He was sent to the hospital from a distance, and when admitted, exhibited all the symptoms of slight concussion. A few stitches were introduced to support the pendulous flap, the hair was cut short, and cold applied to the head. As the patient seemed doing well, rest, with low diet and occasional laxatives, was the only treatment adopted till the tenth day, when, after a rigor, feverish symptoms began to show themselves. He now complained of great thirst, with intense headache, and became flushed and very feverish. He was ordered six leeches behind the ear. He vomited several times during the following forenoon, and in the evening was drowsy and delirious at intervals. From this state of stupor he never rallied, and died in a convulsive seizure fourteen days after admission. Extensive meningitis was found on post-mortem examination.

*Remarks.*—The preceding examples of the more serious forms of head injuries illustrate some important points in the treatment of this class of cases. The case first detailed in the report was one of very severe and extensive compound comminuted fracture; and when we consider the lacerated state of the scalp, and the extreme splintering of the skull, there can be little question of the propriety of removing the shattered and isolated fragments of bone. Even in the absence of any marked symptoms of compression, operative interference was not only justifiable but urgently called for, as the presence of broken pieces of bone, severed from all vital connexion, would, by their irritation of the membranes and their certain necrosis, have undoubtedly been attended by greater risk to the patient. The paralysis, convulsions, and other nervous symptoms, together with the hernia cerebri which subsequently took place, cannot be considered as having been influenced by the operation, for they were no doubt due to sloughing of the membranes, and to inflammation and suppuration within the cranium, following the laceration of the dura mater and contusion of the brain substance, which took place at the time of the accident. As in this case, true hernia cerebri seems always to be accompanied by abscess of the cerebral matter, and to be the result of increased volume due to the presence of the products of inflammation. Though there was great danger from the recurring hæmorrhage, death was caused not by the hernia cerebri, but by the morbid processes going on within the cranium, of which the hernia cerebri was the result.

The fracture in the case of J. T. was attended by a considerable amount of depression, and as from the nature of the accident it was probable that there was much splintering of the inner table, Mr Spence, even in the absence of symptoms of compression, did not hesitate to trephine. The suspicion of the inner table being extensively injured, though there was no external comminution, was amply confirmed by the operation, as several detached pieces of bone were found, and carefully removed. As in the former case, the risks to be dreaded were irritation or puncture of the dura mater by the spicular fragments, and inflammation within the cranium attending the subsequent necrosis; and, although the case terminated fatally, it must be allowed that, while the operation superadded no danger, the fracture being compound, it gave the patient a better chance of life. After the continuance of severe cerebral symptoms for two days, the dura mater became tense and bulging, and as there was no other hope for the patient, it was freely incised, and a small quantity of pus was evacuated. This procedure proved futile; for in this, as in almost all cases where pus is situated below the dura mater, it is not circumscribed, but diffused over the surface of the brain.

In the case of the unfortunate man J. R., there were two fractures,—one compound and fissured, the other simple and depressed; but, in the absence of cerebral symptoms, neither of them called for operative interference. However, the depression in the latter was of small surface and considerable depth, and had the fracture been compound, the correct practice would have been to trephine. At the same time, seeing it was a simple fracture, even if there had been symptoms of compression, it would have been proper treatment to have tried other remedial measures before having recourse to operation, unless the symptoms were unusually urgent.

The possibility of serious injury of the head remaining latent for a considerable time is well seen in the case of D. L., who, although he had sustained an extensive fracture of the skull, accompanied by laceration of the brain-substance, showed no marked cerebral symptoms for several hours after the accident. On admission, the patient was apparently suffering from slight concussion, which soon passed off, and no alarming symptoms manifested themselves until the following morning, when he was seized with convulsions and subsequent coma. The development of these symptoms, and the period of their occurrence, pointed to extravasation of blood as their cause; while the site of the external injury rendered it not improbable that there was fracture of the skull, with rupture of some of the branches of the middle meningeal artery. Accordingly, an exploratory incision was made down to the bone, and as this disclosed an extensive fissure, it was determined to trephine, as affording the patient, who was fast sinking, the only remaining, though a feeble chance for life. A small quantity of blood was found between the dura mater and the bone; but, as this could not be considered a sufficient cause for the symptoms, it was resolved, as a last resource, to puncture the dura mater, which was tense and bulging. The operation, though demanded in the circumstances, was followed by no improvement in the condition of the patient, who gradually died comatose. On post-mortem examination, the fissure was found to extend right across the base of the skull. The base of the brain was also found ruptured at several points, more especially at the line of fracture, and a layer of pus covered its entire surface. The occurrence of a distinct layer of pus over the surface of the brain at so early a period after the accident is certainly remarkable, and may seem to justify a suspicion of the injury being merely a coincidence occurring in the course of an inflammatory action of the meninges. This, however, is rendered improbable, not only by the history of the case, but also by the facts that the pus was most abundant in the line of fracture and at points corresponding to the rupture of the brain-substance, and that the brain-substance itself at these points was undoubtedly the seat of inflammatory action, as proved by microscopic examination.

## TRACHEOTOMY.

1. C. M., æt. 32. Laryngitis of a month's standing, with frequent paroxysms of dyspnœa threatening suffocation. Tracheotomy. Death on second night, from capillary bronchitis.

2. G. M., æt. 2 years and 3 months. Croup in an advanced stage. Tracheotomy. Recovered.

3. T. W., æt. 16 months. A weakly strumous child. Croup. Tracheotomy. Death on the second day. Post-mortem examina-

tion disclosed tabes mesenterica, and infiltration of both lungs, with miliary tubercle.

4. D. R., æt. 19 months. Croup. Tracheotomy. Death on the second day. On post-mortem examination, the false membrane was found to have extended to the smaller ramifications of the bronchi.

5. M. P., æt. 2 years. Diphtheritic croup. Tracheotomy. Death on the tenth day. The laryngeal symptoms were immediately relieved by the operation, and the case progressed favourably till the seventh day, when there was an accession of diphtheritic symptoms, evinced by the formation of false membrane on the lips, the nares, and the edges of the wound, and the gradually failing strength of the patient, the breathing being at the same time perfectly free. Notwithstanding the free administration of stimulants, the patient sank.

6. C. W., æt. 4. Croup. Tracheotomy. Recovered.

7. G. B., æt. 2. Croup. Tracheotomy. Recovered.

8. D. S., æt. 16 months. Croup. Tracheotomy. Died on the second day.

F. O. Hydrocele of the thyroid gland. Laryngo-tracheotomy. This patient first came under Mr Spence's care in January last, on account of a tumour of the thyroid gland, which speedily yielded to blistering and the internal use of iodide of potassium. On July the 3d, he again presented himself, and was then suffering from considerable embarrassment of respiration, with occasional paroxysms of dyspnœa. Speaking, even for a short time, brought on violent fits of coughing, unaccompanied by expectoration. On examination, a tumour was found occupying the position of the isthmus and left lateral lobe of the thyroid gland, pushing the trachea backwards and to the right side, and extending downwards apparently behind the sternum and left clavicle, as was indicated by dulness on percussion and absence of respiratory murmur over the upper sternal and inner part of the clavicular and subclavicular regions. The chest was otherwise normal; the superficial veins of the neck were much enlarged; the left pupil was distinctly contracted. The treatment again pursued was blistering and the administration of the iodide of potassium, but on this occasion without any good effect, as the fits of dyspnœa became more frequent and more severe. On the morning of the 11th, a violent paroxysm suddenly coming on, I was hastily summoned, and, finding the patient's countenance turgid and livid, his respiration stopped, and his pulse scarcely perceptible, and believing that the immediate cause of his impending death was spasm of the glottis, at once determined to perform laryngo-tracheotomy. From the position of the larynx and trachea, the necessary incision, having to be made considerably to the right of the mesial line, laid bare the anterior margin of the sterno-mastoid muscle, and involved several enlarged veins. After the operation was finished, it was found necessary to inflate the lungs through the tube and keep up artificial respiration. The patient now rallied in some degree, but it was evident there was some obstruction below the opening made into the trachea. Accordingly the tube was withdrawn and the trachea explored by the finger, when a convexity of the left side was discovered, so considerable as materially to diminish its calibre. A piece of gum-elastic tubing of large bore was now passed down beyond the obstruction, and its introduction was followed by instant relief to the patient. Next day, Mr Spence examined the tumour by passing his finger through the wound, and, detecting fluctuation, made an opening at that point, when a large quantity of clear fluid was evacuated. He next made a counter-opening by cutting down on the point of his finger, passed downwards and to the right side, through the opening made from the tracheotomy wound. The finger was now passed through the second opening downwards, behind the left sterno-clavicular articulation, but failed to reach the bottom of the cyst. After the evacuation of the fluid, the patient could breathe quite freely through an ordinary tracheotomy tube, which had been substituted for the gum-elastic one. On the following day, symptoms of acute pyæmia set in, and the patient died on the evening of the 15th. On post-mortem examination, numerous metastatic abscesses were found in the lungs and liver.

### *Foreign Body in Bronchus—Extraction.*

G. F., æt. 33, came to the hospital on June 4th, stating that, about half an hour before, the shield of a tracheotomy tube, which he had worn for the last twelve months, had suddenly become detached, and that the cylindrical portion had slipped down into his windpipe. He pointed to a spot corresponding to the bifurcation of the trachea, and said he felt the tube there. He spoke and breathed without difficulty; and the only signs of irritation were severe fits of coughing recurring at frequent intervals. Mr Spence having introduced a gun-shot probe into the trachea, guided it into the right bronchus, but failed to discover any foreign body. The left bronchus was next explored, and then the probe was distinctly felt and heard striking the metallic tube. Chloroform was now administered, and the opening into the trachea having been enlarged, a pair of long forceps, with blades bent almost at a right angle, was guided downwards into the bronchus and passed within the tube. The blades of the forceps were next forcibly expanded, and the tube thus caught was readily extracted, with immediate relief to the patient, who left the hospital quite well four days after.

*Remarks.*—The report embraces a series of cases exemplifying most of the more common circumstances which call for tracheotomy. Of the nine cases in which the operation was performed, seven were cases of croup (including one of diphtherite), and of these four died. In every instance the operation was only had recourse to after all the ordinary remedial measures had proved ineffectual, and when a speedy fatal termination was imminent, as evinced by the gradually failing respiration, the contraction and depression of the costal cartilages attending each inspiration, lividity of the countenance, and the recurrent paroxysms of suffocation. In the case of D. S., the child to all appearance had fairly succumbed before the operation was completed, as respiration had stopped, and no pulse was to be felt at the wrist; and it was only after repeated insufflation of the lungs and the continuance of artificial respiration for some time that the child gradually revived. In another case, not mentioned in the report, while preparations were being made for the operation, a paroxysm suddenly occurred from which the child never rallied, although I instantly opened the trachea. It will be noticed that in the fatal cases of croup, death generally took place about the second day, whereas in the case of diphtheria, the local symptoms did not manifest themselves until the seventh day, after which the patient gradually sank without any embarrassment of respiration. As a general rule death, when it occurs, takes place early in simple croup, while in diphtheritic croup, life may be prolonged until the second or third week after operation, and yet the case eventually prove fatal.

Diphtheria and croup differ also considerably as regards the mode and cause of death, which in the former is principally due to asthenia, and in the latter to asphyxia resulting from the extension of false membrane to the smaller bronchi, capillary bronchitis, and congestion of the lungs.

But while the two diseases differ in these and other respects, the case of M. P. shows that at the onset, it is by no means always an easy matter to distinguish them, in fact that they may be symptomatically the same.

The case of laryngo-tracheotomy has been fully detailed in the report; and all that need be remarked concerning the case of W. N. is, that the tube was readily extracted, owing to the happy way in which it was caught, and that in this, as in the majority of such cases occurring in the practice of Mr Spence, the foreign body was found impacted in the left bronchus and not in the right, in which, according to the statements in books, foreign bodies more frequently lodge.

## AMPUTATIONS.

### PRIMARY AMPUTATIONS.

### *Shoulder-Joint.*

1. D. W., æt. 18. Compound comminuted fracture of the humerus from discharge of a fowling-piece. Long posterior skin flap cut from without, and short anterior flap from inner side of arm. Recovered.

2. J. M., æt. 18. Severe laceration of arm with opening into elbow-joint, caused by the wheels of a railway waggon. Amputa-

tion by long posterior skin flap cut from without, and short anterior flap from inner side of arm. Recovered.

### *Fore-arm.*

1. D. G. Cut from a circular saw, bisecting all the carpal bones, and nearly severing the hand. Double flap amputation at middle of fore-arm. Recovered.

2. T. S., æt. 27; admitted November 18th. Severe crushing and laceration of the hand by machinery. Amputation at middle of fore-arm by skin flaps. Stump healed kindly, but patient died on the 8th of January of pleuro-pneumonia, which came on long after the stump was completely healed.

### *Hip-Joint.*

W. S. Severe compound comminuted fracture of the thigh, with great laceration and contusion of soft parts, and exposure of sheath of femoral vessels from Poupart's ligament to Hunter's canal, the result of a railway accident. In the absence of Mr Spence, Dr Watson was called to the case, and performed amputation at the hip-joint by skin flaps, the outer being somewhat the longer. Patient died on the third day.

### *Thigh.*

1. R. B. Compound comminuted fracture of right leg and arm. A railway carriage had passed over the leg, crushing it to a complete pulp. This patient having lost a large quantity of blood previously, was admitted in a state of complete collapse, from which stimulants freely administered failed to rouse him. As there was oozing of blood into the tissues of the leg, amputation by the long anterior flap was performed at the lower third of the thigh, as affording the only chance of life. Had the state of the patient admitted, the arm also would have required amputation. He lived twenty-six hours after the operation.

2. M. C. Compound comminuted fracture of the leg, resulting from a railway accident. Amputation at the lower third of the thigh by the long anterior flap. Recovered.

### *Leg.*

1. T. M. Compound comminuted fracture of the leg. Amputation below the knee by skin flaps, the posterior being somewhat the longer. Pyæmia. Death.

2. J. B., æt. 21. Severe crushing of foot and leg by wheels of a railway engine. Amputation at the middle of leg by skin flaps. Pyæmia. Death on the sixteenth day.

3. J. N., æt. 26. Compound comminuted fracture of leg, caused by machinery. Amputation below the knee by skin flaps. Rigor on the third. Pyæmia. Death.

### *Ankle.*

1. T. W., æt. 25. Severe lacerated wound of foot, with fracture

of metatarsal bones. Ordinary amputation at the ankle. Recovered.

2. R. S., æt. 42. Severe crush of foot by wheel of a railway truck. Amputation at ankle by ordinary method. Recovered.

### SECONDARY AMPUTATIONS FOR INJURIES.

#### *Thigh.*

1. S. C., æt. 8, was admitted on the 15th of July with a compound fracture of the thigh between the middle and upper thirds, resulting from a fall into an area from a height of 20 feet. The wound in the integuments was very small; and the limb was placed in a long splint. Necrosis of a considerable portion of both the upper and lower fragments of the bone took place, and abscesses formed around the thigh with profuse purulent discharge. As the patient was evidently sinking from hectic caused by the profuse discharge, while the necrosed portions of bone had not separated, amputation was performed on the 26th of August, immediately below the trochanters, by antero-posterior flaps formed by transfixion. The case promised well for some time, but, pyæmia setting in, she died on the 18th of September.

2. C. A., a strong healthy girl, æt. 16, was admitted on the 13th of July with a compound comminuted fracture of the leg, caused by a light butcher's cart having passed over the limb. Pulsation in tibials distinct; temperature of the foot good; no great bruising of soft parts. Several detached portions of bone were removed; and the limb placed in a Liston's splint. *July* 17*th.*—Gangrene of the limb having taken place, amputation was performed at the lower third of the thigh, by the long anterior flap. There was unusually little bleeding during the operation, and there was no sloughing or gangrene of the stump; but the patient took a rigor on the 20th, and died of pyæmia on the 29th.

3. A. N. Popliteal aneurism; rupture. Amputation. Death from morbus cordis.

#### *Ankle.*

1. T. S., æt. 10, admitted on the 8th of June with acute spreading gangrene of the foot, supervening on a severe compound fracture of the metatarsus, caused by machinery, three days previously. This was accompanied by delirium and irritative fever. The ordinary amputation at the ankle-joint was performed. The case did well, and all the symptoms of irritative fever gradually passed off except the delirium, which lasted for several weeks after the stump was completely healed. This ultimately disappeared, and he was dismissed cured on the 7th of August.

2. H. Y. Traumatic gangrene following a severe injury of the foot, the result of a railway accident five days before. Little or no constitutional disturbance. Amputation at the ankle-joint was performed in the ordinary manner, and the patient speedily recovered without a bad symptom.

## SECONDARY AMPUTATIONS FOR DISEASE.

### *Fore-arm.*

W. J., æt. 13 weeks. Congenital erectile tumour in the palm of the right hand, about the size of a small hen's egg, and increasing rapidly in size. The tumour was transfixed at its base by three needles, and tightly constricted by a strong waxed ligature passed round it under the needles. After the separation of the bulk of the tumour by sloughing, repeated hæmorrhages occurred so as to endanger the little patient's life, while the tumour seemed again to increase in size. Amputation was accordingly deemed necessary, and performed at the lower third of the fore-arm, by double flaps formed by transfixion. Patient made a speedy recovery.

### *Hip-Joint.*

L. S. Medullary tumour of femur. Amputation at hip-joint by antero-posterior flaps formed by transfixion. This patient made an excellent recovery, and when she left the hospital had entirely lost the cachectic appearance which she exhibited on admission.

### *Thigh.*

1. M. M. Acute synovitis, ending in destruction of the knee-joint. Amputation by the long anterior flap. Recovery.

2. H. P. Strumous disease of knee-joint of nine years' standing. Amputation at the lower third of the thigh by the long anterior flap. Recovered.

3. J. B. Strumous disease of the knee, first treated by strapping and rest in a wire splint, nourishing food and cod-liver oil being at the same time administered. These means proving of no avail, and the general health failing, amputation was performed at the lower third of the thigh by a long anterior flap. Recovery.

4. J. C. Disease of the knee-joint of nine years' standing. As in this case there were two depressed cicatrices over the joint, amputation was performed by a modification of the circular method instead of by a long anterior flap. Recovered.

5. B. M. Necrosis of the tibia in a strumous subject. Amputation at the lower third by a long anterior flap. Recovered.

6. A. C., æt. 4. Strumous disease of the knee-joint, with ulceration of cartilage. Hectic. Amputation. The stump healed almost entirely by the first intention, but, three weeks after the operation, patient was seized with acute albuminous nephritis, accompanied by hæmaturia and dropsy. This was succeeded by suppression of urine, which soon carried off the patient. After death, the kidneys were very much congested, somewhat softer than natural, and of a deep chocolate colour. There was likewise effusion both into the pleuræ and pericardium.

7. L. M. Synovial degeneration of the knee, terminating in suppuration of joint and ulceration of cartilage. Strength of patient rapidly failing. Amputation by the long anterior flap. Recovery.

8. W. L. Synovial degeneration of the knee, ending in suppuration of joint. Amputation by the long anterior flap. Excellent recovery, stump healing almost entirely by the first intention.

9. A. W. Gelatinous degeneration of synovial membrane of the knee-joint, terminating in ulceration of cartilage and abscess. General health rapidly failing. Amputation at the lower third by the long anterior flap. Recovered.

10. J. C. Synovial degeneration of two years' standing, with caries of the ends of the bones. Amputation by the usual method. Recovered.

11. G. B. Strumous disease of knee-joint, ending in ulceration of cartilage and abscess. Severe hectic. Amputation by the ordinary method. On the thirteenth day after the operation there was hæmorrhage to the extent of about two ounces from the outer angle of the stump wound, the rest of which had united, except where the ligatures were placed. Two days after, hæmorrhage recurred during the night, and, on the stump being opened out, it was seen to proceed from a vessel of considerable size in the periosteum, which was accordingly secured. After this the patient made an excellent recovery.

12. J. C. Caries of the knee-joint, with numerous sinuses discharging profusely. Patient very much emaciated, and excessively weak. Amputation at about the middle of the thigh by the long anterior flap. Patient recovered very rapidly.

13. J. M. Synovial degeneration of the knee-joint. Amputation by the long anterior flap. On the tenth day patient had a rigor, and died of pyæmia on the twentieth day after the operation.

### *Leg.*

1. J. J., æt. 67. Pseudo-malignant tumour of foot, of twenty-five years' growth. Several fungoid ulcers discharging profusely in the skin over the tumour. Patient very weak, and of a cachectic appearance. Amputation below the knee by skin flaps. Patient recovered, and left the hospital very much improved in general health.

2. M. M., æt. 4. Synovial degeneration of ankle-joint, with necrosis of tibia. Patient in a very weak state. Amputation below the knee by a long posterior flap formed by transfixion. The stump healed very rapidly, and the general health of the patient speedily improved.

### *Ankle-joint.*

1. W. R., æt. 8. Caries of tarsus. Amputation by a large internal flap, owing to the existence of a large ulcer on outer side of the heel. Recovered.

2. N. G., æt. 13. Strumous disease of tarsus. Amputation by the ordinary method. Recovery.

3. M. M., æt. 39. Caries of tarsus. Ordinary amputation. Recovery.

4. J. W., æt. 37. Caries of tarsus. Ordinary amputation. Recovered.

5. M. M. Strumous disease of the tarsus. Ordinary amputation. Recovered.

6. J. S. Caries of tarsus. Ordinary amputation. Recovery.

7. J. M. Caries of os calcis. Amputation by a large internal flap, owing to ulceration of the integuments on outer side of the heel. Recovered.

### *Partial Amputation of the Foot.*

D. L. Caries of outer side of tarsus and metatarsus. Removal of the three outer toes, with their metatarsal bones, the cuboid and external cuneiform bones. Recovery.

*Remarks.*—Since the same operation may be performed under a variety of circumstances, and any one case may have some distinctive peculiarity, it follows that no two cases are necessarily strictly analogous. A considerable amount of fallacy may thus lurk in the inferences drawn from the comparison of different classes of amputations, however these may be arranged; still, for the sake of convenience and contrast, we have, as in a former report, classified the amputation cases under three heads: 1*st*, Primary amputations; 2*d*, Secondary amputations in cases of injury; 3*d*, Amputations for disease.

The amputations for injury amount in all to 17, of which 12 were primary, and 5 secondary Of primary amputations there were 2 of the shoulder-joint, 2 of the forearm, 1 of the hip-joint, 2 of the thigh, 3 of the leg, and 2 of the ankle-joint; and of these, 6 cases died, viz., 1 of the forearm (from pleura-pneumonia), 1 of the hip-joint, 1 of the thigh, and 3 of the leg. As regards results, the cases of the upper contrast strongly with those of the lower extremity, for the former may all be considered to have terminated favourably, while some of the latter have been more than usually fatal. The extreme severity of the injuries, the great loss of blood, and the consequent collapse, rendered the case of the boy R. B. extremely unfavourable for the operation, but, had it not been performed, he would very soon have died from the continued oozing of blood; and, under the circumstances, the issue cannot with fairness be estimated in the general result. The great mortality of amputations below the knee, although difficult to account for, may in some cases have been affected by the previous habits of the patient, but no doubt it is generally influenced by the great amount of shock always attending severe injuries, which, although it does not cause death at the time, may so affect the system as to dispose to subsequent pyæmia. The cases of secondary amputation for injury consisted of 3 of the thigh, all of which died, and 2 of the ankle-joint. Those of the thigh thus show a greater mortality than the cases of primary amputation, in which only 1 in 2 died; but it must be remembered that in the cases in which secondary amputation was performed, primary amputation would not have been justifiable. In the case of the girl C. A., a very careful examination of the limb was made, in order, if possible, to discover some cause for the gangrene, but, beyond an unusually small size of the vessels generally, nothing was found which could account for it. The fatal issue in S. C.'s case is not to be wondered at, as the great constitutional disturbance which invariably accompanies acute necrosis is most unfavourable for operation. The two cases of traumatic gangrene, for which amputation at the ankle-joint was successfully performed, present a very striking contrast, as in the one there was a total absence of constitutional disturbance either before or after the operation, while in the other there was severe irritative fever, and delirium which remained long after the healing of the stump.

The secondary amputations for disease amount in all to 25, of which only 2 died; but few cases call for special notice. In the child W. J., amputation of the fore-arm was performed at the tender age of thirteen weeks; and, notwithstanding the previous loss of a considerable quantity of blood, the patient suffered little from shock after the operation, and made a speedy and excellent recovery. Mr Spence having fully reported the case of amputation at the hip-joint in the January number of this Journal for the present year, we would only repeat that the rapid and uninterrupted progress of the stump towards complete cicatrization exceeded the most sanguine expectations, that the general state of the patient seemed to improve every day after the operation, and that she continues to enjoy excellent health. Owing to the state of the integuments in the case of the patient J. C., the thigh was amputated by a modification of the circular method, in order to avoid operating unnecessarily high, but in all the other cases of amputation of the thigh, the operation was performed by Mr Spence's method of the long anterior flap, and the results were most satisfactory as regards safety to life, ease in the dressing, and the formation of useful stumps. Of the two amputations of the leg immediately below the knee, one, that of the patient J. J., illustrates the propriety of operating in pseudo-malignant growths of long standing, even though there be evidence of malignant cachexia well marked, for, after the limb was removed, the patient, though at the advanced age of sixty-seven, speedily regained his health, and lost all appearance of existing dyscrasia. The other was one of those rare cases of disease of the ankle, for which Mr Syme's operation is not suitable; for, as there was extensive necrosis of the tibia, while the dead portions were not loose, and the patient extremely reduced by hectic, immediate operation was imperative. Seven amputations of the ankle-joint were performed for disease with most satisfactory results, of which 5 were performed by Mr Syme's method, and 2 by the large internal flap.

## EXCISION OF JOINTS.

### *Shoulder.*

C. B., æt. 14. Acute necrosis of the upper end of the humerus. On admission, there was a very large abscess in front of the shoulder-joint, and the arm below was excessively œdematous. The patient also was suffering from chronic tubercular peritonitis, while there were signs of consolidation and slight softening at the upper part of the left lung. A free incision into the abscess was made, commencing at a point immediately external to the coracoid process and extending downwards, so that it would suit either for excision of the head of the humerus or amputation at the shoulder-joint, if it should be found necessary. On the abscess being opened and the pus evacuated, the head of the bone immediately started out, and this, with a considerable portion of the shaft which was denuded of periosteum, was removed by the saw. Patient did well for a time, but died of a subacute attack of peritonitis supervening on the chronic disease.

### *Elbow.*

1. C. B. Gelatinous degeneration of the synovial membrane, of eighteen months' standing. Excision by the H method. Cured.
2. F. K. Caries of joint. Excision by H method. Cured.
3. J. W., æt. 9. Synovial degeneration. Excision by H method. Cured.
4. J. A. Synovial degeneration, and suppuration of the joint. Excision by the H method. Cured.
5. A. P. Synovial degeneration. Excision by H incision. Cured.
6. A. S. Cario-necrosis of joint. Excision by H incision. Cured.
7. D. W., æt. 12. Old-standing dislocation. Excision by H incision. Cured.
8. D. C. Caries of the joint, with synovial degeneration. Excision by a single longitudinal incision. This patient made an unusually rapid recovery, and left the hospital with a wonderfully perfect joint.

### *Knee.*

J. P., æt. 24. Caries of the knee-joint. Excision by a semilunar incision, and the limb placed in a Fergusson splint. Considerable oozing of blood from the ends of the bones after the operation. Patient never rallied, but died on the third day.

*Remarks.*—The excisions of joints which occurred during the session comprise those of the shoulder, elbow, and knee. The operation in the case of the patient C. B., first mentioned in the report, may be regarded as one of the removal of a sequestrum rather than the excision of a joint, and was performed under circumstances which scarcely admitted of a favourable prognosis.

From the state of the parts it was impossible to determine whether excision or amputation would be required, and accordingly the incision was made to suit either operation.

There were eight successful cases of excision of the elbow, of which seven were performed for diseased joint and one for an old-standing dislocation. With the exception of one case, where the method by the extended longitudinal incision was adopted, the operation was performed by the ordinary H incision. The degree of motion attained was in every case most satisfactory; and while it cannot be said that in this respect the case in which the operation was performed by the single longitudinal incision was superior to the others, it must be owned that it contrasts favourably with these as regards ease in the dressing, rapidity of cure, and the early period at which passive movement can be commenced.

The case of excision of the knee-joint rapidly proceeded to a fatal termination; but this seems to be an operation which is frequently attended by very great constitutional disturbance, more especially in males. The disease seemed limited to the ends of the bones, and, as regards the age and constitution of the patient, the case seemed favourable to the operation.

(*To be continued.*)

## ARTICLE II.—*Death-rate in the Prisons of Scotland.* By J. B. THOMSON, L.R.C.S. Edin., Resident Surgeon, General Prison for Scotland at Perth.

IT is well known that long after John Howard's days, the state of the prisons of this country was truly disgusting and dangerous. There fever and pestilential diseases were engendered, fatal to the prisoners, and often infecting all who came within the sphere of their influence. Even the judges and members of the courts of law are alleged to have suffered from coming into the neighbourhood of prisons and prisoners. And it is well for the honour of our human nature that we know little or nothing distinctively of the mortality bills of the prisons of those days.

In the early part of the present century, the prisons of England underwent great reforms in their structural arrangements, and in their hygienic and sanatory state; and the physical condition of criminals became much ameliorated. The reforms in England have gradually extended to Scotland; and now our local and county prisons, and the General Prison for Scotland, where long-sentenced prisoners are detained, are conspicuous for the absence of those epidemic diseases at one time so desolating in convict establishments.

Perhaps the most important paper we have upon the Mortality in Prisons is that by Dr Baly, published in the Transactions of the Royal Medical and Chirurgical Society for 1845. In this paper we find that the mortality of Millbank Penitentiary, during the 18 years 1825–1842, was at the rate of 2 per cent. per annum, or 21 deaths per 1000, and this was exclusive of those pardoned on medical grounds. These being estimated to have died at the rate of ⅓ of the liberations, the annual mortality of the Millbank Penitentiary would be 34·209 per 1000, or 3·420 per cent.

About 10 years ago, the average number of deaths of prisoners in Scotland, including a fair proportion of liberated cases of illness, was stated by Mr Frederick Hill, in his work on "Crime," to be about 2 per cent., which approximated very nearly to the ordinary rate of mortality at the same ages in common life. It will be seen that this was somewhat over-estimated, and it will further appear that the ratio of deaths of prisoners has been gradually reduced, owing, doubtless, to the improved dietary and hygiene during the last 10 years.

At the present time, when the treatment of criminals is before the public and the Government as a great and grave subject of discussion, and when a royal commission of inquiry has been appointed by Secretary Sir George Grey as to the working of the penal acts relating to convicts, I have thought it proper to prepare the statistics of mortality in our Scottish prisons as bearing upon this perplexing question. The study of prison life being a specialty in medical science, and confined to very few, I consider it my duty to publish the results of my experience and observation, and should like to see other prison officials follow in the same path. Happily the Reports of the late General Board of Prisons for Scotland afford us very valuable statistics, extending over a period of 20 years. I have had opportunities of verifying their accuracy, and since 1857 I am enabled to speak confidently of the correctness of these Reports as applicable to the diseases and causes of death, etc., in the General Prison for Scotland.

In this paper, what I propose is to ascertain the mortality in the prisons of Scotland.

I. Mortality in the General Prison for Scotland.

II. Mortality in the Local and County Prisons of Scotland.

## I. *Mortality in the General Prison for Scotland.*

This I am enabled to show for 21 years, viz. from 1842, when this prison was regularly opened, until the 31st December 1862. In Table I. it is shown that the total prison population during 21 years was 17541; the sum of the daily average population was 9481; and the number of deaths was 144, being at the average rate of 1·51 per cent. per annum, or about 15 per 1000 prisoners.

### TABLE I.

*Showing the Prison Population, the daily average Population, with the Number and Percentage of Deaths during* 21 *Years, viz. since* 1842, *when the General Prison was opened.*

| Years. | Total Prison Population. | Daily average Population. | Annual Deaths. | Percentage on daily average Population per Annum. | Deaths per 1000. |
|---|---|---|---|---|---|
| 1842 | 404 | 404 | 5 | ... | ... |
| 1843 | 627 | 278 | 6 | ... | ... |
| 1844 | 563 | 217 | 3 | ... | ... |
| 1845 | 541 | 233 | 3 | ... | ... |
| 1846 | 600 | 322 | 7 | ... | ... |
| 1847 | 672 | 332 | 1 | ... | ... |
| 1848 | 682 | 301 | 5 | ... | ... |
| 1849 | 737 | 363 | 2 | ... | ... |
| 1850 | 810 | 385 | 3 | ... | ... |
| 1851 | 784 | 306 | 4 | ... | ... |
| 1852 | 796 | 434 | 8 | ... | ... |
| 1853 | 876 | 459 | 4 | ... | ... |
| 1854 | 1,045 | 580 | 9 | ... | .. |
| 1855 | 1,134 | 498 | 10 | ... | ... |
| 1856 | 998 | 583 | 11 | ... | ... |
| 1857 | 961 | 593 | 12 | ... | ... |
| 1858 | 982 | 561 | 10 | ... | ... |
| 1859 | 1,085 | 632 | 12 | ... | ... |
| 1860 | 1,074 | 675 | 16 | ... | ... |
| 1861 | 1,065 | 637 | 8 | ... | ... |
| 1862 | 1,105 | 688 | 5 | ... | ... |
|  | 17,541 | 9,481 | 144 | 1·51 | 15 |

But the rate of mortality in prisons is affected by a practice more or less prevalent, viz. temporary and conditional liberation in cases where danger to life is certified to result from longer confinement; and the following list shows the numbers liberated on medical grounds from 1842 to 1862 inclusive:—

*Liberated on Medical Grounds in*

| 1842. | 43. | 44. | 45. | 46. | 47. | 48. | 49. | 50. | 51. | 52. | 53. | 54. | 55. | 56. | 57. | 58. | 59. | 60. | 61. | 62. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 12 | 18 | 7 | 0 | 1 | 3 | 1 | 4 | 4 | 3 | 7 | 4 | 3 | 0 | 0 | 1 | 4 | 1 | 2 | 1 | 0 |

The total liberated in 21 years making 76, adding 25 or ⅓ of these as having probably terminated fatally, we should have 144 + 25 = 169; which would bring the death-rate of the prisoners of the General Prison over the whole 21 years to the average of 1·77 per cent. per annum.

### II. *The Mortality in the Local and County Prisons of Scotland.*

Reports of these prisons will be found very full and satisfactory for at least 18 years. The statistics of these prisons are brought up to the end of June in each year, so that they do not exactly tally with the medical reports of the General Prison for Scotland; but the averages are not materially different. It will be observed that the death-rates differ much in the respective local prisons, and these differences I am not prepared to enter upon, but hope attention may be drawn by this table to an explanation of any local and remediable causes which may exist. I have no records of prisoners liberated on medical grounds in the local and county prisons, and probably such must add an item to the annual death-rate. I observe that in several districts, such as Ayr, Fife, and others, smaller prisons have become absorbed into the larger, so that, upon the whole, the table of each county may be regarded as more valuable and complete than the table of the local prisons.

### TABLE II.

*Showing the Average Daily Prison Population, the Deaths and the Percentage of Deaths in the Different County Prisons of Scotland for* 18 *Years, viz.* 1844–1861.

| No. of County. | County. | Average Population. | Deaths. | Percentage of Deaths. |
|---|---|---|---|---|
| 1 | Aberdeen . . . | 1,586 | 14 | ·7 |
| 2 | Argyle . . . . | 394 | 3 | ·7 |
| 3 | Ayr . . . . | 1,553 | 32 | 2 |
| 4 | Banff . . . . | 189 | 1 | ·5 |
| 5 | Berwick . . . | 239 | 1 | ·4 |
| 6 | Bute . . . . | 84 | 1 | 1·1 |
| 7 | Caithness . . . | 117 | ... | ... |
| 8 | Clackmannan . . | 222 | 1 | ·4 |
| 9 | Dumbarton . . . | 430 | 3 | ·6 |
| 10 | Dumfries . . . | 602 | 6 | ·9 |
| 11 | Edinburgh . . . | 8,533 | 79 | ·9 |
| 12 | Elgin . . . . | 180 | 5 | 2·7 |
| 13 | Fife . . . . | 977 | 8 | ·8 |
| 14 | Forfar . . . . | 3,327 | 41 | 1·2 |
| 15 | Haddington . . . | 333 | 5 | 1·5 |
| 16 | Inverness . . . | 443 | 5 | 1·1 |
| 17 | Kincardine . . . | 162 | ... | ... |
| 18 | Kinross . . . | 58 | ... | ... |
| 19 | Kirkcudbright . . | 231 | 2 | ·8 |
| 20 | Lanark . . . . | 11,212 | 143 | 1·2 |
| 21 | Linlithgow . . . | 295 | 1 | ·3 |
| 22 | Nairn . . . . | 30 | 1 | 3·3 |
| 23 | Orkney . . . . | 34 | ... | ... |
| 24 | Peebles . . . . | 76 | ... | ... |
| 25 | Perth . . . . | 1,474 | 20 | 1·3 |
| 26 | Renfrew . . . | 2,429 | 34 | 1·3 |
| 27 | Ross and Cromarty . | 188 | ... | ... |
| 28 | Roxburgh . . . | 397 | 8 | 2 |
| 29 | Selkirk . . . . | 58 | ... | ... |
| 30 | Stirling . . . | 1,451 | 8 | ·5 |
| 31 | Sutherland . . . | 39 | ... | ... |
| 32 | Wigtown . . . | 301 | 3 | ·9 |
| 33 | Zetland . . . | 48 | 1 | 2 |

## TABLE III.

*Showing the Average Daily Prison Population, Deaths and Percentage of Deaths in the Local Prisons of Scotland for* 18 *Years, viz.* 1844–1861.

| No. | Counties. | Local Prisons. | No. | Average Prison Daily Population. | Deaths. | Percentage of Deaths. |
|---|---|---|---|---|---|---|
| 1 | Aberdeen | Aberdeen | 1 | 1,536 | 14 | ·9 |
| | | Peterhead | 2 | 50 | ... | ... |
| | | Huntly | 3 | ... | ... | ... |
| | | Fraserburgh | 4 | ... | ... | ... |
| 2 | Argyle | Inveraray | 5 | 241 | 1 | ·4 |
| | | Campbeltown | 6 | 145 | 2 | 1·3 |
| | | Tobermory | 7 | 4 | ... | ... |
| | | Lochgilphead | 8 | 4 | ... | ... |
| 3 | Ayr | Ayr | 9 | 1,393 | 30 | 2·1 |
| | | Kilmarnock | 10 | 160 | 2 | 1·2 |
| 4 | Banff | Banff | 11 | 186 | 1 | ·5 |
| | | Keith | 12 | 3 | ... | ... |
| | | Cullen | 13 | ... | ... | ... |
| 5 | Berwick | Greenlaw | 14 | 231 | 1 | ·4 |
| | | Dunse | 15 | 8 | ... | ... |
| 6 | Bute | Rothesay | 16 | 84 | 1 | 1·1 |
| 7 | Caithness | Wick | 17 | 117 | ... | ... |
| 8 | Clackmannan | Alloa | 18 | 222 | 1 | ·4 |
| 9 | Dumbarton | Dumbarton | 19 | 426 | 3 | ·7 |
| | | Kirkintilloch | 20 | 1 | ... | ... |
| | | Helensburgh | 21 | 3 | ... | ... |
| 10 | Dumfries | Dumfries | 22 | 602 | 6 | ·9 |
| 11 | Edinburgh | Edinburgh | 23 | 8,485 | 78 | ·9 |
| | | Musselburgh | 24 | 27 | ... | ... |
| | | Leith | 25 | 21 | 1 | 4·7 |
| 12 | Elgin | Elgin | 26 | 180 | 5 | 2·7 |
| 13 | Fife | Cupar | 27 | 689 | 4 | ·5 |
| | | Dunfermline | 28 | 288 | 4 | 1·3 |
| 14 | Forfar | Dundee | 29 | 2,617 | 35 | 1·3 |
| | | Forfar | 30 | 357 | 3 | ·8 |
| | | Montrose | 31 | 246 | 1 | ·4 |
| | | Arbroath | 32 | 94 | 1 | 1 |
| | | Brechin | 33 | 13 | 1 | 7 |
| 15 | Haddington | Haddington | 34 | 329 | 5 | 1·5 |
| | | Dunbar | 35 | 4 | ... | ... |
| 16 | Inverness | Inverness | 36 | 409 | 5 | 1·2 |
| | | Portree | 37 | 6 | ... | ... |
| | | Lochmaddy | 38 | 15 | ... | ... |
| | | Fort-William | 39 | 13 | ... | ... |
| 17 | Kincardine | Stonehaven | 40 | 162 | ... | ... |
| 18 | Kinross | Kinross | 41 | 58 | ... | ... |
| 19 | Kirkcudbright | Kirkcudbright | 42 | 227 | 2 | ·8 |
| | | Maxwelltown | 43 | 4 | ... | ... |
| 20 | Lanark | Glasgow | 44 | 9,763 | 128 | 1·6 |
| | | Lanark | 45 | 312 | 3 | ·9 |
| | | Hamilton | 46 | 634 | 4 | ·6 |
| | | Airdrie | 47 | 503 | 8 | 1·5 |
| | | Carry forward, | | 30,872 | 350 | ... |

| No. | Counties. | Local Prisons. | No. | Average Prison Daily Population. | Deaths. | Percentage of Deaths. |
|---|---|---|---|---|---|---|
| | | Brought forward, | | 30,872 | 350 | ... |
| 21 | Linlithgow . . | Linlithgow . . | 48 | 295 | 1 | ·3 |
| 22 | Nairn . . . . | Nairn . . . . | 49 | 30 | 1 | 3·3 |
| 23 | Orkney . . . | Kirkwall . . . | 50 | 34 | ... | ... |
| 24 | Peebles . . . | Peebles . . . | 51 | 76 | ... | ... |
| 25 | Perth . . . . | Perth . . . . | 52 | 1,342 | 18 | 1·3 |
| | | Dunblane . . | 53 | 132 | 2 | 1·5 |
| 26 | Renfrew . . . | Paisley . . . | 54 | 1,955 | 30 | 1·5 |
| | | Greenock . . | 55 | 463 | 4 | ·8 |
| | | Pollockshaws . | 56 | 11 | ... | ... |
| 27 | Ross & Cromarty | Dingwall . . . | 57 | 141 | ... | ... |
| | | Tain . . . . | 58 | 20 | ... | ... |
| | | Cromarty . . | 59 | 7 | ... | ... |
| | | Stornoway . . | 60 | 20 | ... | ... |
| 28 | Roxburgh . . . | Jedburgh . . | 61 | 394 | 8 | 2 |
| | | Hawick . . . | 62 | 2 | ... | ... |
| | | Kelso . . . . | 63 | 1 | ... | ... |
| 29 | Selkirk . . . . | Selkirk . . . | 64 | 58 | ... | ... |
| 30 | Stirling . . . | Stirling . . . | 65 | 1,418 | 8 | ·5 |
| | | Falkirk . . . | 66 | 33 | ... | ... |
| 31 | Sutherland . . | Dornoch . . . | 67 | 39 | ... | ... |
| 32 | Wigtown . . . | Wigtown . . | 68 | 114 | ... | ... |
| | | Stranraer . . | 69 | 187 | 3 | 1·6 |
| 33 | Zetland . . . | Fort-Charlotte . | 70 | 48 | 1 | 2 |
| 25 | Perth . . . . | General Prison . | 71 | 8,403 | 121 | 1·4 |
| | | | | 46,095 | 547 | 1·1 |

The foregoing tables lead to the following conclusions, viz. :—

1*st*, That the ratio of mortality in the General Prison for Scotland has been, during the 21 years 1842 to 1862 inclusive, at an average of 1·51 per annum, or about 15 per 1000 prisoners.

2*d*, That by adding one-third of the number liberated on medical grounds, we have a death-rate of 1·77 per cent., as the average over the whole 21 years.

3*d*, That the average annual mortality for the 18 years, viz., 1844–61 inclusive, in the county prisons including the General Prison for Scotland, was 1·1.

In a future paper I shall try to analyze the causes of death and what they have to do, if at all, with long confinement and prison discipline. The tables now before us make it very plain that a low rate of mortality prevails in our Scottish prisons, and that the treatment must be good considering the depraved habits and deteriorated constitutions of the criminal classes. Contrasted with the injurious effects of prison life, even half a century ago, we ought to be satisfied that prisoners are not subjected to disease, and fatal results by ill-constructed prisons and starvation dietary; but, when liberated, may be fit to labour and engage in the industrious habits of social men. To this end we have prisons well ventilated, with proper attention to temperature and sanitary requirements,—cells generally containing 700 to 900 cubic feet of air, liberal dietaries, airing and exercise, and scrupulous rules for clothing and cleanliness. There is a tendency in legislation to fly to opposite extremes, but, notwithstanding our favourable mortality reports, it is to be hoped that we may never again revert to the cruel and erroneous systems of a former age, which are more to be dreaded than the careful hygienic and sanitary amenities of the present day. Whatever punitive severities may be had recourse to, let us not neglect due regard to the bodily and mental health of the criminal; always remembering that such conditions are most advantageous to the interests of society at large, as well as to the reformation of the prisoner.

## ARTICLE III.—*On the Pathological Relation between Albuminuria and Puerperal Mania.* By ARTHUR SCOTT DONKIN, M.D.Edin.; M.D.Dur., etc.; Lecturer on Medical Jurisprudence to the University of Durham, Neville Hall College, Newcastle.

MEDICAL science is indebted to Professor Simpson for having first directed attention to the co-existence of *albuminuria* and *puerperal mania*, in a contribution to the Obstetric Society of Edinburgh, in 1856.[1] In this paper Dr Simpson contented himself with demonstrating merely the frequent co-existence of the two morbid conditions, without attempting to explain the exact pathological relation between the renal and mental affections, leaving that to be accomplished by future clinical investigation. Dr Simpson's communication is, I believe, up to the present time the only contribution we possess on the subject, either in our own or any other language.

The following observations on this subject do not apply to those instances of temporary mental aberration sometimes observed during pregnancy, nor to those of brief delirium and incoherence occasionally excited by the sufferings of childbirth, nor to those of insanity manifesting itself during the period of lactation after complete convalescence from the puerperal condition. These, especially the first and last, are all instances of ordinary mental derangement in females predisposed to the affection which is excited in the *first* category by some unexplained influence exerted by gestation on the nervous system; in the *second*, by the sudden shock it receives during the agony of labour; and in the *third*, by its deficient nutrition and exhaustion, resulting from an impoverished blood, or a determination of nutrient materials to the mammary glands.

My remarks, on the contrary, will be strictly confined to *puerperal mania*—the *paraphrosyne puerperarum* of Sauvages,—an affection occurring only during the puerperal period, namely, within a few days or about a fortnight after delivery. On the importance of restricting the term to such cases, Dr Prichard[1] has judiciously remarked that "the states of the constitution during pregnancy and lactation are very different from that of the puerperal period, and the characters of the mental disorder which are displayed at these times are also different. It is, therefore, improper to comprehend all the affections described under one designation and in full; such a proceeding is likely to lead to error both in treatment and prognosis."

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[1] Edin. Med. Jour., vol. ii. p. 766.

If we carefully investigate the writings of obstetric authorities, as well as refer to personal experience, we cannot fail to recognise the correctness of the conclusion at which the celebrated Dr Gooch arrived nearly half a century ago, namely, that there are *two* distinct forms of puerperal mania, to one or other of which all cases can with propriety be referred, *if we found our classification on the presence or absence of certain grave and characteristic symptoms of constitutional disorder attended with great danger to life.* And it cannot be doubted that the too exclusive attention, on the part of observers, to the nature of the mental affection and its various shades and complexions has caused this important principle to be overlooked, and, together with the indiscriminate grouping under one common term of all cases of disordered intellect witnessed in the course of those distinct epochs of the generative process,—pregnancy, parturition, and lactation,—has exercised a most important influence in preventing the genuine affection from being reclaimed to the domain of rational pathology. For there is certainly no subject connected with obstetric science in a more crude and unsatisfactory condition than the pathology of puerperal mania. As to the literature of the subject, I may remark that, since the publication of Dr Gooch's original essay down to the recent contribution of Dr Simpson, already alluded to, nothing has been achieved towards the elucidation of the question.

Now, on a careful investigation of recorded cases, we shall find each of the two groups of the disease to present the following distinctive features:—

*Class I.*—The mania is essentially acute, and runs a brief course; it is always accompanied by a very rapid pulse, sthenic or asthenic, and generally a moist skin. The attack is usually ushered in and attended for some time by pain and heat of head, great intolerance of light, sound, or of any movement in the room, tinnitus aurium, and pervigilium. There is a strong tendency to a fatal issue, death taking place by way of coma or asthenia.

*Class II.* is characterized by complete absence of constitutional disorder; the pulse retains, or occasionally exceeds very slightly, its natural frequency. There is no danger to life, but the mental derangement is generally more or less chronic, and frequently merges into permanent hopeless insanity.

We thus perceive that these two groups or types of the disease are distinguished from each other by characters the most unmistakable and well defined. Consequently, we are led to inquire, *What constitutes the differential pathology between them?* Those cases referable to the "non-constitutional" group, which are dangerous only to reason and not to life, we can readily understand. They are neither more nor less than cases of ordinary insanity, excited, in females predisposed hereditarily or otherwise to the disease, by causes incidental to parturition, such as nervous shock, hæmorrhage, exhaustion, and a variety of other causes which it would be tedious and unnecessary to enumerate. But what are we to understand by the other acute constitutional and fatal class? There is undoubtedly something *special* in their pathology which has not been explained. They have been described by some authorities[1] as instances of cerebro-meningeal inflammation or phrenitis. But this interpretation of their nature is no longer tenable, owing to the fact that in no single fatal case have the traces of congestion or inflammatory action been detected in the brain or its membranes; so that Dr Tyler Smith[2] correctly observes, that "the pathological lesions found after death from puerperal mania do not throw any great light upon the essential nature of the disease. No constant morbid changes are found within the head, and most frequently the only condition found in the brain is unusual paleness and exsanguinity. Many pathologists have also remarked upon the extremely empty condition of the bloodvessels, particularly the veins." Now, this fact is the more remarkable, because the most dangerous and rapidly fatal cases are those whose symptoms simulate most closely those of inflammation of the brain and its membranes. These are the cases which Dr W. Hunter had in view when he stated in his lectures, long ago, that "when puerperal females are out of their senses with fever like paraphrenitis, they will in all probability die."[3] Dr Gooch,[4] referring to the relation between the rapidity of the pulse and the fatality of the disease, pointed out that the frequency of the pulse is the only sure guide in forming a prognosis, and illustrated his observations by showing that "his cases which terminated fatally were all attended with a rapid pulse, while none of those with a slow or moderately-excited pulse died." Subsequent experience has fully corroborated the accuracy of this opinion.

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[1] Lib. of Med., vol. ii. p. 140.

Now, the object I have in view in offering the following observations on this important pathological question, is to demonstrate *that the acute dangerous class of cases are examples of uræmic blood-poisoning, of which the mania, rapid pulse, and other constitutional symptoms are merely the phenomena; and that the affection, therefore, ought to be termed uræmic or renal puerperal mania, in contradistinction to the other form of the disease.* I will endeavour to illustrate this proposition—advanced for the first time—by giving the details of the following most interesting and instructive case, which fell under my own observation a few months ago. And I may here remark, in passing, that the real nature of the case might possibly have escaped detection had it not been for the casual and transient occurrence of facial œdema on the day after delivery.

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[1] Rigby, Syst. of Mid., p. 302.

[2] Man. of Mid., p. 492.

[3] Quoted by Dr Gooch.

[4] Diseases of Women, 2d Ed., p. 116.

The patient was a married woman, æt. 23, and of a short, stout, and robust build; since her marriage, had an abortion, and a miscarriage of a dead fœtus at the sixth month; the latter about twenty-nine months previous to her recent confinement. For a fortnight previous to that event, as I discovered after her delivery, she had suffered from œdema of the feet and legs, together with stiffness of the fingers and puffiness of the eyelids, on awaking in the morning; but during this period she voided daily a considerable quantity of pale urine.

On the morning of the 20th of April, after an easy and natural labour, she was delivered of twins, male and female. There was an interval of twenty minutes between the birth of each, and both were head-presentations. There was no unnatural degree of hæmorrhage after the expulsion of the placenta.

The next morning, the 21st, I was much alarmed on observing her face to be much swollen, puffy, and œdematous, even although she then felt quite well, and there were no symptoms of cerebral disturbance nor excitement of the pulse. Being firmly impressed with the belief, on account of this circumstance, that she was affected with some renal affection, and therefore most probably also with uræmia, I dreaded and predicted the invasion of *renal puerperal convulsions.* She had shortly previous to my visit voided a large quantity of pale urine, which had unfortunately been disposed of; but I requested the next specimen to be preserved for examination.

At 2 P.M. on the same day, I was suddenly sent for, and on arriving found her in a state of great agitation and alarm. She expressed a wish to be held, for, to use her own expression, she felt "swimming away;" she imagined that she had been seized with flooding, and on this account her alarmed attendants had sent for me. There was, however, no hæmorrhage, although the lochia were flowing. She complained of uneasiness and lightness in the head; the countenance was not flushed, but her pulse was fuller and much more frequent than at my morning visit. Believing that she was manifesting the earliest symptoms of uræmic blood-poisoning, and that in all probability uræmic eclampsia would soon supervene, I prescribed a cathartic of two drops of croton oil, and ten grains of colocynth and hyoscyamus pill mass, as the best means of abstracting some of the poison; I ordered the room to be kept cool, dark, and quiet; and in the evening and early part of the night, the purgative acted fully and frequently.

The next morning, the 22d, she was much relieved and quite composed, and the pulse was much quieter. The œdema had entirely disappeared from the face. She had just previously voided nearly two pints of pale straw-coloured urine, and fortunately not mixed with lochia. On examination, it was highly albuminous, and its density 1010; on standing a few hours, a pale flocculent deposit collected, which, on a careful microscopic examination, was found to consist of mucus and epithelium; *but no casts of the uriniferous tubes could be detected*, after repeated searches.

The next forenoon (23d), she was much worse; had not slept during the night, and felt great intolerance of light when the shutters were opened in the morning, and since then saw apparitions of fireballs when her eyes were closed; her pulse was full and very rapid, 130 per minute; her head was hot, with a sensation of throbbing in it; she was suffering much from a drumming noise in her ears, from intolerance of light and sound, while every movement in the room gave her pain. She also complained of what she called "lumps" rising up on her arms and head, and other delusive sensations, which showed that her reason was becoming impaired. She had passed since my last visit nearly two pints of urine of the same quality as on the previous morning. The same dose of croton oil was again prescribed, to be followed by ¼-gr. dose of tart. antim. every two hours, and cold applications to the head.

Two hours afterwards I was sent for, and found her much excited and quite maniacal. Here delirium was not of a violent incoherent kind, but consisted principally of delusions, and much resembled that of delirium tremens. She knew every one in the room, and was sitting on the bed compressing her abdomen violently with both hands, and persisting that there was a second "birth," to remove which it would be necessary to cut open the womb. The medicine prescribed at my previous visit had not been administered in consequence of the sudden invasion of the mania, which had rendered her uncontrollable. Under these circumstances, I had recourse to blood-letting, and after the abstraction of about twenty ounces of blood in the sitting posture, the *force* of the pulse was subdued, and, symptoms of fainting manifesting themselves, the vein was closed. In less than an hour after the bleeding, she became very much more calm, collected, and rational, and felt greatly relieved in her head. The croton oil was now administered, and followed by the antimony, as previously prescribed. In the course of the evening and night the former acted freely, and the latter, though repeated every two hours, produced neither nausea nor vomiting. About 8 P.M., the delirium and excitement greatly increased, but in a milder degree, and continued till past midnight, when she fell into a sound sleep for an hour and a half, and then awoke calm and collected, but still suffering from delusions.

Concerning the blood-letting, it is necessary to observe that I had recourse to it for the following reasons:—*First*, Because of the *fulness* and *force* of the very rapid pulse, and the *urgency of the head-symptoms;* the former condition being completely subdued by it, and the latter greatly mitigated in less than an hour. *Secondly*, Because I considered the case to be pathologically the same as one of sthenic uræmic puerperal eclampsia, in which affection experience has shown blood-letting to be of such essential service.

The condition of the blood abstracted is a most important pathological feature in the case. It formed a firm coagulum, without the buffy coat; its coloured corpuscles were not appreciably diminished in quantity nor altered in character. But the blood-serum was very remarkable, and presented the characters described by Frerichs and others, as witnessed in cases of uræmic puerperal convulsions, in which the serum is saturated with carbonate of ammonia, resulting from the decomposition of retained urea. The serum was of a *strong violet hue*, and quite unlike the colour of healthy serum; its density was 1022. It distinctly effervesced on the addition of acetic acid, and evolved fumes of ammonia on the subsequent addition of liq. potassæ in small quantity. The experiment was made and repeated in the presence of a professional neighbour, about two drachms being employed each time, within a few hours after its removal from the vein, and before decomposition had commenced.

On the 24th (the day after the blood-letting), her pulse was rapid, 125, but soft and compressible. The alarming head-symptoms already described, and the thirst, which before was troublesome, had all subsided. But although not excited, her mental condition was not improved; the same delusions were there; she answered questions with readiness. The antimony was continued.

On the 25th, I found that she had slept two hours in the middle of the previous night, but awoke quite maniacal, excited, and violent; delusions religious; in this state she continued all day; pulse as rapid as ever. Voided to-day nearly three pints of pale urine, density 1010, highly albuminous, and ammoniacal within six hours after being passed.

On the 26th, not excited nor violent, and much more rational; slept twice during the previous night, more than two hours each time. Head quite relieved; pulse not so frequent; passed to-day about two pints of urine of the same quality as yesterday. Antimony and cold applications to head to be discontinued.

From this date up to the 2d of May—a period of seven days—there was little or no alteration in the condition of the patient; her mania was occasionally subject to brief periods of remission, and she had occasionally short intervals of disturbed sleep, without any improvement. She was also at times so violent that it was necessary to restrain her in bed. The pulse continued about 120 per minute, although her skin was moist and perspiring during the whole course of her illness.

From the 26th to the 29th she passed her urine unconsciously in bed, so that it could not be examined. But on the 29th she passed into a vessel about two pints of pale amber-coloured urine; its density had risen to 1020, and the quantity of albumen very considerably diminished, there being just sufficient to render the urine slightly turbid by heat and nitric acid; but it was still alkaline within four hours after being passed.

On the 30th of April and the next two days (1st and 2d May), the urine passed deposited, for the first time on cooling, an abundance of lithates, its density being increased;—there being, however, no improvement in the condition of the patient either as regarded her pulse or her reason.

But on the 3d of May, at my visit, I was informed that she had through the previous night enjoyed a profound and uninterrupted sleep, of about eight hours' duration, attended with profuse perspiration; after which, she awoke with her intellectual faculties almost completely restored, and with her pulse reduced to 96 beats in a minute. The urine passed on this day was loaded with lithates, its density being 1025, with not a trace of albumen.

On the 4th and two following days, she continued to improve, after a sound long sleep each night; so that, on the 7th of May, her pulse was reduced to 78, and her urine had ceased to deposit lithates, although its density continued at 1025, and was not albuminous. From this time she was perfectly convalescent; and, under the influence of a nutritious diet and a preparation of iron, she soon regained her natural colour and strength.

The lochial discharge ceased soon after the invasion of the mania; the milk, though suppressed during the attack, returned with convalescence, and she nursed her elder child at the breast,—the younger, not having strength to suck, soon died.

It will be seen, therefore, that the disease came to a critical termination four days after the kidneys had been restored to a healthy working condition, and the process of depuration had been actively established,—the duration of the mania and rapid pulse having been nine days.

The treatment of the case during the first four days, by blood-letting, purgatives, antimony, and cold applications, I have already detailed; these undoubtedly subdued the force of the pulse and the alarming head-symptoms, the continuance of which might have been productive of serious mischief. During the subsequent five days the treatment was confined to a light farinaceous diet and milk, when she could be induced to take food. Hyoscyamus was twice administered, without inducing sleep; while beef-tea, and the administration of wine on one occasion, produced great mental excitement and increased rapidity of the pulse.

Now, the first or earliest of the series of pathological conditions exhibited by the case just delineated was undoubtedly some renal affection. Of this we have proof in the anasarca which preceded labour several days, in the characteristic pasty-looking facial œdema observed on the day after delivery, and in the albuminous impregnation and very low density of the urine recognised on the day following. It is, therefore, important to ascertain, if possible, what the exact pathological condition of the kidneys was. That they had not undergone any structural alteration, resulting from extraneous organic deposit, as in Bright's disease, is sufficiently evident from the acute evanescent nature of the affection. Again, that it was not an inflammatory condition of the kidneys is, I think, demonstrated by the fact that there was no fibrinous effusion, characteristic of inflammation, from the vessels of these organs; for had there been such, fibrinous casts of the uriniferous tubes, embracing epithelium, etc., and blood-corpuscles, would have existed in the urine in appreciable numbers. Yet the most careful and frequently-repeated microscopical examination of the urinary deposit could not detect them.

The only morbid condition, then, which could have possibly existed under these circumstances was *passive congestion* from mechanical compression of the renal veins, caused by the pressure of the gravid uterus. That such a condition is capable of producing all the phenomena of renal disorder observed in this case will be apparent on a little reflection. In the first place, a certain degree of passive vascular congestion would very seriously interrupt the secreting power of the kidneys, by which they separate from the blood the solid constituents of the urine. In the second place, the quantity of blood circulating through these organs, in a given period to be depurated, would be greatly diminished. In this way uræmia would be speedily induced, and the density of the urine, consequently, very greatly diminished, although there would not necessarily be any diminution in the quantity of its watery constituent, which, however, would be impregnated with albumen. It has been proved, by experiments on the lower animals, that a ligature tied round a renal vein produces albuminuria. Besides, it is an established principle of pathology, that when the vascular system of an organ suffers passive congestion from mechanical venous obstruction, a large quantity of albuminous fluid transudes from the congested vessels,—serous dropsy, in short, is established. In the case of cirrhosis, for example, in which the contraction of Glisson's capsule mechanically obstructs the portal circulation, ascites is rapidly developed; and when, from advanced heart disease, or any other cause, the flow of blood through the inferior vena cava is obstructed, excessive œdema of the lower extremities is the result. In the former instance, the effused fluid accumulates in a serous sac; in the latter, in the meshes of areolar tissue. But in the case of passive renal congestion from a similar cause, the effused fluid accumulates nowhere, but passes directly into the tubuli uriniferi, to be voided by the bladder in the shape of urine. When we recollect, therefore, that the whole of the blood circulating through the kidneys passes, after leaving the arteries of these organs, *first* into the Malpighian tufts, and that the sole function of these tufts of capillaries is to draw off water from the blood, we can have no difficulty in understanding, when they become passively congested, why, as in the present case, the albuminous urine of low density daily passed should be increased rather than diminished in quantity.

That *uræmia* was the *second* link in the chain of morbid changes in this case is evident from the low density and albuminous condition of the urine at a period anterior to the development of constitutional symptoms and of cerebral and mental derangement. If further proof were requisite, it is furnished by the peculiar and distinct violet-colour of the blood-serum, together with the presence in it of a considerable quantity of carbonate of ammonia—a product of decomposed urea; these being the characters which, according to Frerichs, Litzman, Braun, Heller, Kletzinsky, Oppolzer, Gegenbauer, Lehmann, and others, are generally found in the blood-serum of puerperal females attacked with uræmic eclampsia.[1]

It is necessary, next, to consider the morbid phenomena or symptoms observed in the present instance. The most characteristic of these were,—1. The local indications of cerebral disturbance, pain and heat of head, tinnitus aurium, intolerance of light, sound, and of shaking of the room, apparition of fireballs, which ushered in and accompanied the early stage of the mania; 2. The permanently rapid pulse; and 3. The mania itself. As to the first group of symptoms, they certainly resembled those of cerebro-meningeal inflammation. Yet some of the most striking features of that affection were absent, such as a flushed face, injected conjunctivæ, a hot dry skin, persistent nausea, vomiting, and the like. Besides, it is an established fact, that such a group of head-symptoms very frequently ushers in not only attacks of puerperal mania, in which no cerebral lesion can be detected after death, but also very generally attacks of puerperal convulsions, the result of uræmia. It is, therefore, I think, a logical inference to attribute these local symptoms to the previously-existing uræmia, and not to inflammatory action. The combined force and rapidity of the circulation at the commencement of the attack might have exercised an influence in exciting them, but that they were not the effect of its rapidity merely, is, I think, proved by the circumstance, that the frequency of the pulse continued unabated for several days after they had entirely subsided.

The next question to be determined is, to what cause are we to attribute the persistent rapidity of the pulse. That it was neither excited nor maintained by cerebral inflammation is clearly shown by its continuance long after the local symptoms—which in this case merely simulated such an affection—had entirely disappeared. And as neither in the uterus, its appendages and serous investment, nor in any other organ whatever, could the slightest indication of inflammation be detected, it could not possibly have been dependent on that cause. There was certainly no exhaustion nor asthenia to account for it. In the absence, then, of these and every other cause capable of powerfully exciting the heart's action, is it not a logical inference to conclude that the very rapid pulse was the effect of uræmia,—a cause which we know did exist to a very marked degree. It has been already shown that at the very outset of the attack the blood-serum was saturated with carbonate of ammonia, and as the ammoniacal and other morbid conditions of the urine for several days afterwards would indicate, that it continued so saturated, is it not highly probable that this powerfully diffusible stimulant was the cause of the long-sustained vascular excitement witnessed during the course of the disease? That it was the effect of uræmia there can be little doubt; but whether the special agent producing it, as well as the other serious symptoms, mental and local, which accompanied it, was carbonate of ammonia, or some other compound intermediate between urea and its ultimate decomposition into the former substance, or to extractive matter of the urine, I will not venture to express an opinion. This is a most important question, which I trust chemical research will erelong determine, because, if the special morbid agent could be demonstrated, we might possibly be able to discover also an antidote to neutralize its action.

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[1] See Trans. of Braun on Puerperal Uræmic Eclampsia, *Edinburgh Medical Journal*, vol. ii. p. 1029.

The history of the case shows that the intellectual derangement and the rapid pulse were coeval in their origin, duration, and disappearance. Both were suddenly developed at a time when an attack of uræmic eclampsia had been predicted, both continued so long as uræmia existed, and both suddenly subsided four days after the kidneys had resumed their healthy function, and the process of depuration by them had been actively established. These important facts demonstrate not only the correlation or intimate connexion between the mania and the vascular excitement, but also the dependence of both on the same cause, namely, uræmia. That this cause is adequate to the production of such a series of morbid phenomena there can be no reason to doubt, when we reflect on the action of other poisons, whether morbid or artificial, such as alcohol, the virus of typhus, and the like.

There is one special process inseparably connected with the puerperal condition which must exert a powerful, if not a critical, influence in exciting the toxicological action of uræmia, should that condition accompany and follow parturition, or even should it suddenly supervene after childbirth from any acute renal affection. I allude to the *involution* of the hypertrophied uterus. It has been ascertained that the weight of this muscular organ at the completion of labour is from 1½ to 2½ lbs., while, 14 days afterwards, from loss of substance in the interim, it weighs only 10 or 11 oz.[1] Now, this process of rapid diminution and regeneration of the uterus necessitates during its progress, especially when most active in the first two weeks after delivery, the absorption into the blood of an abundance of effete nitrogenous compounds. It follows, therefore, as an unavoidable sequence, that when any renal affection interrupts the elimination of these from the blood, the uræmia to which it had previously given rise will be seriously aggravated in degree, and its poisonous action on the nervous system most probably developed. By this greatly-increased disturbance, then, after labour, of the balance between absorption and elimination of waste materials, may possibly be explained the sudden invasion of the symptoms of uræmia in whatever way manifested during the puerperal period.

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[1] *Vide* Farre on Uterus, Cyclop. of Anat. and Physiol., vol. vi. p. 658.

But the question at once presents itself,—Why should uræmia in the puerperal female produce convulsions and coma in one instance, and mental aberration with a rapid pulse in another? To such a question the reply may with propriety be given, that we cannot expect the poisons engendered in the blood by the retention of the renal secretion to be exempt from the well-ascertained principle of toxicology,—that the action of almost every known poison is modified or entirely changed in character according to the dose, to idiosyncracy, and the like influences. Besides, it is quite possible that in the one disorder a species of poison may be developed different from that which excites the other.

These observations will, I trust, serve to illustrate the principle which I have ventured to advance, namely,—*that the acute dangerous cases of puerperal mania with a rapid pulse ought to be termed renal or uræmic puerperal mania.*

If it should be objected that a single case is not sufficient evidence to establish the accuracy of my views, I must observe that the maxim which applies to investigations in natural history is equally applicable to those of pathology, namely, that although a single typical specimen may not be sufficient to establish a species, it is at least amply so to display its characters. But even should future investigation not corroborate the accuracy of my observations, yet I trust they will effect some good at least by directing others still more closely to the examination of one of the most important and, at the same time, neglected subjects of obstetric pathology. I say neglected, because I can only find recorded five cases of the acute dangerous variety of puerperal mania in which attention was directed to the condition of the urine. Four of these are the cases recorded by Professor Simpson.[1] In all of the four albuminuria existed at the outset of the mania. In one of them puerperal convulsions occurred before delivery, with very marked albuminuria; and after a very brief convalescence and absence of albumen, acute puerperal mania set in, and the urine was again found to be highly albuminous. In another case there were two sudden attacks of puerperal mania, with a week's interval between them, and at the commencement of each attack the urine was loaded with albumen, and free from it in the interval. In one of these cases the duration of the mania was two or three weeks, and the albuminuria observed at the outset of the disease had disappeared entirely before the restoration of the intellect.

The fifth case alluded to is recorded by the late Dr Graves, in the last edition of his Clinical Lectures.[2] This was the case of a young woman (æt. 21), apparently of sound constitution. On the sixth day after giving birth to a seven-months child, she became the subject of acute puerperal mania, accompanied with a very rapid pulse (125), when admitted into the hospital, on the second day of the disease: she had been twice bled previously. She died on the eighth day of the attack, and up to her death the pulse is recorded to have kept as high as 120. Her skin was moist and perspiring, but the lochia and milk were suppressed. There was great pervigilium, she having slept twice, and on one of these occasions, after taking, in frequent small doses, three grains of acetate of morphia, the mania closely resembled delirium tremens. Death took place by way of asthenia. An investigation of the body was made six hours after death, before decomposition could have altered the most delicate tissue. Attention was specially directed to the condition of the brain and uterus. "But," observes Dr Graves, "the most careful examination could discover in the brain no phenomena in the remotest degree capable of explaining the occurrence of delirium or death." He further adds, that "the structure of the uterus was natural, and it exhibited nothing worthy of remark in its interior. The rest of the abdominal viscera were healthy." The kidneys are not separately mentioned by Dr Graves, but he directs especial attention to a symptom which he considered to be of "very considerable importance;" and very justly so, for it was no other than a great "diminution of the urinary secretion." The patient is described as having once voided urine, and that once on the third or fourth day of the attack. Unfortunately, however, her urine was not examined, otherwise the pathology of the case might not have appeared a mystery.

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[1] Edin. Med. Jour., vol. ii. p. 761. Dr Simpson, more recently, states that he has repeatedly seen the same connexion between puerperal mania and albuminuria, as well as had other instances communicated to him. (*Lec.*, *Med. Times and Gaz.*, *Nov.* 10, 1860.)

[2] Vol. ii. p. 301.

NEWCASTLE ON-TYNE, *Feb.* 1863.

## ARTICLE IV.—*On the Nomenclature of Scapulo-Humeral Dislocations.* By JOSEPH BELL, M.D., F.R.C.S.Ed., one of the Demonstrators of Anatomy in the University of Edinburgh.

THE nomenclature of the varieties of dislocation at the scapulo-humeral articulation, has, from the earliest ages of surgery, been one of those questions which have been very frequently brought under the notice of the profession. This has been the result, partly of the importance of the accident itself, and of the interest attaching to it; partly of the difficulties which occasionally present themselves in the diagnosis and treatment of the injury, any mistake in either being almost certain to be followed by grave and permanent bad effects.

It is, however, very remarkable that up to this day the varieties in relative position which (under varying circumstances as regards either the force or direction of the dislocating cause) the articular surfaces may assume, have not yet been thoroughly determined.

The nomenclature, and even the very number of possible dislocations to which the shoulder-joint is liable, are still undecided. Such being the case, any anatomical facts which can be made out regarding the relative position of the articulating surfaces will tend in the direction both of simplicity and precision.

Before entering on these special anatomical points, the briefest possible historical sketch of the more important previous classifications of these injuries is required.

All of these will be found to group themselves under two heads,—

1. *Surgical.*—These, though vague and inexact, are convenient, and for all merely practical purposes are probably accurate enough; the distinctions between the various kinds are very general, and are based upon broad views of the relation in position of the new situation of the head of the humerus to its old seat. Adverbs of position are used, as "forwards," "backwards," "outwards," etc.

2. *Anatomical.*—These are minute to a fault, complicated, aiming at an accuracy which is indeed splendid in theory, but in practice is suited rather to the post-mortem theatre than to the surgical waiting-room. The type of this class is the division of the present French school, headed by Malgaigne, and in it the varieties are differentiated and named according to the position held by the head of the humerus and its relation to the various processes of the scapula.

## *Surgical Classifications.*

Hippocrates lays special stress only on the dislocation into the axilla. Celsus describes two varieties: 1. Into the axilla; and, 2. Dislocation forwards. Galen speaks of dislocation forwards as being very rare, but adduces a case or two. Paulus Ægineta describes three varieties: dislocations downwards, inwards, and outwards.

In later times we find that Ambrose Parè describes four varieties: upwards, downwards, forwards, and outwards; and he takes special care to tell us that a dislocation *backwards* never occurs. Richard Wiseman allows one form of dislocation only, that into the axilla, though he has occasionally found the displaced head in front, under the pectoral muscles; but he regards the latter position as probably only secondary to the first, the result, either of a double injury, or of attempts to reduce the bone.

In 1723, J. L. Petit distinguished four varieties; and his descriptions have more anatomical exactitude than can be found in those of any of his predecessors. The varieties are:—

1. *Inwards*, into the axilla.
2. *Forwards*, under the pectoralis major, between the coracoid and the clavicle.
3. *Downwards*, on the under surface of the scapula.
4. *Backwards*, under the spine of the scapula at the root of the acromion.

The great classical authority of the old French school, Boyer, adopted the following division of Desault:—

1. *Downwards*, on the inferior border of the scapula.
2. *Inwards*, between the subscapular muscle and fossa.
3. *Upwards*, behind the clavicle (very rare, and always secondary to one of the two preceding ones).
4. *Outwards or Backwards*, between the infraspinatus muscle and fossa (so rare that he did not know of a case).

Chelius, whom we may take as the representative of the German school, has the following division:—

1. *Downwards.*
2. *Outwards*, always primary.
3. *Inwards*, often primary, usually secondary.
4. *Inwards and Upwards*, always secondary.

As the most representative examples of the English school at two periods, we may take Cooper and Syme. Sir Astley Cooper has four varieties, three complete and one partial.

1. *Downwards and Inwards*, usually called into the axilla; in which the head of the humerus rests on the inner side of the inferior costa of the scapula.
2. *Forwards*, below the pectoral muscle, where the head of the os humeri is placed below the middle of the clavicle, and on the sternal side of the coracoid process.
3. *Backwards*, where the head of the bone can be felt and distinctly seen forming a protuberance on the back and outer part of the inferior costa of the scapula on its outer side.
4. *Partial*, in which the head of the bone is found resting against the coracoid process of the scapula on its outer side.

Syme has three varieties substantially similar to those of Cooper, *downwards*, *forwards*, and *backwards.*

In looking at all these classifications, we cannot fail to remark the marvellous discrepancy which exists between some of them. To what is this to be ascribed? Does it depend upon inaccurate observation on the part of the authors or of some of them? Or does it not rather depend on the inadequacy of the terms used, especially of such words as "forwards," "downwards," etc., for *distinctive* designation?

When we remember the great mobility of the scapula upon the trunk, and the normal obliquity of the direction of the glenoid fossa, we cannot wonder at the differences of the adverbs used to express relations of parts, which relations in all probability were really the same, but viewed under different conditions of position of either the trunk or limb.

It was this want of precision in terms which made the modern French school, with Malgaigne at their head, adopt a nearly new system of nomenclature, which, for the sake of brevity, and in consideration of its special basis, I shall call the *anatomical.*

Before leaving the surgical systems, one type had better be chosen for future reference and comparison; and of these by far the best, are the classifications (practically the same) of Cooper and Syme, which, without aiming at excessive refinement of accuracy, are at once correct, simple, practical, and comprehensible.

### *Anatomical Classifications.*

Anatomical classifications are such as depend for their distinctive designations on the relation which the displaced head of the bone bears to the osseous prominences of the scapula or to the clavicle. The simplicity and accuracy of the description of the varieties of dislocations at the hip-joint, which by nearly universal consent have received their names on this principle, doubtless gave the cue to this plan; but as yet the shoulder has not been so successfully treated as the hip.

Malgaigne's classification is as follows:—

1. Dislocation into the axilla, containing three sub-varieties.
    1. Complete subcoracoid, common.
    2. Incomplete subcoracoid, rather rare.
    3. Subglenoid, rare.
2. Dislocation forwards.
    4. Intracoracoid, the most common of all.
    5. Subclavicular, rare.
3. Dislocations backwards.
    6. Subacromial, rare.
    7. Subspinous, very rare.
4. Dislocations upwards.
    8. Supracoracoid (Malgaigne knows only two cases).

The classification proposed by Mr Flower, in his contribution to the most recent English system of surgery, is upon the same plan as that of Malgaigne, but modified in the direction of brevity and simplicity. It is as follows:—

1. *Subcoracoid; forwards and slightly downwards; common.* On to the neck of the scapula in front of the glenoid fossa, and immediately below the coracoid process.

2. *Subglenoid; downwards and forwards; rare.* Head of the humerus in front of the inferior costa of the scapula, below the glenoid fossa.

3. *Subclavicular; very rare.* To the inner side of the coracoid process under the clavicle.

4. *Supracoracoid; upwards and forwards; very rare.* On to the fractured coracoid process.

5. *Subspinous; backwards; very rare.* On to the back of the neck of the scapula, beneath the spine or posterior edge of the acromion.

Now, there are several points in these anatomical classifications which deserve notice, and perhaps reprobation.

1. The introduction of the *very* rare and practically useless variety "*supracoracoid*" upon very slight grounds. Malgaigne had *seen* one case only, and that a very doubtful one, as it was two

and a half months after the accident when he first saw it. Two cases have been put on record in this country by Messrs Holmes and Prescott Hewitt, but in both the injury was complicated with fracture. In fact, it is almost impossible to conceive of such an injury without a fracture along with it, and thus such injuries ought rather to be classed under the head of dislocations with fracture, than of dislocations at the shoulder-joint properly so called.

2. The distinction between the subcoracoid and intracoracoid, which Malgaigne has made and Flower has omitted, is founded upon such minute differences, and so many cases are found intermediate, and cannot be classified under either, that it is *practically* useless, and, as I hope to show, it is even in a scientific point of view unnecessary.

In the table at the end of this paper, I have endeavoured to arrange, 1. The typical standard surgical division; 2. Malgaigne's; 3. Flower's modification of Malgaigne's, in such a manner as to show at a glance their mutual relations, and have ventured on adding another system of nomenclature based, like Malgaigne's, on anatomical relations of position, but which I cannot help thinking both simpler and more anatomically accurate.

In the following exposition of this plan of nomenclature, I intend to confine myself to the discussion of the purely anatomical points deduced from the normal anatomy and from dissections of old dislocations, and shall not do more than merely glance at one or two of the more important symptoms.

As I have never had the opportunity of dissecting for myself a recent or even an old unreduced dislocation of the humerus, I have had to avail myself of the dissections of others, as recorded in surgical works, especially of the classical treatises of Sir Astley Cooper and Malgaigne; but I have had ample opportunities of satisfying myself of the correctness of the main facts of my views as far as normal anatomy is concerned in them.

The principle of my plan of division, which I will call *Second Anatomical plan*, is, instead of considering only the relations of the humeral head as a whole to the various scapular prominences, to add to this an inquiry as to what *special portion* (ridge or groove) *of the humeral head is applied to what portion of the scapula?*

I must here premise a single word of anatomical description regarding the posterior aspect of the head of the humerus.

We need here consider only the *posterior* aspect; for in all dislocations of the humerus, except where complicated with fracture or lesion of the integuments, as in machinery injuries, the bend of the arm still looks (in general terms) forwards. We have, then, closely bounding the articular surface of the head, the anatomical neck (Fig. 1, A B C), extending backwards, downwards, and inwards. Immediately outside of and below the upper half of this line (A B), we find the two lower muscular facets of the greater tuberosity for the infraspinatus (E) and teres minor (F). Internal to the lower of these facets, we see, in all well-marked bones, a groove which I think has not hitherto met with the attention it deserves from surgeons (B D). This groove runs nearly parallel to the axis of the shaft of the humerus, and joins the anatomical neck (A B C) at B, at an angle of about 50°. This angle varies with the angle formed by the axis of the head with the axis of the shaft at different ages and in different bodies.

This groove I shall have to mention occasionally under the name of the posterior groove of the humerus.

Now, the great principle of my plan of nomenclature is, that in all forms of dislocation of the humerus in which the injury has been comparatively moderate, and the muscles only stretched, not torn, these two grooves, the *anatomical neck* and the *posterior groove*, are the parts of the humeral head which *alone* can be in contact with the scapula. Their being in contact with the scapula, then, gives us at once a distinction between the more usual and simple forms of injury and those of greater gravity and rarity.

Fig. 1.

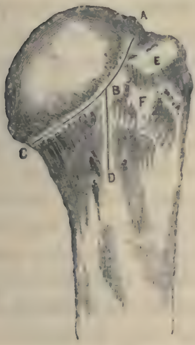
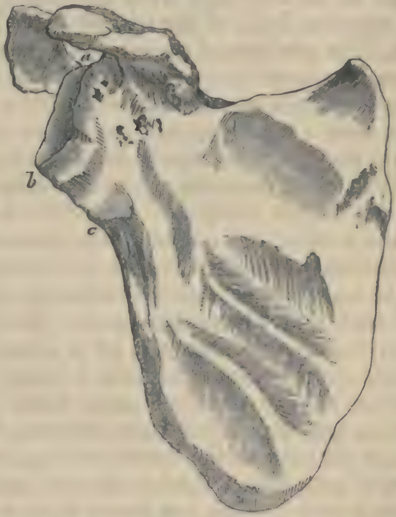


Fig. 2.



But we have here the means of making yet a further subdivision, by asking on what part of the scapula do these grooves rest? I shall endeavour to prove that the anterior glenoid border, and the upper inch or inch and a half of the inferior costa of the scapula, form a sinuous ridge (*a b c*, Fig. 2), on part of which the grooves on the humerus rest in all the more ordinary forms of dislocation without rupture of the muscles; and we can thus make a division into two forms, under the names of *cervico-glenoid* and *cervico-costal*, according as the neck of the humerus rests on the line *a b* or *b c*. Let me now try to prove this.

If we take first a preparation of the normal shoulder-joint with all the muscles removed, but the capsule still entire; rupture or cut the capsule on its *inner aspect* (1), and protrude the head of the bone; —where is it arrested? We find that the articular surface of the head can be protruded so far that either the *anatomical neck or posterior groove* (2) can be made to rest on the *anterior border of the glenoid cavity* (*a b*). The outer portion of the capsule is then put on the stretch, and without further laceration it can go no further. I pause here to prove the two points in the preceding sentence that are in italics.

1. *Inner aspect* of capsule. Why inner? Because in ninety-nine cases out of a hundred, or even in a much greater proportion, the dislocation is either forwards or inwards, and the capsule is found to be ruptured from the lesser tuberosity to the insertion of the scapular head of the triceps. *See* Malgaigne, ii. 468.

2. The position of the *anatomical neck or groove.* This is proved in various ways:—(*a*) By dissections of recent dislocations. Malgaigne records one such, in which "Le col anatomique de l'humerus était retenu sur le rebord glenoïdien, et le trochiter appliqué sur la partie interne et inferieure de la cavité glenoïde." And (*b*) By dissections of old dislocations, as that dissected by Mr Key and recorded by Sir Astley Cooper.[1] In it "the head of the bone was thrown on the neck of the scapula, near the edge of the glenoid cavity, and immediately under the notch of the superior costa." This position of the head will be seen to involve the precise position of the anatomical neck that I have described. But still better marked is another description by Malgaigne of a dissection of a dislocation of old standing, which he figures in his atlas: in it, fig. 2 represents the portion of the humeral neck which corresponds to the glenoid border, and which has been grooved by the pressure; the glenoid border also shows the corresponding effect of the continued pressure.

A point connected with the symptomatology of humeral dislocations, which I think has never been very fully explained, will both give and receive elucidation here.

The obliquity in the direction of the well-marked groove (A B C) of the anatomical neck, if fitted on to the anterior border of the scapula, will explain, in a manner that nothing else will, the tendency which the elbow has to project from the side in all these more usual dislocations.

Take, however, another preparation of a shoulder-joint, but this time retain the special muscles of the shoulder-joint—by these I mean the supraspinatus, infraspinatus, and teres minor on the one side, the subscapularis on the other. This preparation will show much more clearly how it is impossible, so long as these muscles remain unlacerated, that the grooves (A B C D) can do otherwise than fit on to some part of the ridge of the scapula. In such a displacement, while the muscles are not torn, but only put on the stretch in a tolerably equal manner, some parts or other both of the groove and ridge that maintained in mutual contact. All dislocations of the shoulder which fulfil these conditions, *i. e.*, have any portion of the grooves (A B C or B D) in contact with any portion of the ridge *a b c*, I have ventured to include under the one great division of *Cervico-scapular*, and it will be found that far the greater number of these injuries, including all the less dangerous ones, in which the prognosis is favourable and the recovery good—all, in fact, in which the muscles have not been lacerated at all, or only to a slight extent—come under this general head. (*See* Table of Dislocations.)

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[1] Eighth edit. (quarto), 1832, p. 299.

But, I said, this is capable of a still further and a very simple subdivision, according to the position on the ridge of the different parts of the groove. Let us take these in detail.

1. Let us put the bones in position, the muscles being still intact, and place them so that the groove (A B) of the humeral neck rests as high up as possible on the glenoid border (*a b*). On looking at the relations of the rest of the head of the bone, in space, and to the scapular prominences, we find that it is the *dislocation forwards* of the surgeons, the subcoracoid of Malgaigne. In it, Malgaigne tells us, the coracoid impinges on the articular head of the humerus about its middle, a centimetre (·39 of an inch) inwards from the bicipital groove.

Again, still without rupturing the muscles, or displacing our groove from the glenoid border, let a slight change be made in the relative position of the bones: let now the posterior groove (B D) of the humerus rest against the glenoid border, and the difference of position can easily be accounted for by a difference in the direction or amount either of the original force which caused the injury, or the consecutive muscular displacement; looking at the bones in this position, we find the relations are precisely those of the *intra*-coracoid dislocation of Malgaigne, *i. e.*, two-thirds or three-fourths of the articular head have now passed within the coracoid process, the tip of which directly overhangs the upper end of the bicipital groove. Besides the evidence gained from dissection of the normal parts concerned, the correctness of this view is, I think, strikingly confirmed by the following facts:—

1. The symptoms of intracoracoid dislocation, as given by Malgaigne, are very similar to those of subcoracoid, with this special exception, that the obliquity of the axis of the arm and the separation of the elbow from the side are much less marked in the former than in the latter. Now, this is very well explained (and no other surgeon has, as far as I can discover, ever attempted to explain these differences in the symptoms in this manner) if we remember the great obliquity of the anatomical neck in relation to the axis, which is the part involved in subcoracoid, and the comparative parallelism of the posterior groove (B D) to the axis, which is the part involved in the intracoracoid, a difference in direction equal to about 40° to 50°.

2. Cases such as the following, of ancient luxation, actually prove at least the occasional presence of this position. It is from Malgaigne (ii. 515). I quote a small portion only of the description of the preparation. "The greater tuberosity was separated from the head behind and above by a large deep sulcus, which above was not less than ·589 of an inch (15 millemetres) in depth. The coracoid corresponded in position to the upper part of this groove, which indeed it seemed to have aided in forming. The head of the humerus was flattened at its posterior part, and played on a concave articular surface, hollowed out at one part at the expense of the glenoid border.

3. I now quote a case which, besides being interesting from some rare points in its symptoms, is a good example of a class of cases which I think go far to prove the relation of this posterior groove of the humerus, on which I have insisted so much, to the glenoid border in certain forms of dislocation. It is one of those cases where, in addition to the primary dislocation, there is a consecutive fissure of the head of the humerus. How is this fissure produced? Not by any peculiarity in the direct violence which caused the injury; not by any conceivable action of the muscles; but by the sharp edge of the glenoid border being forced as a wedge against the posterior groove of the humerus. This is, I hold, the only rational manner in which we can explain such a complicated lesion, and in relation to the question of position, this case greatly impressed me.

A lad of about 16 years of age, spare, but tolerably muscular, was thrown to the ground with great violence on his right shoulder in a playful scuffle, and while down, was unavoidably trodden upon by the others.

When I saw him, about an hour after the injury, his shoulder was very much swollen and painful. The arm lay by his side, and the elbow was *slightly* separated. The head of the humerus could be felt most distinctly in its new position, immediately below the coracoid, but further *inwards* and forwards than is usual in dislocations forwards. The contour of the shoulder was not much changed, the deltoid less flattened than usual, and the great tuberosity could be felt to be not much out of its normal position, but certainly at a greater distance from the lesser tuberosity than usual. In fact, the breadth of the upper end of the humerus was increased. The lesser tuberosity was remarkably prominent, considering the amount of swelling, and its outer edge was felt under the skin to be sharp and defined. The most curious and characteristic symptom—one which I had never seen before, or read of—was a puckering of the skin just outside of this edge of the lesser tuberosity. The skin was actually drawn inwards as if nipped or bitten from below, forming a very evident dimple with puckered edges.

The diagnosis was made that this was a dislocation forwards, complicated with a longitudinal fracture separating the greater tuberosity from the head and the lesser tuberosity, though, owing to ligamentous and muscular attachments, the displacement was not great. This accounted for the position of the head, and its greater apparent breadth, while at the same time the shoulder was not flattened. No evidence by crepitus, shortening, or otherwise, of any absolute separation of either fragment from the shaft could be obtained.

The dislocation was easily reduced under chloroform by the heel in the axilla, and the curious puckering of the skin at once disappeared when the head was extricated from its abnormal position, though the portion of skin involved became a centre of intense ecchymosis. The case has hitherto gone on most satisfactorily. The fracture has united with no deformity, except the slightest thickening of the head, and motion at the scapulo-humeral joint is now (four months after the accident) almost normal. The position of the groove (A B C) of the humerus on the edge of the groove in dislocation explains in a most satisfactory manner the causation of those not uncommon, but hitherto rather inexplicable injuries, I mean fractures of the anatomical neck of the humerus.

Again, there is a third mutual relation of our posterior groove and neck of humerus, and the sinuous scapular ridge which may be present without any rupture of muscles. This is when some part of the line (A B C) is now in contact, not with the anterior glenoid border, but with the line (*b c*) on the upper edge of the inferior costa, or axillary border of the scapula. This is the *dislocation downwards* of the surgeons, the subglenoid variety of Malgaigne—my *cervico-costal.*

In proof of this I may adduce the following considerations,—1. If we lay the head of the humerus in the position defined by Syme, *i.e.*, resting on the neck of the scapula, over the origin of the long head of the triceps, the neck corresponds to the edge. 2. One of Sir Astley Cooper's cases, recorded in p. 286 of his eighth edition, defines pretty accurately this position of the head. It lay on the inferior costa of the scapula, while the supraspinati and infraspinati were stretched over the glenoid fossa, the teres minor was little affected, and the subscapularis was slightly lacerated. 3. This is specially confirmatory of my view. Malgaigne, with all his love for an exact nomenclature, states (op. cit. 502) that, even so long ago as the year 1836, he had noticed that, between this form of dislocation (his subglenoid) and the preceding one in his division (his subcoracoid), there existed intermediate shades, the humeral head being detained at distances below the top of the coracoid, varying from two to three or four lines; the distance being determined in each case by special circumstances, such as the position of the rent in the capsule, the amount of consecutive muscular action, and the like. Malgaigne does not by this explain, or attempt to explain, why all these intermediate shades of his, though varying in position, are all on the same, or nearly the same, vertical plane.

This can be explained, I hold, by the fact which I have been trying to make out, namely, that in the cases where the proper muscles of the joint are not completely lacerated, *they must keep the neck and posterior groove of the humerus* (one or other of these) fixed on some point of a sinuous, but still tolerably vertical line, *i.e.*, the *anterior edge of the glenoid continued into the upper inch of the inferior costa of the scapula.*

So far, then, we have an anatomical bond of union in this relation between the neck and groove and the scapula, which includes all the more ordinary dislocations of the humerus, all those in which the violence of the dislocating cause has not been very great, or the displacement excessive; and the name, common to them all, of *cervico-scapular*, if not very euphonious, has the merit of being both simple and expressive of this anatomical fact. But we have, again, another subdivision based also on anatomy, according to the position which, as we have already seen, the neck or posterior groove may take on the sinuous scapular line.

When fitting on above the lower corner of the glenoid (on line *a b*), *cervico-glenoid* will include the subcoracoid and intracoracoid of Malgaigne, the dislocation forwards of the surgeons; when fitting on below that corner (on line *b c*), *cervico-costal* will correspond to the subglenoid of Malgaigne, and the dislocation downwards, or into the axilla of the surgeons.

So far for the more ordinary dislocations; but this method of nomenclature applies with even greater simplicity and anatomical accuracy to the other rarer varieties in which the displacement is more excessive.

Dislocation backwards of the English surgeons is a very rare injury, and includes Malgaigne's two varieties of subacromial and subspinous.

Such displacements are generally the result of very severe injuries, and the exact position of the head of the bone varies much in different cases.

Great discrepancy exists in the accounts of various authors regarding the length of the limb, as to whether it is shortened or elongated. This is explained by the correspondingly great variety in the positions in which the head has been described as lying in the various cases; and if the point which I now wish to prove be allowed, this various length is exactly what, anatomically speaking, we should expect; for *all* the cases of dislocation backwards have this one common character, that the *neck and posterior groove have no relation to the sinuous line on the scapula, but are completely separated from it*, and this having no special ridge of bone to fix its position, the head of the bone is now at the mercy both of the dislocating force and of the secondary muscular contraction, which latter is limited only by the position of the spine and acromion.

Proofs that this displacement really does occur are found,—(*a*) In the altered axis of the arm, which is quite incompatible with any possible mutual relation of the groove and edge; the arm in these cases being usually directed forwards against the chest. (*b*) In thin individuals especially, the head can be easily felt in its new position. (*c*) We have already seen that the proper muscles of the shoulder-joint, so long as they remained intact, were the agents by which the groove and sinuous ridge are kept in mutual contact. Now, to permit it to go off the edge (as can be shown in any normal preparation) one or both sets must be destroyed, either lacerated or torn from their insections. Again, if the displacement is to be in an outward direction, it must be the inner muscular plane, *i. e.*, the subscapularis, which is torn or displaced. Now that this does occur in dislocation backwards, is shown in many recorded cases. 1. In one recorded by Malgaigne (op. cit., p. 537), he says, "the capsule was lacerated behind, and, strange to say (we now do not think it at all strange), the tendon of the subscapularis est décollé de la petite tubérosité, sans doute par sa rencontre avec le rebord glenoïdien." 2. In one of seven years' standing, seen by Sir Astley Cooper, the tendon of the subscapularis was ruptured at its insertion, and the head of the bone lay on the outer border of the glenoid cavity, which it had sensibly depressed, and had itself been flattened at the point of contact. A name such as *extrascapular* or *extrajugal* may be chosen (if an anatomical nomenclature be required at all) to express anatomically the fact which specially underlies all the varieties of position in these rare dislocations, namely, that by laceration of muscles, the neck of the humerus is now completely separated from—and is to the outside of—that sinuous ridge, which in all the previous varieties had supported it, and limited its possible positions.

As in the preceding cases we found the humerus dislocated backwards with great displacement, there is yet another set of cases which cannot be included under any of our previous heads, and these are by no means very rare, in which the displacement is also very great, but in the opposite direction, forwards and inwards. Of these Malgaigne has collected a very motley group, under the general title of *subclavicular;* but this, in many of the cases, is by no means descriptive of the position. In general, he tells us, in such cases the head of the bone makes an apparent prominence in front, so that the clavicle appears to be carried a little forwards, and the supraclavicular fossa seems to be deepened.[1] Occasionally the head of the bone is under the thickest portion of the great pectoral.[2] Again, the head of the bone has been found covered by the skin only, having made its way to the surface by separating the deltoid and pectoralis major close to the cephalic vein.[3] In another, the head has been even pushed through the integument into the hollow of the armpit.[4] In other cases the head has been found under the scapula in such a manner as to project the lower

[1] Malgaigne, op. cit., p. 524.

[2] Hey of Leeds, case 6, p. 312.

[3] Malgaigne, loc. cit.

[4] Hey, loc. cit.

extremity of it outwards.[1] And in yet another, recorded by Pinel and quoted by Malgaigne, the head of the humerus was situated at the inner side of the scapula, between the subscapularis and the upper border of the scapula, only an inch and a half from the sternal end of the clavicle.

I have quoted these few cases, not as an exhaustive *resumé* of all possible positions of the head of the humerus in such abnormal displacements forwards, but merely as illustrations, recorded by authors whose powers of observation are beyond all doubt, of a few of the very various positions in which the head may be found, and they might easily be multiplied almost indefinitely. Yet all such varieties have been classed by Malgaigne under the one head—subclavicular, and have been included, in ordinary surgical nomenclature, along with the much more common and far less dangerous cases of dislocation forwards. In my plan of nomenclature all these cases should be classed together, not as by Malgaigne, under a name expressive of a distinct anatomical relation to the clavicle, which they do not all exhibit, but under one which pretends to no more accuracy than that it expresses the one great underlying fact which is common to them all, that the neck has been displaced from the sinuous ridge of the scapula, and this time it is *inwards*.

Anatomical evidence of this is hardly necessary; one point, however, deserves notice. To permit such a displacement, the other set of muscles, namely, the supraspinatus, infraspinatus, and teres minor, must be either lacerated or detached. There are comparatively few dissections of such cases recorded, but such lacerations are invariably present. They are well recorded in one of the cases quoted from Hey of Leeds, in which he had obtained a dissection; and at the same place he quotes another case with similar laceration, recorded by Mr Thompson. I propose the name intrascapular or intrajugal as including all such cases, and yet separating them quite enough for all practical purposes from the more ordinary and less dangerous forms.

These observations are published, and this new system of nomenclature proposed, not from any idea that in ordinary practice, or, in common surgical parlance, any change is required from the time-honoured nomenclature of Cooper, but rather from the feeling that as an anatomical system of nomenclature has been proposed (and no doubt as regards scientific accuracy it has its advantages), the system of Malgaigne and his followers, including Mr Flower's modification, is not satisfactory, being at once redundant and deficient. Besides, I have endeavoured in this paper, and especially in the tabular view of the systems which follows, to bring together and reconcile as far as is possible the different systems, as, from practical experience, I have been made aware of the great difficulties at present in the way of a student in his study of the symptoms and treatment of dislocation at the shoulder-joint, from the transition state in which at present the subject of the nomenclature is, and the consequent apparent discrepancy in statements made by the old and new school even about matters of fact.

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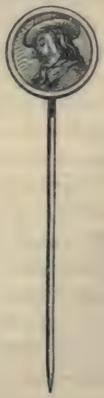
[1] White of Manchester, vol. i. p. 101.

In illustration of the preceding remarks, I append a table which exhibits in a condensed form the opinions entertained by previous writers regarding the terms used to designate scapulo-humeral dislocations, together with the nomenclature proposed by myself.

*Nomenclature of Scapulo-Humeral Dislocations.*

| SURGICAL. | ANATOMICAL (founded on Scapular Relations only). | | ANATOMICAL (on Scapulo-*Humeral* Relations). | |
|---|---|---|---|---|
| (*Cooper* and *Syme*.) | (*Malgaigne*. | *Flower*.) | (*The Author*.) | |
| Downwards, into axilla. | Subglenoid. | Subglenoid. | Cervico-scapular. | Cervico-costal. |
| Forwards. | Subcoracoid complete.<br>Subcoracoid incomplete.<br>Intracoracoid. | Subcoracoid. | | Cervico-glenoid. |
| | Subclavicular. | Subclavicular. | Intrascapular. | |
| Backwards. | Subacromial.<br>Subspinous. | Subspinous. | Extrascapular. | |
| | Supracoracoid. | Supracoracoid. | | |

## ARTICLE V.—*Case in which a Breast-pin was swallowed by a Child, and passed by Stool twenty hours after.* By THOMAS ANNANDALE, F.R.C.S.E., M.R.C.S. (Eng.).



ONE day in the month of January 1863, a respectable tradesman in Newcastle came to my father in great consternation about his son, who had just swallowed a breast-pin. It appeared that the child, three years of age, had suddenly begun to cry, and the father's attention being directed to the boy's throat, he saw the sharp point of the pin sticking up from between the fauces. The father made an attempt to seize it with his fingers, but failed, and the pin passed down out of sight, and caused no further inconvenience. The accident happened immediately after the child's dinner, at twelve o'clock. The child had had for his dinner one or two mouthfuls of roast-beef, and had partaken freely of a rice-pudding.

The parents were advised to keep the patient quiet, and carefully watch his stools.

The child suffered no inconvenience, and next morning, about half-past nine o'clock (twenty hours after the accident happened), he had a motion from the bowels without any pain. In this stool the pin was found, of which the annexed woodcut is an exact copy as to shape and size.

*Remarks.*—It very rarely happens that a sharp substance, when swallowed, passes so safely and quickly through the alimentary canal as in the present case. Such substances, when not arrested in some portion of the pharynx, œsophagus, or intestinal canal, often become lodged in the rectum, and give rise in this, as in other portions of the canal, to serious consequences.

The most remarkable instance on record, of foreign bodies of a dangerous character being swallowed, is the well-known case reported by Dr Marcet, of a sailor, who, in the course of a few years, swallowed no less than thirty-six clasp-knives, several of which were passed by stool.[1]

In South's edition of Chelius's Surgery, there is a case related by Dr Barnes, in which a juggler accidentally swallowed a table-knife with a bone handle, and died in consequence. At the post-mortem examination, the knife was found beginning to protrude through a gangrenous opening in the commencement of the duodenum.[2]

M. Velpeau communicated to the Academy of Medicine, in June 1849, the account of a case, where a farmer accidentally swallowed a tinned-iron fork, 8 inches long and 1 broad; twenty months after, he passed the greater portion of it by stool,—the rest appears to have been dissolved while in the stomach and intestines.

Another case, related in Chelius's Surgery, was that of a peasant, who, while endeavouring to procure vomiting with the handle of a knife, let it slip, and it passed down. About two months after, the knife was removed from the stomach by gastrotomy, and the patient recovered. The knife was 6½ inches long.[3]

In the Transactions of the Pathological Society, there is an account of a girl, æt. 7, in whom a swelling formed a little below the umbilicus, from which feculent matter was discharged, and afterwards a hair-pin came out.[4]

In the Med. and Chir. Trans., there is the case of Mrs B., in whose stomach there was found, after death, a mass of pins weighing 9 oz., and in the duodenum a similar mass, weighing one pound.[5]

In the Lancet for 1851, there is a case, related by Mr Gast, of a girl who threw up and passed by stool a great number of pins, some of which gave rise to abscesses in her throat.[6]

At a meeting of the Medico-Chirurgical Society of Edinburgh on the 5th of January 1859, Mr Jardine Murray related the account of a case in which he removed from the pharynx a needle that had perforated the neck. At the same time, Dr Gairdner mentioned three cases in which fish-bones had perforated the alimentary tube, and given rise to suppuration and other serious consequences.[7]

In the Lancet for 1860, there is an account of a case where a fish-bone had penetrated the walls of the stomach, passed through the diaphragm and pericardium, and entered the substance of the heart.[1]

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[1] Med. and Chir. Trans., vol. xii.

[2] South's edition of Chelius's Surgery, vol. ii. p. 388.

[3] Loc. cit.

[4] Trans. Path. Society, vol. vii.

[5] Med.-Chir. Trans., vol. xxxv.

[6] Lancet for 1851, vol. i.

[7] Edin. Med. Jour., vol. iv. p. 769.

In the Medical Gazette for 1842–43, there is a case related by Mr J. Bell, in which a sewing-needle, after being swallowed, had perforated the right common carotid artery, and given rise to fatal hæmorrhage.[2]

Cases have also been reported in which pins and needles, after being swallowed, have found their way out through the external tissues of the body. My friend, Mr Turner, Senior Demonstrator of Anatomy in the University of Edinburgh, informs me that he has several times met with cases in the dissecting-rooms where needles have been found lying in the tissues of the body. There are also many cases on record where fish-bones, game-bones, and other sharp substances, have been removed from the rectum, after having passed through the rest of the intestinal canal.

The explanations of the safe passage of the pin, in the present case, appear to me to be the following:—

1*st*, The rounded and blunt form of the head of the pin, which was the first portion to descend into the pharynx.

2*d*, The length of the pin.

3*d*, The fact that the pin was swallowed immediately after a hearty meal.

4*th*, The nature of the substances taken at the meal.

The shape and size of the head of the pin would prevent its being arrested in its passage downwards into the stomach, especially as this portion was the first to enter the tube.

The second cause, namely, the length of the pin, would also tend very much to guide the body safely into the stomach. If the pin had been so short as to admit of its turning round in the lower portion of the pharynx or œsophagus, it might very easily have become arrested in either of these situations. A short time ago, a child was brought to me by its mother, who said that it had swallowed a pin a few minutes before. On introducing my finger into the patient's throat, I felt the pin lying across the lower part of the pharynx, and fixed in that situation. I passed down a pair of common dressing forceps, and easily removed the body. It proved to be an ordinary pin about one inch in length.

When the pin, however, had arrived at the stomach, there was plenty of room for it to move freely about, and permit the sharp point to become fixed in some portion of its walls. We must therefore look to the other causes in order to account for the further safe passage of the pin through the intestinal canal.

It is quite reasonable to suppose that the pin, when it entered the stomach, became embraced by the food just swallowed, and was thus enabled to follow without danger the various movements which the contents of this organ undergo during the process of digestion. In the same manner, it might pass out of the stomach into the intestinal canal, and, being there protected by the ingesta, continue its passage until it became invested by fæcal matter, when it was discharged by the rectum along with the patient's stool.

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[1] Lancet for 1860, vol. ii. p. 186.

[2] Med. Gazette, 1842–43, p. 694.

If the stomach had been empty, it is difficult to imagine how the pin could have escaped without becoming lodged in some portion of its coats.

The fourth cause, no doubt, assisted in the safe termination of this case. There could scarcely have been a better substance than rice-pudding for forming a protective covering round the foreign body.

Swindlers in London, who often swallow false coin when detected in the act of passing them, treat themselves in the following manner:—"The treatment usually pursued by the man in his own person is peculiar and not irrational. He avoids purgative medicines as worse than useless; on the other hand, he has recourse to a constipating diet, and feeds for some days on hard-boiled eggs and cheese in excess, beyond his usual diet."[1]

In South's edition of Chelius's Surgery, there is related the case of Sir F. Chantrey, who accidentally swallowed the gold fastening of one of his teeth, and, by way of favouring its passage through the bowels, he ate freely of suet-pudding. Nothing more was heard or seen of the tooth-plate, and whether it passed or not is unknown, but it never gave any inconvenience.[2]

In regard to the time that elapsed (twenty-hours) between the swallowing of the pin and its discharge by the rectum, I can find very few instances on record in which this interval was so short.

In the case of the sailor before-mentioned, one of the knives was passed by stool twenty-four hours after being swallowed.[3]

Dr Gray has seen a pin reappear, per anum, four days after it was swallowed.[4]

Dr Albert discovered a pin in the alvine discharges of a child, on whose face the mother had accidentally dropped it two weeks before.[5]

In the Lancet of February 14, 1863, in a letter signed "A Public Vaccinator," a case is mentioned, where a girl, twelve months old, swallowed a common flat tin whistle, and passed it by stool twenty-one hours after.

*Treatment.*—When, therefore, a foreign body of a sharp or dangerous nature has been swallowed, and has passed beyond the reach of operative measures, the proper treatment appears to be,—1*st*, The administration of that kind of dry food which will best form a protective covering around the body; 2*d*, The avoidance (in the first instance) of all purgatives, medicinal and otherwise. The patient's stools should also be carefully examined, in order to ascertain if the substance passes by this channel.

[1] Holmes's System of Surgery, vol. ii. p. 467. [2] Loc. cit. [3] Loc. cit.
[4] American Journal of the Medical Sciences, vol. xxix. [5] Loc. cit. p. 58.

# Part Second.

## REVIEWS.

*Clinical Memoirs on Diseases of Women.* By ALFRED H. M'CLINTOCK, M.D., etc., etc. Dublin: Fannin and Co.: 1863.

*A Handbook of Uterine Therapeutics.* By EDWARD JOHN TILT, M.D., etc., etc. London: Churchill and Sons: 1863.

TWO well-known authors appear simultaneously presenting to the profession elaborate works on the diseases of women. This comparatively modern or recently isolated field of medical science and practice, the diseases of women, has its interest and importance amply attested by the copious authorship which increases upon it. Under these circumstances, it is the duty of the critic to aid and to guide the profession in judging the new writers, who are ambitious to teach, as well as to exert an influence on the authorship itself in the way of encouraging, correcting, or repressing. For these purposes we have taken these two works together, and they are well adapted for a short notice, being both characteristic productions of an era of great activity in all departments of science.

"Medicina tota est in observationibus" is the appropriate motto of the carefully prepared and philosophical work of Dr M'Clintock. From its first page to its last it exhibits that clinging to facts which is the best security against all kinds of license and of looseness so common in medical authors; and it also shows that carefulness in appreciating what is and what is not a fact, without which an author may be clever, ingenious, or speculative, but can never claim the high character of a good observer. Good observations abound in Dr M'Clintock's work; they form the foundation of all that is valuable in medical science, and they are far more rare than we believe to be generally supposed.

The work of Dr Tilt is written on quite a different plan from the work of Dr M'Clintock. It contains few observations, and these of little value. The book is not based on observation, and it is a waste of space to insert histories which prove nothing, and for the most part illustrate nothing.

The work of M'Clintock is one which will bear careful scrutiny, and is of permanent value. It is written in the style of a scientific book. It aims at presenting no startling novelty or ingenious hypothesis, but is the plain production of one well versed in gynækology, presented to his brethren of the same science and art, and it will command their attentive perusal.

Dr Tilt's book differs from it *toto cœlo*. It forms a clever and valuable exposition of a kind of practice which is well seen only in large towns and in wealthy communities. As such, it is interesting and important, and many members of the profession will be grateful to Dr Tilt for collecting together and arranging so great a mass of therapeutical resources.

It is undeniably true that the number of therapeutical measures recommended for any disease is in proportion to the obscurity of its nature and progress, and also to the inefficiency and uncertainty of the medicines. Dr M'Clintock's work treats of pelvic inflammation and abscess, procidentia uteri, inversio uteri, fibrous tumours of the uterus, polypus of the uterus, tumours of the vagina and vulva, pelvic, pudendal, and uterine hæmatocele, stone in the bladder, mammary inflammation and abscess, secondary hæmorrhage after parturition, dropsy of the ovum, cystic disease of the ovum. These are not diseases that strongly affect ladies living in ease and idleness. They are all substantial tangible entities, too important to escape careful study, too serious to be trifled with or treated by any kind of juggling with drugs. They are among the gravest, the most painful, and the most dangerous of the diseases of women. But they are not excitors of those agonies, tortures, and excruciations which are perfectly familiar symptoms of what may be called the minor diseases of the uterus, and which so often form a cherished part of the wretched livelihood of women in idleness and ease. They are maladies not to be dallied with by novel prescriptions, ingenious bread-pills, or by a mere skilful withdrawal of the attention from the disease.

The diseases of women, to which the work of Dr Tilt is mainly addressed, are of a kind often designated under the name of the minor uterine ailments. Not long ago, many of them would have been confused together and treated as hysteria, or whites, or piles, or biliousness, or dyspepsia, or mere weakness, or fidgets, or ennui, or utter fudge. Now they receive different designations from patients and practitioners. They are called ulceration, version, flexion, squatting, eruptions, spasms, stricture, catarrh, pruritus, congestion, hypertrophy, neuralgia of the ovaries, uterus, vagina, or vulva, as the case may be. These names indicate real acquisitions of modern science, and the field of many noble achievements of modern practice. But they indicate also a rich field for female curiosity, suspicion, alarm, and claim for sympathy. They give unlimited scope for unscientific, empirical, haphazard, ever-varying treatment. These names indicate a class of cases in which the name is sometimes the great point, and in which the mere name of the medicine is, in due correspondence, the great point. These diseases require the utmost skill of the physician to diagnose, on account of their frequent obscurity, on account of the frequent triviality of all that is discoverable by the senses, aided or unaided, and on account of the great amount of reliance that must be placed on the statements of the patient, be they honest or dishonest, plain or varnished, exaggerated or the reverse. They also require the utmost wisdom in their treatment. It is to cases of this kind that Dr Tilt's book chiefly refers, and its tenor indicates his ardent wishes as much as his deliberate convictions. Practitioners will find in it abundant suggestions, many sagacious advices, and often a spirit of wise caution against remedies that are dangerous in a degree not justified by the gravity of the disease treated.

But we wish that Dr Tilt had been more reserved, or cautious, or reticent in his therapeutical suggestions. Our brethren with scanty whiskers or snub noses will despair when they read that in treating the diseases of women they "must not be ugly." Our busy brethren will avoid this specialty when they read that they must reason their patients, not laugh them out of their fancies, however ridiculous—Sisyphus like. Our worthy, guileless brethren will shrink from a profession in which they can succeed only by a kind of play-acting, the direction of the look, the tone of the voice, and every point in outward manner and expression being done by regulation. Readers, in general, will be interested by the curious measures recommended or noticed in some cases; powdering the sheets with camphor; sponging the head with camphor water and eau de Cologne when the symptoms indicate a tendency to insanity; eau de Cologne, with as much camphor as it will dissolve, rubbed into the scalp for pseudo-narcotism, amounting to stupor; "the occasional use of matrimony;" the Chelsea pensioner, an air-pessary curing amaurosis, etc.

Let us now turn for a little to the work of M'Clintock, and open it at any part. To take a somewhat novel subject, we fix on an interesting chapter on uterine or pelvic hæmatocele. Many practitioners will join with him in calling to mind cases of this recently re-discovered disease, which, at the time, were obscure and perplexing, but which he now has little doubt were examples of pelvic hæmatocele, and upon this supposition only can he explain their course and symptoms. In his notice of its history we find the names of Ruysch, Recamier, Velpeau, Bernutz, Goupil, Nélaton, Voisin, Robert, Vigues, Puech, Gulmont, Nonat, Trousseau, Becquerel, Genouville. In England, Tilt, Bennet, West, and Simpson were the earliest observers.

In regard to the intra-peritoneal and extra-peritoneal sites of the effusion, we find the following remarks:—

> "The cases where the hæmorrhage has taken place within the peritoneum have been distributed under two heads, viz., the *encysted* and the *non-encysted*. In the former the extravasation is smaller in quantity, and arises slowly, so as to give time for the circumscribing of the effusion by lymphy exudation and adhesions. This is much the more common variety. In the *non-encysted* variety the amount of extravasation is very great, and takes place rapidly, so that the danger to life is imminent, and death generally ensues as the effect of the shock, or in consequence of the profuse loss of blood. The *extra-peritoneal* or sub-peritoneal variety of hæmatocele is considered to be less dangerous than the other. M. Nonat attaches much weight to this distinction of intra and extra peritoneal hæmatoceles. 'In the latter case,' he says, 'the tumour forms behind and below the os tincæ, pushing it forward; and the swelling itself, when seen from the vagina, has a violaceous colour, which he regards as

sign of considerable importance for the diagnosis." Some of the French pathologists, who have given attention to this subject, entertain doubts as to the possibility of a uterine hæmatocele forming outside of (or underneath) the serous membrane. The major part, however, recognise the two forms; and some even affect to be able to diagnose between them during life. M. Bernutz is one of those who strongly contend that the sub-peritoneal or extra-peritoneal variety of pelvic hæmatocele is only met with during pregnancy or the puerperal state; and he stoutly maintains that there is not on record a single irrefragable example of its occurrence at any other time. His opinion upon this point is entitled to the highest respect, as he has studied the whole subject of pelvic hæmatocele with the closest attention, not only in the writings of others, but at the bedside and in the dissecting-room. The existence of this extra-peritoneal form of pelvic hæmatocele has been very seldom verified by post-mortem examination; but if, as may naturally be supposed, it is a less dangerous form than the intra-peritoneal, the opportunities of demonstrating its presence in this way must be more rare. The latest writer on pelvic hæmatocele, viz., Dr Matthews Duncan of Edinburgh, states that this has always appeared to him to be, probably, a common form of the disease."

As is well known, the tumour can be easily felt in most cases. But it has never appeared to us unlikely that a recent intra-peritoneal non-encysted effusion may present no definite characters to the examining finger. Should, however, such a patient survive the first dangers of the effusion, the educated finger would not long have any difficulty in discovering the hæmatocele.

"The possibility of feeling this tumour from the earliest stage is considered a matter of certainty by nearly every writer upon this disease. M. Sireday has published a case, however, which completely subverts this opinion. It occurred under the observation of M. Aran. The woman presented the symptoms of intra-abdominal hæmorrhage, but no trace of retro-uterine tumour could be detected by internal examination during life or after death. On laying open the abdomen, the pelvis was entirely filled with blood; and this peculiarity was observed—the coagulum was not in any way bound down or encysted by adhesions or lymphy effusion of any kind. In reference to this case, M. Bernutz very justly observes that it should put us on our guard against too hastily concluding that there is no sanguineous extravasation because there is no perceptible hypogastric or retro-uterine tumour, or because the tumour is slow in developing itself."

In continuing this subject, Dr M'Clintock makes valuable remarks on the diagnosis and treatment of the disease, which latter, he evidently thinks should be, in the main, expectant. Another and still rarer kind of hæmatocele next comes under consideration, namely, spontaneous pudendal thrombus. In the same chapter are given examples of the various kinds of perineal, labial, and vaginal hæmatocele, or thrombus. This is the only chapter to which we shall call special direction, and we conclude with an extract regarding what may be called the true uterine hæmatocele :—

"Let me now turn to that very rare variety of thrombus in which the lip, or the lower part of the cervix of the womb, is the seat of extravasation. To all such cases I would restrict the term *Uterine Hæmatocele.* The late Dr Montgomery drew attention to this subject in a short paper published in the *Dublin Quarterly Journal of Medical Science* for May 1851. Denman, whose acute observation nothing escaped, had, however, distinctly noticed the accident. He writes,—'The uncoloured mucous discharge from the vagina which pretty generally occurs before labour, on its accession is usually tinged with blood, or a small quantity of pure blood is discharged. This sanguineous discharge, which varies in quantity and appearance in different women, is popularly called a *show;* and it happens more particularly at two periods of a labour,—when the os begins to dilate, and when it is finally dilated. In the first instance it is probably occasioned by the separation of a few of those vessels by which the membrane which connects the ovum to the uterus was originally bound, and in the second by the effusion of some blood before extravasated in the substance of the os uteri; for this part, in some cases, acquires an uncommon thickness from that cause, independent of any œdematous or inflammatory tumefaction."

After describing the history of a fatal case, our author adds,—

"At the necropsy the uterus was found well contracted down in the pelvis. 'On the left side of the cervix, about one inch from the os uteri, was observed a ragged, sloughy-looking opening, the edges of which were very irregular and of a black ash-grey colour. This opening, which was large enough to admit two fingers easily, communicated with a cavity the size of a small orange; it seemed to be formed in the substance of the cervix, and its external wall was found to be the projecting tumour before mentioned, as seen from the outside. On laying open this cavity, and washing away some loose clots (but carefully observing that there were no laminated coagula), the lining membrane was found rugous, of a firm consistence, and resembling very much the mucous membrane of the vagina. Opening into this sac were seen the mouths of five or six bloodvessels, large enough to admit a small bougie.' The preparation of this uterus is preserved in the museum of the Lying-in Hospital. I had an opportunity of examining the parts in the recent state, and have little doubt that the case was one of thrombus forming in the structure of the cervix uteri, and bursting on the fourth day after parturition—the usual time when these bloody tumours give way. The displacement of the coagulum which had temporarily closed the mouths of the lacerated vessels occasioned a renewal of the hæmorrhage, and in such quantity as to destroy life."

After some other cases and remarks we find the following interesting passage:—

"The hæmorrhage which occasionally takes place in the *second stage of labour* (and to which Dr Hardy and myself have specially directed attention in our Practical Observations on Midwifery, etc.) may proceed from a ruptured thrombus of the uterine lip. Denman, as we have seen, notices the fact that a discharge of blood from this source sometimes occurs simultaneously with the passage of the head through the os uteri. I cannot venture to say how often in my experience hæmorrhage at this period of labour has been so produced; as, to determine this point beyond doubt, a minute examination of the os should have been made soon after the birth of the child. But I have recognised it as a cause in more than one instance, and I have no manner of doubt that in some other cases the hæmorrhage likewise proceeded from a ruptured thrombus of the os uteri. And this conclusion I came to from the tumid state of the lip before the head cleared it, and from the hæmorrhage continuing after delivery, in spite of a firm contraction of the uterus, with a quiet state of the circulation.

"The rupture of one of these uterine hæmatoceles may also give rise to troublesome or dangerous hæmorrhage post partum, for the restraining of which the most effectual treatment would be the direct application of cold, and plugging the vagina. If not already aware of the existence of this lesion, the persistence of the hæmorrhage, *with a firm contraction of the uterus*, should, at all events, suggest the possibility of its presence, when a careful digital examination would, in all probability, remove any doubts that might exist."

These extracts will give the well-informed reader some means of guessing what he may expect to find in the other portions of the work. The chapters are all excellent, and contain much matter for reflection for the obstetrical student.

*China from a Medical Point of View in* 1860 *and* 1861; *to which is added a Chapter on Nagasaki as a Sanitarium.* By CHARLES ALEXANDER GORDON, M.D., C.B. London: Churchill: 1863. Pp. 464: 8vo.

DURING the last few years, China has acquired a new interest in the minds of almost all classes in this country. Previously sealed against the admission of foreign influences, whether by means of religion, art, literature, or commerce, it was at once a source of mingled regret and curiosity; but now it is comparatively open to all comers. Since the late allied expedition, by which the kingdom of the celestials was shaken to its very centre, an entrance has been obtained for a more wholesome commerce than had hitherto existed, and an open door of admission has presented itself to the divine and to the physician, separately as well as in the combined office of medical missionary, without whose aid such remarkable facilities for intercourse with the Chinese can neither be sustained nor improved.

One of the first fruits of the expedition, in a medical point of view, is the book now before us,—a work which, independently of its purely medical matter, is rife with amusement and instruction. Medically, it is of importance to those who may hereafter have charge of troops in the same country, to whom the information concerning the climate, and the sanitary regulations requisite for the maintenance of health there, will be of the utmost value; and, for similar reasons, the civilian would do well to read it carefully.

The first chapter of the book is devoted to a review of the history of China, comprising notices of its more prominent features. The first place noticed in detail is the island of Hong Kong, in the estuary of the Canton or Pearl River, which was ceded to the British Crown in January 1841. The notoriety which Hong Kong obtained for its unhealthiness for a long time was scarcely second to that of the stations on the west coast of Africa. So great was the mortality amongst the troops, shortly after their settlement there, that in August 1841, out of a strength of 631, the 37th Madras Native Infantry had 386 men in sick quarters, chiefly suffering from fever, but many from exhaustion alone. A committee was constituted to inquire into the occasion of the sickness, and although no satisfactory cause could be ascertained, yet from that time things began to improve, and the character of the island was somewhat redeemed. Barracks were constructed on a better plan, the personal hygiene of the troops was more strictly regarded, and to these, as circumstances which have given rise to the improved condition of the residents, may be added the works which have been carried on for the development of the town of Victoria. Hong Kong is remarkable for the wetness rather than for the heat of its climate; the rain falls heavily through a large portion of the year, whilst in the remainder,—December, January, and February,—

there is scarcely sufficient water to be obtained in the island to supply the wants of the inhabitants. From meteorological observations, made by Lieutenant Courtenay of the Royal Engineers, during nine months of the year 1861, the following items are selected :—

| | Mean height of Barometer. | Mean Temp. | Rain in Inches. |
|---|---|---|---|
| January, . . | 30·068 | 62·0° | 2·500 |
| February, . . | 30·095 | 57·5 | 2·165 |
| March, . . | 30·020 | 62·3 | ·415 |
| April, . . . | 29·903 | 70·5 | 6·400 |
| May, . . . | 29·803 | 76·8 | 15·472 |
| June, . . . | 29·768 | 82·0 | 13·100 |
| July, . . . | 29·738 | 81·5 | 14·279 |
| August, . . | 29·665 | 84·0 | 5·160 |
| September, . . | 29·749 | 81·3 | 10·277 |

The diseases most fatal to Europeans in the south of China are fevers and bowel affections. The fevers are of the ordinary types, but are remarkable in many points. Intermittent fever, for example, is peculiar in its erratic disposition, and from the fact that it frequently has no regular period of recurrence. Moreover, it is seldom complicated with enlargement of the spleen, although it adheres most obstinately to those in whom it has once obtained a seat. Dysentery and diarrhœa add largely to the mortality of the troops. The former, probably in most cases, comes on in the hæmorrhagic or scorbutic form, the hæmorrhage from the bowels being very profuse. Hepatitis is said to be of less frequent occurrence than in India, except where it forms a complication with dysentery, when it is perhaps fully as common. Phthisis is by no means rare ; but to what extent the climate is responsible for the development and progress of this disease it is impossible to determine in the midst of so many other predisposing causes. Apoplexy and deaths from *coup-de-soleil* are not of frequent occurrence.

Respecting Canton, the author has not much information. The following, as touching the salubrity of elevated positions, may be quoted :—

> "There is a point in connexion with the healthiness, or rather the unhealthiness, of the British portion of the troops stationed at Canton, which becomes of very great importance if it be determined to retain a force there. Hitherto, the soldiers who occupy buildings and huts situated upon the 'heights,' and, as might be supposed, thereby removed from the malarious influences of the lower ground, have suffered very severely in health, and many have died from various diseases, especially from a very malignant form of remittent fever. At the same time the native Indian troops, whose barracks are situated upon the low ground, and, in the instance of one, surrounded on all sides by swamps and tanks, which are used as nurseries for frogs for the market, there has been no unusual prevalence of sickness. It may indeed be considered that this comparison between Sepoys and British soldiers proves nothing. Very probably it does not; and I will even admit, that to compare the one with the other in any way is reprehensible upon my part. There are, however, and have been, a considerable number of soldiers stationed in the centre of the city, and surely there can be no reason why their condition should not be compared with that of their comrades on the heights. While there the latter

were suffering from severe sickness, and were losing individuals of their number almost daily; those in the city were by no means unusually unhealthy. In endeavouring to assign a cause for this unexpected circumstance, I find myself altogether at a loss. It is, indeed, useless, with our present very limited experience of Canton as a military station, to speculate regarding it. Yet the circumstance deserves to be borne in mind by those who may hereafter have more extensive opportunities than have yet been afforded of extending inquiries regarding this and allied subjects."

The bulk of the volume is devoted to a description of the city and neighbourhood of Tien-Tsin, the port of Peking. This part of the work comprises a description of the character and customs of the inhabitants; the topography, zoology, climate, hygiene, mortality, and pathology of the city and its surrounding districts. The author's remarks are accompanied by many elaborate statistical tables, which, although based upon limited data, are nevertheless of great value.

Nagasaki, recommended by the author as a sanitarium for troops stationed in China, is the principal seaport and commercial city of Japan, situated on the S.W. side of the island Kin-Sin. The city is built on the slope of a hill, and has a population of from 70,000 to 100,000. The grounds upon which the author rests his recommendation of Nagasaki as a convalescent residence are not well supported by meteorological facts; not that these facts do not support him so far as they go, but they are scanty in the extreme, and insufficient of themselves to determine the value of the climate. That the reader may understand precisely what the author has to say in favour of Nagasaki, we quote Dr Gordon's words :—

"The climate, at the time of my visit, namely, towards the latter end of October, was remarkably agreeable, the temperature so moderate that, without inconvenience, we were able to walk about during the whole day, without the necessity for any protection from the rays of the sun. It does not appear that any extended series of meteorological observations have been instituted at Nagasaki; from various sources, however, I learn that the maximum summer heat does not exceed 88° Fahr. in the shade, and the cold does not descend below that sufficient for the formation of hoar-frost. The atmosphere contains a moderate amount of moisture, but, so far as can be learned, the climate does not merit the character of a particularly moist one. A copy of the *Nagasaki Shipping List and Advertiser* having fallen into my hands, I transcribe therefrom the only series of observations on the climate that it has been my fortune to meet with. These were taken in the month of July 1861, the thermometer being placed in a veranda having a *westerly* exposure, and consequently showing a higher range than it would have done had it faced the north, as is usually the case.

| Date. | 8 A. M. | Noon. | 8 P. M. | Wind and Sky. |
|---|---|---|---|---|
| July 1, | 80 | 83 | 80 | South-west, cloudy. |
| 2, | 82 | 84 | 80 | South-west, —— |
| 3, | 80 | 87 | 82 | Calm, clear. |
| 4, | 80 | 87 | 80 | South-west, clear. |
| 5, | 83 | 87 | 80 | Cloudy and rain. |
| 6, | 80 | 83 | 81 | Calm, rain. |
| 7, | 81 | 84 | 83 | South-west, cloudy. |
| 8, | 78 | 85 | 83 | —— |
| 9, | 82 | 85 | 83 | —— |

These few observations are very imperfect, and have evidently been made by a person unaccustomed to such matters. They are in one respect valuable, however, in showing that, in what is usually the hottest month throughout the year, the temperature here is really moderate. We learn that there are no such extremes of climate experienced at Nagasaki as there are on the adjoining coast of China; and we are informed that this arises from a cause of an analagous nature to that which gives to the British isles their moderate summer heats and comparatively mild winters. An offshoot of the great equatorial oceanic current having passed upwards along the channel which separates Formosa from the mainland of China, continues its course thence in a north-easterly direction to Nagasaki, and then from here onwards. Thus, it is to the island of Kin-Sin much what the Gulf-stream is to the south of England. Japan is, in fact, in geographical position and its climate, much in the same relation to the eastern great continent what the British isles are to the western; and it seems to me that this circumstance will sooner or later attract that importance which it deserves in reference to a station for ships of war and troops."

Dr Gordon's book is full of interest, and will doubtless be read by many, both professional and non-professional, readers, to whom it will afford information of an entertaining and instructive character. To those who are about proceeding to the locality which he describes, Dr Gordon's work must necessarily be of immense importance; but we may safely recommend it to our readers as an agreeably-written work upon a variety of interesting subjects relative to China,—a perusal of which they may enjoy without ulterior views. We cannot close, however, without noticing what we consider to be a very unfortunate blemish in the work. We refer to the manner in which Dr Gordon speaks of the utility of medical missions. We need not reproduce his remarks; they are based upon very silly arguments, which have been frequently refuted. We hope there are few military, especially medical officers, who, with so much talent and energy, have so little heart for so good a cause.

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## *Report of a Committee of the Associate Medical Members of the Sanitary Commission on the Subject of Amputations through the Foot, and at the Ankle-Joint.* New York: Baillière Brothers: 1862.

It is satisfactory to know that amid the disorganization which has been but too apparent in various departments of the Federal army, one at least has done its duty, and has merited and received the full confidence of the public,—that department is the medical; and it is highly honourable to the profession that our transatlantic brethren have evinced an unselfish devotedness which would be even more appreciated were it less common than it actually is. No doubt, amid the hundreds who were suddenly attached to the medical staff, there may have been some unfitted and unworthy to assume its grave responsibilities, but these have been the rare exceptions, and have been entirely lost sight of amid the general good conduct of the others.

The Report before us is published under the circumstances thus detailed in the preface:—"The attention of the sanitary commission has been directed to the fact, that most of our army surgeons, now in the field, are unavoidably deprived of many facilities they have heretofore enjoyed for the consultation of standard medical authorities. It is obviously impossible to place within their reach anything that can be termed a medical library. The only remedy seems to be the preparation and distribution, among the medical staff, of a series of brief essays or hand-books, embodying, in a condensed form, the conclusions of the highest medical authorities in regard to those medical and surgical questions which are likely to present themselves to surgeons in the field, on the largest scale, and which are, therefore, of chief practical importance. The commission has assigned the duty of preparing papers, on several subjects of this nature, to certain of its associate members, in our principal cities, belonging to the medical profession, whose names are the best evidence of their fitness for their duty."

The object of the Report is to determine whether Chopart's, Syme's, or Pirogoff's operation gives the most favourable results, and is therefore to be recommended for general adoption by military surgeons.

A minute description of the mode of performing the three operations is first given, and then follows an estimate of their relative merits as determined by, 1*st*, the comparative danger of the operation; and, 2*d*, the comparative usefulness of the stump. After a careful consideration of the various statements which have been adduced in favour of, or in opposition to the three modes of operating, the following conclusion is arrived at:—"Of the amputations through the tarsus or at the ankle-joint, preference should be given to Syme's operation as affording a minimum mortality, with a stump best adapted to an artificial limb." To this report are attached the signatures of several of the first American surgeons, including that of Valentine Mott. It must be gratifying to Professor Syme to find the merits of his operation thus authoritatively recognised; and we have no doubt that the decision arrived at by the American surgeons will be, in course of time, universally accepted. Many of the so-called improvements of Mr Syme's original operation, which have been from time to time suggested, have had for their object the overcoming of imaginary disadvantages, and the great cause of the non-success which has attended some operations is to be found in their neglect to follow out accurately Mr Syme's instructions. Performed as originally directed, we are confident that no other operation, in this situation, will give, either immediately or permanently, such satisfactory results.

# ON BERLIN AS A MEDICAL SCHOOL.

By William Anderson, M.D., Resident Physician to the General Hospital, Birmingham.

It may be of some importance to those who wish to avail themselves of the opportunities afforded by the continental schools of medicine, to know what a splendid field for the cultivation of many of the most important branches awaits them at Berlin. One has only to mention the names of Virchow, Von Graefe, Traube, Frerichs, Du Bois-Reymond, Langenbeck, Romberg, and many others, in order to give an idea of the brilliant array of talent which Prussia has centred in her capital; and when one considers that these eminent men are all devoted to teaching, and that they are liberally supplied with everything that can make that teaching effective, and that they are provided with able assistants, whose numerous classes form a good preparation for and assistance to those of the principal, it cannot be wondered at that Berlin should attract working men, who wish to see a subject handled in a thorough way, from all parts of the world.

I wish I could describe at length the brilliant cliniques of Traube and Frerichs at the Charité, of V. Graefe and Langenbeck at their respective hospitals, and all that is to be seen and admired at the other medical institutions; but in the meantime I must content myself with attempting to give an idea of the manner in which pathology is taught in the Pathological Institute by its director, Prof. Virchow.

The Institute is a handsome building, situated in the grounds surrounding the Charité Hospital, and is in all its arrangements most completely adapted to the purposes for which it is intended.

Here one may sometimes see eight or ten post-mortems per day (during my stay there was an average of four or five), conducted by the professors' able assistants, who dictate the condition of every organ, so far as it can be observed by the naked eye, to the clerks, thus at once teaching careful observation and preserving, for reference, a record of every case examined. Those organs which are thought to present interesting morbid changes are preserved fresh in cellars, so that they may be available for examination at the following courses:—

I. The Demonstrative Course, two hours, twice a-week, at which all the important organs that have been met with on the intervening days are brought up arranged on trays, so that one has the advantage of contrasting the different pathological appearances. The professor selects the most important for his lecture, which he at the same time illustrates by numerous drawings, and the room is so arranged that the microscopical preparations, made at the time by the assistants, who are always present, are sent round to all, and thus the student first sees the organs themselves and compares them with others, hears them described by the professor, and then has the microscopic appearances presented to him.

In connexion with this course a section is made once a-week by the professor, in detail and at great length, in order to teach the best methods of examining all the different regions.

II. Practical, Histological, and Pathological Course, two hours, three days a-week. In this course the student, under the guidance of the professor, selects organs of remarkable interest for examination, or investigates any special subject. Each has his separate seat with microscope, reagents, and every possible convenience for microscopical research, the professor going from one to another, and in German, French, or English, guiding, advising, and directing, removing difficulties, and exposing sources of fallacy, all with an ease and readiness which, although to be expected from one of his genius and experience, is truly wonderful to the tyro in such matters.

III. Lectures on Pathology and Therapeutics, one hour, four days a-week, illustrated by numerous preparations from the museum, which are handed round, as well as fresh specimens, and experiments on living animals.

IV. Special Free Course on Tumours, one hour, twice a-week, also richly illustrated by preparations and fresh specimens.

V. Course of Practical Histology and Pathology, two hours, three days a-week, by the first assistant to the Institute. This is of the highest value, both for its own sake and as preparatory to the work under the professor. Here again the student is furnished with every appliance and means for macroscopic and microscopic research, and, under an able teacher, led to distinguish and describe the different histological and pathological appearances, as well as to make preparations of them.

There is attached to the lecture-rooms a laboratory, where, under the special guidance of an assistant who confines his attention to this department, the most extensive chemico-pathological researches may be carried on. This requires such close attention, however, and occupies so much time, that it is chiefly entered upon by those who can devote their time and energies to such objects of inquiry.

Those who intend to give much time to the microscope (and one very soon finds that he can do little else if he avails himself of all the opportunities at his disposal), are invited by the Professor to study in a room immediately adjoining the apartments for the lectures and sections, where every personal comfort and every advantage to be gained by constant intercourse with the Professor and his assistants, and with those gentlemen who have been long accustomed to the niceties of microscopical work, is afforded. Here one meets, on terms of the most friendly intercourse, with zealous students and young graduates of all nations,—Russians, French, Danes, Swedes, Italians, Americans, and our own countrymen,—attracted by the world-wide fame of the director of its Institute; and I feel sure all carry away a grateful remembrance of the impartial kindness and attention with which he delights and instructs every one with whom he comes in contact.

The course above mentioned commences at eight A.M. Many who devote themselves entirely to the study of the various branches there taught, remain the whole day, having their zweite Frühstück or luncheon there, and dining late; in fact, being occupied by their attendance upon the sections, the different courses, and their own independent researches for the entire day, in the most agreeable and profitable manner.

I should not omit to mention that the teachers of other departments take full advantage of the number of bodies to give the most brilliant demonstrations in operative surgery, physical diagnosis, etc., the physicians not hesitating to put themselves to the severe test of giving a full diagnosis, mapping out the different organs, healthy and diseased, thrusting down strong needles to fix them, and then opening the body to ascertain how far they are correct, or, if in error, what has been the source of fallacy.

# Part Third.

## MEDICAL NEWS.

## MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION XLII.—MEETING VII.—PATHOLOGICAL.

8*th April* 1863.—JAMES SPENCE, Esq., *President*, in the Chair.

### I. EXCISION OF THE ELBOW-JOINT.

*Mr Spence* showed a case where he had performed excision of the elbow-joint by a longitudinal incision. The operation by the usual H incision gave excellent results, but it had the disadvantage that the wound did not unite very readily,—the longitudinal method, though a little more difficult of performance, was superior in regard to the simplicity of the after-treatment, and to the facility with which the wound healed; passive motion also could be begun sooner than in the case of the other method. Mr Spence had operated several times by the longitudinal method, and in all the cases the results had been satisfactory; it was particularly advantageous in young subjects, where the dressing of the wound after the transverse operation was found to be very troublesome. He had lately met with a case where sinuses almost indicated the H incision, yet where he performed the longitudinal operation, leaving the sinuses untouched. In the case of the lad now brought before the Society, the longitudinal operation had been performed eight weeks ago; the wound was almost healed, and there was not only passive power of pronation and supination, flexion and extension, but the patient had already considerable power in executing movements for himself. In a former case, which occurred before the beginning of the present session, the results had been still more satisfactory; the wound healed almost entirely by the first intention, and at the end of three weeks the patient could move the limb himself. Of course there were cases where this method of operating was contra-indicated.

*Mr Edwards* asked, if in the performance of the longitudinal operation much lateral dragging was required; and would wish to be informed if Mr Spence had met with cases where stiffness of the limb had resulted in consequence of the bones growing together, as had been complained of by the London surgeons.

*Mr Spence* replied that if a good free incision was made, little lateral dragging was required. The difficulties of the operation were indeed very trifling, in comparison to the advantages gained; the appearance of the limb was decidedly better than after the transverse method, no apparatus was required, only a sling and occasionally a figure-of-eight bandage. With regard to stiffness of the limb in consequence of growing together of the bones, there was certainly a risk of this in cases of anchylosis or necrosis, unless large portions of bone were removed; Mr Spence, however, had only met with it twice or thrice in his own earlier practice, but recently in one patient, a girl suffering from anchylosis, although a large portion of bone was removed, considerable stiffness did arise. In this respect the method by longitudinal incision presented an important advantage, as passive motion might be begun eight or ten days after the operation.

### II. ACCIDENTAL REMOVAL OF THE EAR.

*Mr Spence* showed the ear of a patient which had been removed in a singular manner. A railway porter, while engaged in cleaning trucks, was knocked down by the buffer of an engine; the engine and tender passed over him, but he was dragged for a short way along the ground, and his ear was completely removed by some sharp body upon the ground. The head had not been injured, there being only a slight bruise of the scalp. The man recovered perfectly well, but hearing did seem to have been somewhat affected.

### III. EXCISION OF THE HEAD OF THE HUMERUS.

*Mr Spence* showed four specimens of the head of the humerus, removed on account of caries and cario-necrosis, where the operations had been quite successful. The first case had occurred several years ago, and at the time seemed very unpromising, as there was not only caries of the head of the bone, but necrosis of the shaft, and alteration of the muscle; the result had been exceedingly favourable, and there was now considerable power of movement in the joint. In the second case, a girl, a large extent of bone had been removed, but she too had a good deal of power in the limb. The third case which had been operated on about two months ago, had made an excellent and rapid recovery, and the man could already use the arm with some freedom. The fourth case had been operated on a few days ago, but everything was going on well. Mr Spence remarked that there was certainly a greater tendency to phthisis in patients with disease of the head of the humerus, than in those with disease of the elbow-joint. Still, excision of the head of the bone was much preferable to amputation at the shoulder-joint, as the risk from shock was so much less.

### IV. FRACTURE RESULTING FROM ACUTE NECROSIS.

*Mr Spence* showed a specimen removed by amputation at the lower third of the thigh, where the operation had been rendered necessary by the occurrence of a fracture, in a case of acute necrosis of the tibia. The patient, at the time of the operation, was in a state of great exhaustion; and pyæmia had supervened. The preparation was interesting pathologically, as showing bone in the different stages of separation and new formation.

### V. CANCER OF THE MAMMA.

*Mr Spence* showed a specimen which illustrated an important surgical principle. The case was one where there was a small tumour of the mamma, accompanied by slight retraction of the nipple. Although the tumour was movable, and was apparently quite circumscribed, still, as there was retraction of the nipple, and as the patient was past middle age, Mr Spence, guided by what he had seen in similar cases, had removed the whole mamma. An examination of the tumour showed the propriety of the treatment adopted. Although apparently circumscribed, the tumour, when cut into, was found distinctly cancerous; and the species of cyst by which it was surrounded, also consisted largely of cancer-cells.

### VI. INJURY OF LONGITUDINAL SINUS.

*Mr Spence* showed a specimen of an injury of the head, occasioned in a curious manner, and which had given rise to a not very common lesion,—injury of the longitudinal sinus. A cabman, during a high gale of wind in January last, while waiting upon a party engaged in shopping, was struck upon the head by a portion of brick from the coping of the building, the result being that a portion of the cranium was driven upon the brain. When brought to the hospital, a large depressed compound comminuted fracture was found. Fragments of bone were removed, others were elevated. During, and previous to the operation there had been a constant oozing of dark blood, but, on removing a sharp fragment of bone, there was at once a jet of blood evidently proceeding from the longitudinal sinus. The bleeding was, however, readily repressed by the application of the finger, and no other dressing than a piece of lint was required. For some days the patient went on well, but rigors occurred, followed by paralysis, convulsions, and coma, and the patient died. On examination, the bone was found healthy, and the edges were assuming a rounded-off appearance; the wound of the dura mater was closed by a fibrinous clot; examined from the interior, there seemed to be little or no blocking-up of the rest of the sinus; the surface of the brain was covered with pus, and there was a small abscess of the cerebellum. Mr Spence remarked that all the cases of this kind he had seen had died, not from loss of blood, but from inflammation of the membranes of the brain or cerebral abscess.

### VII. CASE OF AXILLARY ANEURISM.

*Mr Spence* showed a specimen of axillary aneurism. The patient, a man about sixty years of age, had been sent to the hospital, by Dr Struthers of Leith, on account of an aneurism occupying the right axilla. The tumour was tolerably large, pulsated freely, was apparently pretty well defined below, and its contents were evidently quite fluid. This aneurism, however, was not the only one; there was another in the abdomen; and other contra-indications to an operation existed. In the course of a few days the patient suddenly died, in consequence of the bursting of an aneurism of the abdominal aorta into the lung. The aneurism of the axillary artery now shown, was one of those in which there was an alteration of the middle and internal coats, but no ulceration of any coat. The case was one in which, had no other aneurism been present, a question might have arisen as to the propriety of reverting to the old operation of laying open the sac and tying the artery above and below, or of tying the third part of the subclavian. No doubt, there were certain cases in which the old method presented advantages; but, on the other hand, the present case showed that the principle could not be carried out so widely as proposed by Mr Syme, who maintained that, as aneurism was not a local disease, we were as likely to find the artery healthy near the aneurism as at a distance from it. In the case now under consideration, the only point at which a ligature could have been applied was the subclavian; the whole axillary portion was diseased and dilated, and from the termination of the subclavian no portion of the coats was healthy. Had the old operation been performed no definition of the vessel would have been found, and the coats of the arteries would have been found so diseased that no ligature could have been applied. Mr Spence therefore believed that in the case of the axillary artery the Hunterian method was still the best, except in cases where the aneurism became diffused, and then the other plan had undoubtedly considerable advantages.

### VIII. AORTIC ANEURISM PRESSING ON THE TRACHEA.

*Dr Haldane* showed a specimen of an aneurism of the aorta, which had given rise to paroxysms of difficult breathing, on account of which tracheotomy had been performed. The case was one in which considerable difficulty seemed to have attended the diagnosis. The patient, a man about fifty years of age, suffered from constant difficulty of breathing, subject to paroxysmal exacerbation, and there was considerable alteration of the voice. During a paroxysm of peculiar severity, tracheotomy was performed immediately below the cricoid cartilage; the operation, however, gave little relief, and soon afterwards the patient died. On examination, the ascending aorta, from an inch above the semilunar valves, and the whole of the transverse portion of the arch was dilated into a large aneurismal sac. The posterior surface of the upper part of the sac was closely applied to the trachea, and in a space of about an inch and a half the calibre of that tube was found to be considerably diminished. The pneumogastric and recurrent nerves were not involved. Dr Haldane observed that the appearances found on post-mortem examination abundantly explained the non-success of the operation. The difficulty of breathing had been occasioned by the direct pressure of the aneurism upon the trachea, and as the tube could not be passed below the seat of obstruction, the operation could give no relief. Tracheotomy might be useful in cases where the pneumogastric or recurrent nerves were irritated by the pressure of the tumour, and where the difficulty of breathing was paroxysmal; in cases, however, where the difficulty of breathing was constant (even although liable to paroxysmal exacerbations), the probability was that it was occasioned by direct pressure upon the air-passages, and in such cases Dr Haldane would invariably recommend abstinence from operative interference.

*Mr Spence* observed that tracheotomy should be strictly limited to cases in which there was no direct pressure, for where this existed the operation would not only fail to give relief, but death might actually be caused by the introduction of the tube. In cases where dyspnœa was due to pressure on the nerves, it was different, and there the operation might at least act as a palliative.

### IX. RUPTURE OF THE ASCENDING AORTA.

*Dr Haldane* showed a preparation for which he was indebted to Dr Littlejohn. A man was found dead in a lodging-house, under somewhat suspicious circumstances. By direction of the authorities, Drs Littlejohn and Inglis made an examination of the body. No marks of injury were found; but, on opening the body, the pericardium was found filled with about twelve ounces of fluid and loosely-coagulated blood. The hæmorrhage had proceeded from a minute opening in the aorta. When the aorta was laid open, its inner surface presented a very irregular appearance, due partly to semi-transparent thickening, partly to destruction of the inner coat resulting from atheromatous degeneration. The opening in the vessel, from which the hæmorrhage had proceeded, was situated about an inch above the posterior semilunar valve; it barely admitted a common probe, and was situated in the base of one of the so-called ulcerations of the artery. The internal and middle coats were destroyed, and the external coat had become very slightly dilated, and had thus given way. The heart was a little enlarged, and its muscular substance was very fatty.

### X. DISEASE OF SUPRARENAL CAPSULE.

*Dr Haldane* showed the left kidney with its suprarenal capsule, removed from the body of the same patient from whom the last preparation (IX.) had been obtained. The capsule was much enlarged, and had a coarsely lobulated appearance. On section, it was found to have been entirely converted into a material of a putty-like appearance and consistence, contained within a firm cyst, and divided into three masses or lobules. Not a trace of the normal structure of the gland remained. On microscopic examination, it consisted of granular and amorphous matter, mixed with a comparatively small number of crystals of cholesterine. The right suprarenal capsule was natural. There was not the slightest discoloration of the skin. Dr Haldane observed that the case was a good example of that form of lesion, with which alone Dr Wilks of Guy's Hospital believed that the so-called Morbus Addisonii was associated. It was true that, as only one capsule was diseased, the case could scarcely be adduced in opposition to the opinion that a peculiar train of symptoms was connected with a certain morbid condition of the capsules; but, regarding the correctness of this opinion, Dr Haldane was still sceptical, and in the course of a very extensive experience he had met with nothing to confirm it.

### XI. VILLOUS CANCER OF THE URINARY BLADDER.

*Mr William Brown* narrated the following particulars of a case which had lately come under his observation:—

The subject of this case was fifty-nine years of age, and died on 24th March. For above thirty years he was a laborious literary and professional man. Public speaking was always attended by much perspiration; and when this was abruptly checked, he readily suffered from catarrhal or rheumatic symptoms. For many years his chief ailments had reference to the respiratory organs: there was cough, harsh voice, copious expectoration of mucus; sometimes there was pleurodynia; on two or three occasions slight hæmoptysis.

In October 1859, he first consulted Mr Brown about his urinary organs. There was a frequent call, and pain after the urine had passed. The urine at that time was normal in its character. He had for many years been unable to retain his urine during the entire night, but had not thought this symptom worth noticing. During 1860, he had still these symptoms, but they had not become more urgent. In April 1861, they were more troublesome, but no change in their character occurred till January 1862, when, after some extra fatigue, the urine began to be bloody. This continued for some days, and at length seemed to be checked by nitrate of iron. Occasionally after this the bleeding took place, but to a small extent each time. The frequency in the calls did not diminish: during the night they were, on many nights, every hour. His general health became affected, and his appetite capricious. He was often urged to use the proper means for ascertaining the exact nature of the disease, but determinedly refused, from his nervous dread of pain. In the beginning of this year, perspiration, which used to be so abundant during and after exertion, began to diminish, and at the same time he began to suffer from thirst. He at length acquiesced in Mr Brown's suggestion, and Mr Syme saw him on the 11th March.

The bladder was explored by Mr Syme with care, and scarcely any pain was caused by it. No stone was discovered, but an unhealthy state of the bladder was more than suspected. A further examination was proposed on a subsequent day.

An hour after the exploration, blood began to issue from the urethra, the urine could not be discharged, and severe pain in the region of the bladder and along the canal, was experienced. By the use of the catheter the pain was relieved, but the hæmorrhage still continued. This continued for several days,—violent pain as the urine began to accumulate in the bladder, always relieved by the use of the catheter. After a while (under the use of remedies) the urine became less bloody, but its fœtor became greater, and it contained more mucus. The urine dribbled, or rather was discharged by a jerk, in teaspoonfuls. The jerk was always painful, but the pain was not so severe as formerly. The repetition of this pain every ten or fifteen minutes, even although not very intense, was very exhausting. Attempts were made to keep up the strength by stimulant and nutritive fluids every hour. The intellectual faculties remained unimpaired. The body was examined twenty-two hours after death by Dr Joseph Bell.

*Dr Joseph Bell* gave the following description of the results of the post mortem examination:—When the bladder was cut open from above, after its complete removal, its whole cavity seemed to be filled with a loose, whitish, soft substance, streaked with tinges of pink and yellow. On letting a very gentle stream of water flow into the bladder a quantity of this substance was washed out; but a far greater quantity still remained adherent both to the base and the sides, nearly up to the fundus. The bladder was very strongly contracted, and its muscular coat much thickened. It contained no urine.

Looked at by the naked eye, this structure which lined the walls seemed to consist of innumerable slender filaments about one-sixth to one-fourth of an inch in length, matted together and moving in masses (like the pile of a very loose velvet or flock) when water was poured on them. The pale colour was apparently powdered over by a whitish frosting.[1]

Under the microscope the soft structure was found to consist of immense numbers of delicate filiform villi, very long in proportion to their breadth; some terminated in club-shaped extremities, others, and these the larger ones, being prolonged into flask-shaped projections all along their sides as well as at the edge; others, as shown in fig. 1, being markedly dendritic, with numerous branches from a single stem. The whole presented a densely granular appearance, as if from the presence of numberless very closely packed small cells, firmly attached to each other. *The frosted white appearance* scattered over the villi was at once explained by the microscope showing abundant and well-formed crystals (triangular prisms) of the triple phosphate, which lay entangled in the meshes of the villi. These villi appeared to grow not from any special tumour or pedunculated growth attached to the bladder wall, but from the very mucous membrane itself, or rather perhaps from a cellular stroma in which no separation could be traced, but which was attached to the mucous membrane.

These villi were deserving of special notice as to three points,—their relation to the mucous membrane, their vascular supply, and their contents. On making a section vertically through villi, mucous membrane, and muscular coat, the villi were seen to rise decidedly from the submucous areolar tissue; on adding acetic acid, the fibres of it were traceable into the roots of the villi, and both roots and fibres were infiltrated with numerous and closely packed nucleated cells. It to a certain extent removed the granular haze, and enabled me to see the vessels running into the villi. They were best seen in the more delicate villi, which contained less opaque contents.

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[1] The whole mass of soft tissue resembled very closely, both in colour and appearance, some of those marine animals or vegetables, the Flustræ, especially some of the more delicate and filiform varieties.

Fig. 1.

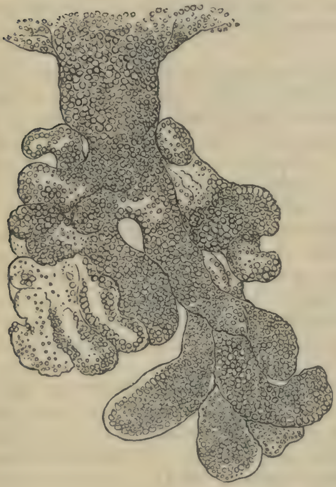
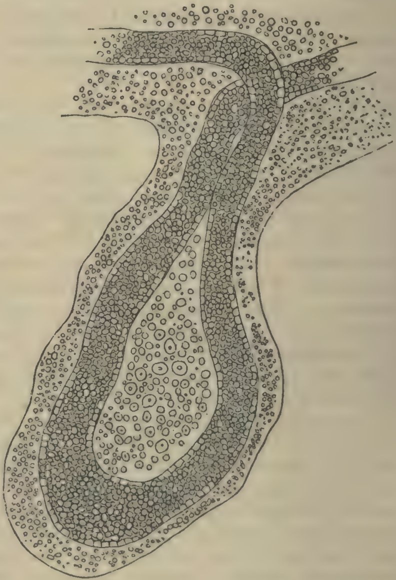


Fig. 2.



On examining a villus before the addition of any reagent, with the exception of a few of the younger and more delicate flask-shaped projections, they all seemed closely packed with a densely granular-looking structure (fig. 1), which had always a slight, sometimes a very distinct, yellow colour. On adding ether the yellow colour and granular appearance gradually disappeared, and, when added in sufficient quantity, the appearance shown in fig. 2 was seen. The loop of vessel (a colossal capillary), which was before only dimly visible, is now quite clear, and seen to be coated with a dense, closely packed layer of nucleated cells, while the rest of the contents of the villus are cleared up, the fat being dissolved and the yellow colour removed. The elongated nuclei in the capillary walls are distinctly visible.

*Remarks.*—The best description of villous cancer would be found in an essay by Rokitansky, an abstract of which was given in the second volume of Paget's Lectures on Surgical Pathology. The literature of the subject, since the publication of that essay, was not extensive; but several cases like the present were on record.

Mr Sibley, in the seventh vol. of Path. Trans. of London, described two cases—one of the bladder, the other of the colon—both proving fatal, chiefly from the drain on the system from hæmorrhage. In general character his description of the villi was not different from the one now given, though in his bladder case the disease was in the form of a tumour as large as a goose egg, with two smaller tumours close by. Mr S. alluded to the cellular character of the contents, but (apparently from not having used reagents) seemed not to have recognised the loops of vessels, or the remarkable arrangement of the cells above referred to. He also regarded the disease as *non-malignant*, though the presence of the numerous nucleated cells where no cells ought to be, and the manner in which they infiltrate the tissue, seemed pretty conclusive proof of a certain amount, at least, of malignant character. The remarkable similarity in appearance of these cells to those of the villi of the chorion in a fœtus about the tenth week was worthy of remark.

A case of villous cancer, recorded by Dr Bristowe in vol. xi. *op. cit.*, p. 95, tended to confirm the view of its malignant character. The villi, which were arranged, some in masses, some side by side, in the mucous membrane, were in shape like the others; but, on microscopic examination, the villi were found to contain loops of vessels, and seemed indeed to consist of elongated bundles of nucleated cells ranged along the vessels. The structure of the stomach, the pancreas, and liver were all infiltrated with cancer masses.

In another case in the same eleventh volume, recorded by Dr Hicks and Mr Bryant, the villi were passed in the urine during life, and recognised. In these villi the capillaries were specially marked, several branching tufts being found in each villus.

Another case, also very similar, is recorded by Mr Henry Thompson in vol. viii. p. 262, of the Path. Soc. Transactions. In it the deposit of phosphates was well marked and the lining membrane of the bladder thickened. The symptoms had been hæmaturia, micturation every fifteen minutes, and latterly purulent urine tinged with blood. The tumour, which was distinct and pedunculated, was about the size of a large strawberry.

Mr Moore, in Holmes's System of Surgery, agreed with Mr Sibley, so far, at least, that *some* of the so-called villous growths were not cancers; but it must be said that the case recorded, along with the one given by Dr Bristowe, already noticed, proves that some of the cases were (as Rokitansky said at first) *cancers*, insomuch that they infiltrated the tissues with new cells which developed endogenously. And that the constitution was so long of being seriously affected was no more wonderful than it was in the well-known epithelial forms of cancer. Thus many tumours seated on a free surface would give rise to excrescences, which, according to the nature of the surface, appeared in the form of villi, papillæ, or warts. These papillæ might exist, and even be in an immensely exaggerated form, without any cancer being present in them; but they might become cancerous, *i.e.*, these cells which constituted them might become subject to a new form of growth,—endogenous development by nuclei and nucleoli, either, *primarily*, from change in their own connective-tissue corpuscles, or, secondarily (and this is much more likely), by spreading of the disease from the connective tissue of the skin or mucous membrane.[1]

Virchow had made on this point a most important practical observation, which explained a difficulty often felt in the history of such cases of cancer of the bladder, and very specially so in cases such as the one under review at present. We might have for a very long time symptoms implying the existence of a probable cancer of the bladder,—irritation, frequent micturition, pain, and catarrh,—without any hæmaturia or sudden exacerbation, and yet at a certain stage we suddenly had excessive and continuous hæmaturia, and a rapidly fatal termination.

The explanation of this, according to Virchow, was that, during the first stage, the disease, though present, was merely an epithelial cancer, not yet having assumed its papillary form. Once, however, that the disease had spread, and a formation of villi taken place on the surface, the hæmaturia began. Why? 1. Each villus was not now clothed with a firm layer of epidermis, but lay almost bare under a loose epithelial covering. 2. They were well supplied with blood by the loops or single branches leading up to the surface; they would now bleed if mechanically injured. 3. Any spasmodic contraction of the bladder would both fill them with blood and also, by the mechanical friction of the surfaces, cause the hæmaturia. And for the production of hæmaturia, Virchow has told us that it was not necessary the papillæ should be cancerous;

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[1] See Virchow's Cellular Pathology, English Translation, p. 467.

that he had seen the papillæ quite benign, and in such a state that, had they been on the surface of the skin, they could easily have been removed by the knife and forceps, or by ligature.

It is not to be wondered at that, in the case narrated, the hæmorrhage was so persistent and excessive, when we remember the very large amount of vesical surface affected, and the consequent amount of mechanical friction the villi would sustain from the very powerful, thick-walled, and firmly-contracting bladder.

### XII. FALSE DIVERTICULA OF THE INTESTINES.

*Dr John Struthers* showed two specimens of false diverticula, or herniæ of the coats, of the intestines. In the first case, that of an old female subject, the diverticula occurred along nearly the whole length of the jejunum and ileum, and along the sigmoid flexure of the colon. One, about the size of half a walnut, occurred in the duodenum, close to the entrance of the bile duct. Along the jejunum and ileum they occurred as small projections along the mesenteric attachment, the absence or looseness of the serous coat having apparently facilitated their occurring in this situation. They varied in size from that of a large pin's head to that of a small split-pea, smaller on the ileum than on the jejunum, and were arranged mostly in two rows, a quarter to half an inch apart,—the diverticula of each row succeeding each other at intervals generally of from half an inch to an inch. In the sigmoid flexure they were much larger and very numerous.

In the second case, that of an adult female subject, the sigmoid flexure of the colon only had been preserved. Along eight inches of the flexure the projections were very numerous, arranged in clusters chiefly along the attached margin, where the peritoneal covering was therefore absent, but a number occurred also on the side farthest from the peritoneal attachment. They varied from the size of a pea to the size of the end of the little finger, and varied from a quarter to half an inch in length. They were in all forty-six in number,—twenty-seven occurred along or near the attached margin, nineteen on the opposite side. Some of them arose by a constricted, some by a gradual neck, and most of them enlarged into a bulging fundus.

Structurally, all of these diverticula were formed by bulging of the mucous membrane through the muscular coat, as was plainly enough seen in the now dried and transparent preparations. Such diverticula must not be confounded with the *diverticulum verum ilei*, the unobliterated remains of the vitelline duct, not unfrequently found, always towards the lower end of the ileum, always solitary, and occupying usually nearly the whole width, and always the entire coats, of the intestine. Dr S. had described and figured a considerable number of cases of the true diverticulum in the *Edinburgh Medical and Surgical Journal* in 1854, and in his "Anatomical and Physiological Observations."

### XIII. CONSOLIDATION OF THE ATLAS AND OCCIPITAL BONE.

*Dr John Struthers* showed a preparation in which the atlas and occipital bone were seen to be completely consolidated, without any distinct trace of disease. At the attachment of the check ligaments there was a worm-eaten appearance, but no true disease, and at several places minute foramina were seen, indicating increased vascularity, but there was no irregularity or bony excrescence as in rheumatic and other conditions of disease. The articular surfaces of the two bones were completely consolidated, without any trace of the line of union. The inferior articular surfaces of the atlas were quite normal. The posterior and anterior occipito-atloidean ligaments were converted into bone, so that the two bones were continuous all round, except where the vertebral artery and suboccipital nerve had perforated, the foramen for them being situated behind the transverse process of the atlas, and just behind the condyle, and having about the usual size of a vertebral artery. The foramen in the transverse process of the atlas is unusually large on the left side, and is open on the right side. Leading forwards from the foramen in the transverse process of the atlas to the anterior outlet of the anterior condyloid foramen, there was on both sides a distinct broad groove, the meaning of which was not evident. The anterior condyloid foramen was not larger than usual. The tubercle and form of the anterior arch of the atlas were seen, with the odontoid facet behind, but the bony continuity between them and the basilar process was not much thinner than the arch itself. It was pierced close to the left of the middle line by a vascular foramen which would admit a thick pin. The union of the posterior arch to the occiput is by a thinner plate of bone, and the posterior arch itself was thinner than usual, and without any rudimentary spine. To the right of the middle line there was an irregular aperture about a quarter of an inch in breadth, and one-twelfth of an inch in height, across which ossification has not extended. There was no history to this remarkable specimen. It occurred in a prepared set of the bones of the head, the rest of the skeleton being wanting. It is not from an old subject, as the bones have all separated well at the sutures, but it is adult, as the basilar process had been separated with the saw from the sphenoid. We had instances of serial vertebral consolidation in the sacrum, and in the union of the occipital to the sphenoid, and of the two sphenoids to each other; in most cetaceans, in the union of most of the cervical vertebræ to each other; in birds, in the union of the lumbar as well as sacral, and more or less of the dorsal, vertebræ; and in chelonians, in the fusion of the thoracic segments. These are normal conditions, and the adaptation of them is evident; but here we have the consolidation of two bones, which normally move on each other by a diarthrodial joint,—a condition which must have considerably restricted the nodding movements of the head. The most probable explanation of the specimen is the supposition of former disease, leading to anchylosis, which had remained, while all trace of disease had afterwards been removed. But what is remarkable about the specimen, besides its general healthiness, is that there is not merely articular anchylosis, but filling up of the anterior and posterior spaces, making the consolidation complete.

### XIV. VARIETY OF THE MUSCLES OF THE AXIS, ATLAS, AND OCCIPITAL BONE.

*Dr John Struthers* showed a dissection, in which the muscles which normally arise from the spine of the axis were shifted down to the spine of the third vertebra. The spine of the axis wanted its usual bifurcation and great size. The rectus capitis posticus major muscle arose entirely at the spine of the third vertebra. The recti minores were normal between the atlas and occiput. The interspinales muscles were absent between the atlas and axis, but present between the axis and third vertebra. The obliquus inferior muscle arose in its great bulk at the spine of the third vertebra, and this at first appeared to be the entire muscle; but a portion, about one-fourth the size of the lower, was then found coming normally from the spine of the axis, lying deeply on the lamina, and joining the large portion above its middle. The obliquus superior was normal. At first the attachment of the rectus major and obliquus inferior at the third vertebra seemed to be directly to a massive spinous process; but, on dissection, it became evident that, while the spine of the third vertebra was larger and more largely bifid than usual, the muscles were fixed to it indirectly, that they were attached to a thin flat oval semamoid-like bone, resembling a rather small-sized almond. This epispinous bone was attached to the corresponding tubercle of the third spine by a strong ligament, and by ligamentous fibres to its fellow of the other side. The interspinales muscles in the space between the third vertebra and axis were large, and seemed also to be attached below entirely to the epispinous bones; but there was a deeper and smaller pair of interspinales, which were attached directly to the spine of the third vertebra. The muscles which normally pass from below to the spine of the axis, were attached to the third spine through the epispinous bones instead of reaching up to the axis.

This was undoubtedly a very singular variety. Looking to the absence, as usual, of interspinales in the space between the first and second vertebræ, and their presence in the space between the second and third, while the recti majores were at the same time attached below the latter space; and looking also to the fact that the obliquus inferior retains a small head of attachment to the spine of the axis, we must draw the conclusion that the epispinous bones represent the two tubercles which are usually developed from the spinous process of the axis, for the attachment of the rectus major and obliquus inferior muscles above, and, below, for the semispinalis and other muscles. But it remains a question how these tubercles came in this body to be separate, and to be shifted down to the spine of the third vertebra, carrying their muscles with them.

### XV. SPECIMENS OF GALL-STONES.

*Dr John Struthers* showed specimens of gall-stones, remarkable for their number, size, and form. They were taken from the gall-bladder of an old female subject. They were 39 in number, were all about the same size, were all three or four sided pyramids, 22 of them having four, the remaining 17 three sides, besides the base. The sides and bases were nearly all flat. Together, in their now dried state, they weigh 1 oz. 6½ drachms avoirdupois. Each stone is about half-an-inch in length and the same in breadth. The cystic and other bile ducts were of the natural size.

### XVI. GUNSHOT INJURY.

*Mr Edwards* showed a musket bullet which had been removed from a patient who had been wounded at the celebrated Bull Run. The bullet was quite flattened, in consequence of having struck against the femur, but had not produced fracture of the bone.

*Dr Watson* believed from the appearance of the bullet that it had struck against a stone and thus been flattened before lodging in the limb.

### XVII. ACUPRESSURE AFTER EXCISION OF THE MAMMA.

*Mr Edwards* showed a scirrhous mamma which he had removed to-day, and in which the bleeding had been stopped by ordinary sewing-needles with the greatest possible ease. For stopping bleeding from vessels of that size, Mr Edwards did not believe that there was any means so handy and efficient as sewing-needles with a piece of wire attached.

### XVIII. SYPHILITIC DISEASE OF LOWER EXTREMITY SIMULATING ELEPHANTIASIS.

*Dr Watson* showed the cast of a foot and leg which he had removed by amputation within the last few days. The foot was enormously enlarged, and presented the appearances of elephantiasis attended with extensive ulceration. Each toe, the great one alone excepted, was the size of the fist, and with the dorsum of the foot presented a tuberculated appearance and an ulcerated surface. The sole of the foot in the metatarsal region was occupied by a large mushroom-like protuberance, about the size of half a shaddock, with a warty outline and an ulcerated surface. Three similar but smaller protuberances occupied the sole of the foot in the tarsal region. A probe could be introduced at several points of the ulcerated surface and penetrated to the bone, which was opened out and spicular in character. The affection of the bone and periosteum was, however, limited, and not general like the enlargement of the soft parts. The lad from whom the specimen had been obtained was twenty-two years of age, and had suffered from the disease since infancy. His family history was one which led to the conclusion that the affection was due to congenital syphilis, and in that respect, in the implication of the osseous textures producing the affection of the soft parts, the disease differed from ordinary elephantiasis, and should therefore be classified under the head of "*Podelkoma*," as described by Professor Miller in his Principles of Surgery, p. 264. The measurements of the foot were as follows:—Circumference round the toes, 19 inches; round the heel and ankle, 14 inches. Length of sole of foot, 10½ inches; greatest width, 7½ inches; circumference above ankle, 7½ inches. Rapid cicatrization of the stump and improvement in the patient's general condition followed the amputation.

### XIX. SEVERE INJURY OF THE FOOT.

*Dr Watson* showed the skeleton of a foot, in which every single bone was comminuted, with the exception of the phalanges, and held together only by the fibrous shreds of periosteum and ligament which invested them. The patient, from whom this specimen was obtained, was an adult male, a railway porter, who, some weeks ago, had his foot and the lower part of the leg crushed by a train passing over the limb. While such an amount of injury was done to the hard textures, the skin remained intact, only one small aperture, which would hardly have admitted a probe, existing upon the dorsal aspect of the foot. Dr Watson amputated below the knee, and the case had gone on satisfactorily.

### XX. GANGRENE OF THE FORE-ARM.

*Dr Watson* exhibited a fore-arm which he had amputated a few days before on account of gangrene. The patient, a collier, had sustained a compound comminuted fracture of the fore-arm, three inches above the wrist, from the falling in of the roof of a seam in which he was working. The condition of the hand was such at the time of the accident as to justify the surgeon in attempting to save the limb. Within three days afterwards gangrene set in, and, when seen by Dr Watson, sphacelation had extended up as high as the seat of the fracture. Above this point the limb was the seat of inflammatory engorgement; while the patient was suffering from feverish excitement, a very rapid pulse, and considerable nervous irritability. Amputation was performed and everything was progressing favourably.

### XXI. COMPOUND COMMINUTED FRACTURE OF THE PATELLA.

*Dr Watson* showed a specimen of comminuted fracture of the patella, produced by direct violence. The patient, from whom this was obtained, being given to habits of intemperance, had been locked up by her husband in her bedroom, which was two flats from the ground. In making her escape from confinement, by dropping from the window, she fell, and the knee came in contact either with the edge or corner of a stone step which was immediately beneath. A compound comminuted fracture resulted, communicating with the knee-joint. An attempt was made to save the limb; gangrene, however, of the skin and superficial textures set in, and amputation was performed. The line of fracture was stellate, breaking the bone into three fragments.

### XXII. DISEASE OF BLADDER AND PROSTATE.

*Dr Watson* showed the bladder and prostate of a patient, who died with symptoms of inflammation of the bladder. The prostate was enormously enlarged and fully the size of the closed fist; the middle lobe projecting, like a walnut, from the centre of the general mass; the canal of the urethra lay quite to the anterior part of the swelling, while a false passage in the line of the ordinary urethral route, was of the usual size of the natural canal, and was lined by what presented all the aspect of a mucous membrane. The bladder contained two large phosphatic calculi and a great number of small ones. The direction of the ureters through the hypertrophied muscular coats of the bladder was very oblique. The right kidney was dilated into an irregular cyst, which, with the ureter, was distended with a pultaceous urinous fluid. The left kidney was similarly affected though to a less extent. The patient from whom this specimen was obtained, was about seventy years of age, and for thirty-one years of his life had been the subject of urinary symptoms. Dr Watson only saw him within a few days before his death; he had then retention requiring the use of the catheter, which, previous to this, he has been in the habit of passing for himself. The presence of the calculi was recognised during life. The moribund condition of the patient only admitted of alleviation.

### XXIII. CYST OF BLADDER, ENLARGED PROSTATE, URÆMIA.

*Dr Watson* showed a specimen of a bladder with enlarged prostate, implicating principally the middle lobe, which was perforated by a false passage, while the natural channel was displaced forwards, so that the catheter must always have passed along the abnormal route. In this case there was a large cyst formed in the coats of the bladder immediately above the entrance of the left ureter, and communicating with the vesical cavity by an opening about a quarter of an inch in diameter. The ureter above this was distended, especially where it is inserted into the hilum of the kidney. The right kidney was completely atrophied and the ureter shrunk.

When Dr Watson first saw this patient, some weeks before his death, he complained of constant dribbling of urine, for which he had for nearly two years worn an elastic urinal. He had been under the care of a practitioner elsewhere, who had examined the bladder with a sound, and prescribed iron tonics and cold-bathing. Dr W. found the bladder largely distended above the pubes, the urine ammoniacal, and what escaped to be merely the surface drainage of the urinary secretion. On passing the catheter, a large quantity of offensive urine came off, and the suprapubic swelling disappeared. The catheter was passed twice or thrice daily for some weeks, when the symptoms of general debility so far yielded that the patient was able to be out of doors. About a week before his death the urinary irritability recurred, the quantity of urine withdrawn by the catheter was less than before, and a swelling began to be observed in the left hypogastric region, which was only partially influenced by passing the catheter. These symptoms continued till twenty-four hours before his death, when fatal coma became suddenly developed, obviously due to uræmia, and attended by complete absence of all urinary secretion in the cavity of the bladder, while the tumour in the hypogastric region became more obvious.

### XXIV. STRICTURE OF THE URETHRA.

*Dr Watson* showed a specimen of stricture of the urethra, with fistula in perinæo, hypertrophied bladder, and dilated prostatic and membranous portions of the urethra. The whole of the urethra anterior to the bulb was contracted, but the portion of the canal immediately behind the scrotum was most diminished in calibre. The subject of this condition had been an officer in the army, and suffered from stricture for fully ten years, for which, however, he had not been subjected to any treatment. Dr Watson was requested to see him on account of retention of urine, due to a large abscess in the perinæum. This he opened, and then relieved the retention by passing a No. 2 catheter into the bladder, and drawing off its contents. The urine escaped freely after this, but symptoms of pyæmia made their appearance, and the case terminated fatally. The kidneys were found enormously enlarged, in an almost diffluent state, presenting on section the aspect of delicate fibrous tissue infiltrated with oily particles and urine, and resembling sloughing cellular tissue more than anything else.

## MEDICAL NEWS.

### ACTION FOR SEDUCTION—BROMWICH *v.* WATERS.

*Professor Simpson* asked permission to detail a case which had lately occurred in England, of great and grave interest to the medical profession. He was anxious to draw their attention to it at this meeting, as, at his request, his friend Dr Ramsbotham of London was present, and he was desirous that Dr Ramsbotham should have an opportunity, if he thought proper, of sustaining the opposite side of the question. Dr Keiller, who was also present, would with Dr Ramsbotham aid him in stating the case correctly to the Society. The case itself was important as showing how any medical man might any day be subjected, though perfectly innocent, to the most unfounded accusations and gravest danger. Besides, it involved the momentous question, how far physicians of high standing in the profession were justified in going into the witness-box, and assisting lawyers and others there to persecute and ruin an honourable and blameless brother physician.

The case to which he wished to call the attention of the Society was the subject of a trial last week at the Chester Assizes, at which Dr Keiller, Dr Ramsbotham, himself, and others were medical witnesses.

The defendant in this trial, Dr Waters of Chester, had strong claims on our sympathy and respect. He was a Fellow of our College of Physicians, one of the gold medallists at our University, formerly Senior President of our Royal Medical Society, one of the most accomplished and esteemed provincial physicians in England, and possessed, he believed, of the largest practice in Chester. He was about fifty, married, and the father of a grown-up family; but latterly he had been persecuted almost to his ruin and death by what looked more like a dark and foul conspiracy than aught else. Some four years ago, he (Dr Waters) was in charge of a gentleman, in Chester, dying of consumption. He had pronounced him to be the subject of phthisis. Dr Gully of Malvern, another graduate of our University, but, he was sorry to say, rather an "irregular" practitioner, as they should find, was brought down to see this patient. He visited him without speaking on the subject to Dr Waters; and Dr Gully declared that Dr Waters was quite wrong in his diagnosis; that the ailment was not phthisis, but gouty congestion of part of one lung associated with some curable form of dyspepsia. The gentleman died. Two practitioners, unconnected, he (Dr Simpson) believed, with either Dr Waters or Dr Gully, examined the body, and found that death had resulted from consumption, proving that Dr Waters was entirely right and Dr Gully entirely wrong. The lady, who, he (Dr S.) was informed, was chiefly instrumental in bringing down Dr Gully, was implicated to such an extent, that she felt extremely irritated. In short, it seemed to have appeared to her a case demanding retribution; and we all know, how Byron writes it, "Revenge is sweet, especially to women."

About February 1860, Dr Waters was asked to see Mary Whalley, the servant of Miss Bromwich, an old maiden lady living near Chester, who was an intimate friend and close ally of Dr Gully's female patron there. This woman Whalley was then about twenty-nine or thirty years of age, being now thirty-two according to the official evidence adduced in Court, though there she swore that she was even now only twenty-eight. She had been ill for years, and was, when Dr Waters was asked to see her, suffering from rejection of food, retching, and occasional vomiting of blood. At first he supposed that she might have ulceration of the mucous membrane of the stomach, or hepatic derangement, or possibly some organic disease. On examining the upper part of the abdomen and chest, he could find, however, no evidence of any organic malady. Various medicines were used for some time without much effect. Subsequently she began to complain of pains affecting the sacral and hypogastric regions, and stretching down the limbs. She specially complained of spasms connected with the vomiting, originating in the hypogastrium, and stretching upwards. She likewise became affected with leucorrhœa. These symptoms naturally and properly led Dr Waters to suspect some disease of the uterus, and he thought himself called upon to ascertain if any such existed. On making a digital examination, the cervix uteri was found to be tender to the touch. In making this examination, Dr Waters declared on oath in Court that he found no difficulty in introducing two fingers. Sergeant Shee, in opening the case, stated that Dr Waters introduced two fingers into the vagina at this first examination. In the "verbatim" report of the trial, the Judge asked Dr Waters:—"You were aware she was not a virgin when you used it (the speculum), or before you used it?—The digital examination satisfied me that she was not a virgin, and I felt less delicacy in using the speculum on that account. The woman herself has sworn that I introduced two fingers. Now I hold that two fingers cannot be introduced into a virgin vagina." He then examined the tender neck of the uterus by means of the speculum, and discovered it to be congested, swollen, and with its surface roughened by superficial ulceration or excoriation. He touched the excoriated part with solid lunar caustic a few times; generally at intervals of a fortnight or longer. She went in April 1861 to the Isle of Man, and was there attacked with fits of a hysterical character. She had one of especial severity on her return voyage from the island towards the end of May. According to her own evidence in Court, after this special fit "the hysteria continued regularly." Dr S. mentioned these dates particularly, as it had been sedulously attempted to be averred at the trial that the hysteric fits were brought on by the use of the speculum and kept up by it. On the contrary, they came on and were developed to the greatest degree when she was absent from Chester, and away from all local medical treatment. The report of the trial in the *Times* newspaper gave an entirely wrong impression of the true state of the case in this respect. The speculum and caustic were not used after the spring of 1861, and the hysterical fits came on when she was absent from Chester in April and May. From time to time, however, Dr Waters prescribed for her till the commencement of the following spring of 1862. From one of her visits to Dr Waters, on a Saturday in November 1861, Whalley returned home in a cab. It was the only occasion on which she did so. That day was an important one so far as regarded the object of the trial, for it was *the* day which Whalley fixed upon as the date on which Dr Waters had used such liberties with her as to make her subsequently become a mother. The learned and very careful Judge who presided at the trial, Baron Bramwell, ruled in his charge to the jury that the circumstances adduced in evidence about the sale of a pony belonging to Dr Waters, etc., fixed this important Saturday to be the 30th of November 1861. Two women, who had been servants of Dr Waters, appeared as witnesses, and gave full particulars of Whalley's visit to Dr Waters' house on this occasion. One of these servants, Emma Bradbury, now a married woman, swore that she opened the door of Dr Waters' house to Whalley and showed her into the study, where Elizabeth Jones, another servant of Dr Waters, happened to be at the time. Shortly afterwards, on coming up again from the kitchen, she found Whalley had removed into the breakfast-room, sitting on a chair before the fire, tossing her head about, and not looking well. When she spoke to Whalley, and asked her what was the matter, she said she was very ill. On this the servant went to Dr Waters, who up to this time was engaged with another patient, and told him of Whalley's sick appearance. "He told me," to quote the words of Bradbury's own evidence, "to ask my mistress to give her a glass of wine. I went to my mistress, who was in the dining-room, and Mrs Waters gave me a glass of wine, which I gave to Whalley, who drank it. She afterwards said to me, 'I shall be better just now.' Within a few minutes her sickness increased, and she vomited on the hearthrug and fire-irons, and lay on the floor."[1] Bradbury visited her every ten or fifteen minutes during the two or three hours she remained in Dr Waters' house. The Judge himself specially interrogated Bradbury as to Whalley's state during that time, and she declared that, whenever she visited her, she found her always hearing, seeing, and speaking rationally; never, in short, in the least degree unconscious or insensible. Dr Waters' other servant, Elizabeth Jones, saw Whalley when she came into Dr Waters' house, and subsequently offered to help her up when she found her lying on the floor. Whalley stated to her that the walk from her mistress's to Dr Waters' had proved too much for her. "I was," to quote this witness's words, "in the room with her about a quarter of an hour or twenty minutes. She was quite sensible all the time. She told me she had cancer." One of these servants at last called a cab, at Mrs Waters' suggestion, and Whalley drove home in it. "No one went back to fetch her bonnet and shawl when she got into the cab, as she had them on, and they had never been off during the whole of the time she was in the house." When she left the house, Dr Waters was, as sworn to by his two servants, not at home, and did not return for some time afterwards. He had been called out early in the afternoon to the paddock, about the sale of the pony alluded to, and had subsequently taken a drive with his children. Dr Waters was uncertain whether or not he saw Whalley that afternoon; and Bradbury, his servant, swore that she visited her so frequently in her sickness, that if the doctor did see her, which she thought he did not, it must have been for a very few minutes only. Sometimes, she added, he went out and forgot his patients in the house. Mrs Waters gave evidence corroborative of these statements of Bradbury and Jones concerning the wine, cab, etc. But, in the charge brought against her husband, the wine which he so kindly and innocently had ordered her, was hypothetically changed by Whalley and her abettors into a potion of strange unknown power, and this unhappy woman altered the simple facts connected with the preceding visit to Dr Waters' house into the following chain of cool and calculated misrepresentations and falsehoods. He quoted her evidence from the *Chester Chronicle*, to the following effect:—

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[1] These extracts were read chiefly from the Report of the Trial in the *Chester Chronicle* of April 4. One or two extracts are from the fuller reports taken in short-hand.

"His (Dr Waters') house is about a mile from my mistress's house in Boughton. I went into the consulting-room on that occasion, and felt very cold and faint. I asked the servant, was the doctor in? She said, 'Yes;' and I saw him in a few minutes. He asked me how I was, and I replied that I was very poorly. He then said he would fetch me something to refresh me, and he brought me something in a glass, and offered it to me to drink. The liquor appeared to be wine. I said I was afraid of drinking because it disagreed with my liver. He said I was to drink it, and it would refresh me. I then drank the liquid in two or three draughts. I scarcely recollect doing anything after that, but I felt some one unfasten the upper part of my dress in front. I don't remember anything more until I was awakened by the doctor. I was then in another room, lying on the hearthrug. When I awoke I felt very sick. I do not know if the doctor was then present when I was sick. He afterwards asked me, was I ready to go? I told him I felt so ill that I did not think I could walk. He said, 'You had better take a cab to go home, as it is nearly six o'clock.' A female servant then came into the room, and I was taken to a cab. I missed my shawl and bonnet. I told the servant I wanted my shawl, and she went back to the room and brought it to me, and I put it on. I was driven home to Boughton, and as I could not get out myself, I was assisted to the house by the cabman. I have not had any operation performed on me by Dr Waters since then. He once afterwards offered me something to drink, but I said I would rather not have it, as the other made me so ill. In Dr Waters' house I did not see any woman till I was taken up. None of Dr Waters' servants ever gave me anything to drink—not a glass of wine. No woman gave me any wine."

It was needless to quote this woman's evidence further, or to comment upon it. All the leading facts which she thus swore to as true were sworn to as utterly *untrue* by Dr Waters' household. The draught given was simple wine and nothing more. It was *not* given, as Whalley swore, by Dr Waters himself, but by Mrs Waters and one of his servants. It produced no insensibility or unconsciousness, as Whalley swore it did. She did not see Dr Waters when she got the wine, nor speak to him about the cab at the end of her visit as she averred, for he was not then in the house at all. She spoke to both servants, and often; though she averred she spoke not to any. She did not leave her bonnet and shawl behind, for they were never off; and so on. She made other as unfounded statements when in the witness-box regarding imaginary conversations with Dr Waters, as that Dr Waters asked her not to tell that caustic was applied, etc., etc., all of which were sworn to by Dr Waters as equally and as utterly untrue.

The preceding visit to Dr Waters was on the 30th November. If she had become pregnant on that day she should have been delivered in the first week of September; but five or six weeks before that date, on the 26th of July, she produced a child, which was admitted by her own witnesses and counsel to be the usual size of a child at birth at the full time. There was a fatal anachronism, therefore, as to the alleged date of her impregnation when she fixed on the 30th of November. In the course of the trial Whalley's counsel argued that, if she had invented the story, she would surely have taken care to place her alleged ill-usage by Dr Waters at a proper date, and that therefore the want of correspondence between the alleged date of impregnation and the acknowledged date of delivery was an argument in favour of the truthfulness of her story. The learned Judge, however, sagaciously observed that those who invented lies usually fell unwittingly into some clumsy error by which the truthlessness of their tale could be proved. When Whalley averred that Dr Waters was the father of her child, she indulged in a statement of which, according to her own evidence, she had no proof, because, according to her own story, she was insensible at the time. Even taking up her own truthless tale, was there any more legal evidence in it that Dr Waters was the father of her child than that any other man in Chester was the father of it? Some of the evidence adduced in the trial did not appear to have left Baron Bramwell with any exalted idea regarding the improbability of there being other fathers for Whalley's child in Cheshire. In his charge the Judge observed that there had been "called several witnesses for the purpose of showing that the woman Whalley was a light character. She had been made out to be particularly intimate with Joe Smith, but she denied Joe Smith altogether, except on the occasion of the shilling bet with the washerwoman that he dare not kiss her. Then they had Nelson, who had seen her in a very improper condition. They had Letitia Bailey and Martha Jones, both distinctly swearing that they had seen Joe Smith kiss and 'clip' her. Harriet Woods had also sworn the same. Martha Jones's evidence was unimpugned; she had spoken to Whalley having put her arms round Joe Smith's neck, and had threatened to tell his wife. Was that an invention? How could they think that all these women came into Court for the purpose of committing gross perjury? Whalley had denied all this, but she was positively contradicted by these witnesses. Harriet Woods went further, and proved that Whalley had told her that she had allowed more than one person to take indecent liberties with her. Next came Anne Barker. If they believed her, the child's father was at once accounted for. Anne honestly owned to a great dislike to Whalley; but if she had come there and perjured herself, why not have told the additional lie of saying she did not dislike her? He (the Judge) considered this a marvellous story, and said that if it stood alone, and there were no confirmatory evidence, he should have cautioned the jury against believing it. They had these five witnesses impeaching Whalley's conduct."

But to finish the remainder of Whalley's case, it was necessary to remark that, with her mistress, Miss Bromwich, she went to Malvern in April 1862, and was soon after placed under the care of Dr Gully, who treated her as if she were suffering from a uterine tumour. In the course of her evidence at the Chester trial, Whalley swore, "I felt something moving in my stomach. I asked Dr Gully if I was in the family way?" Dr Gully's opinion, however, of her case was comically enough stated in Court by the counsel for Miss Bromwich as follows:—"Miss Bromwich was told by Dr Gully that it was all nervous nonsense; that the tumour had enlarged, was becoming active, and might discharge, and the consequences of its dispersion might be very serious." As already stated, the "active" tumour did discharge its contents on the 26th July in the form of a full-sized child. It was not till two or three months afterwards that Dr Waters heard of Whalley and the termination of her case. The first announcement in regard to it was in a letter from a lawyer in September, informing Dr W. that Whalley had produced a child of which he must be the father. It turned out that the child had been baptised Edward Waters, after him. Dr Waters, of course, denied the paternity. The late action at the Chester Assizes was an action brought by Miss Bromwich to recover from Dr Waters damages for the loss of the services of her servant Whalley. "There was," said the Judge, "a difference in this case between an action brought by a parent for the seduction of his daughter, and one brought by a mistress for the loss of the services of her servant. In the former case, the plaintiff, if he recovered, would be entitled to substantial damages; whereas, in the latter case, which was not of such grave importance, nominal damages might suffice." One or two simple facts besides this show the animus with which the case was conducted against Dr Waters. Mr Serjeant Shee, one of the most learned, perhaps *the* most learned and able pleader at the present moment at the English bar, was brought to Chester to conduct the prosecution against Dr Waters. Serjeant Shee did not belong to these Assizes, and it is a most unusual thing thus to bring a special pleader out of his circuit for a special object. The fee which he received for his share of the work was said in Cheshire to be something almost fabulous. Altogether, it was averred by legal authorities there that Miss Bromwich and Co. must have spent from £2000 to £3000 in laying the prosecution before the Court. And yet this old maiden lady had the effrontery to allow her old servant Whalley, for she had been many years with her, to be attended by Dr Waters as a pauper, without remuneration She was ready to spend her money to prosecute the doctor, but not to fee him. Nay, she was not content with taking advantage of Dr Waters' professional services gratuitously, but seemed to argue rather angrily in Court, as did her counsel, that Dr Waters should have answered, also gratuitously, all the consultation letters with which this idle old lady bored him regarding this hysterical servant of hers. And of what, after all, did the evidence against Dr Waters consist? Of five lay witnesses, if we may call them so, and, he regretted to add, of four physicians. The five lay witnesses were Whalley, her mistress, Miss Bromwich, and her factotum, a man Chew, and his boy, whose sole evidence consisted in stating that they had seen Whalley come home in the cab; and the cabman, who proved that he had driven Whalley from Dr Waters's house to Miss Bromwich's. Whalley, as we have seen, swore to a number of statements which Dr Waters' servants and Mrs Waters proved to be utterly false. She swore to others in regard to her communications with Dr Waters, which Dr Waters swore to be as totally false. Miss Bromwich's evidence amounted to little or nothing, except that she had had communications with Whalley about the speculum being used, etc., and that the child was baptized in Dr Waters's name "for fun."

The evidence of the dreadful charge against Dr Waters amounted thus actually to nothing; but something was tried to be made out against him by the plaintiff bringing in four physicians as witnesses. While the action was being got up and prepared, Dr Waters and his legal advisers were repeatedly told that the medical evidence against him was *the* chief evidence, and quite overwhelming in its character. Three of the physicians who were witnesses for the prosecution were men of high standing in the profession, brought down for this special object from London. To a Medical Society their medical evidence in this terrible case would be a matter of interest.

Dr Taylor he (Dr S.) would first notice, as being non-obstetrical. What he had come down to say they did not know, for he said he knew nothing about ulceration of the cervix uteri. He (Dr S.) fancied that he was, perhaps, brought down to say, as he *had* said in his book, that a woman might become impregnated when unconscious. But this evidence was not needed, for the proof was complete that the woman was never unconscious. But as Dr Taylor *was* in the witness-box, he (Dr S.) wrote out a question, which he handed to Mr Welsby, the able leading counsel for Dr Waters. Dr Taylor has written a most excellent book on toxicology. He was asked, "Do you know of any drug or poison which, when swallowed, would instantaneously, or almost instantaneously, produce insensibility, as this woman Whalley has said"—Dr Taylor hastened to say, "Yes, I do." "Stop, sir," continued Mr Welsby, till I finish my question, "—and from which the woman would spontaneously recover in the course of two or three hours?" "I know many (said Dr Taylor) which would throw a person into instantaneous insensibility, but not without causing death." He (Dr S.) supposed Dr T. was thinking specially of prussic acid and its compounds; but surely no doctor would ever give a poisonous dose of these for any such felonious purpose as that ascribed to Dr Waters. Dr Taylor, in fact, gave excellent evidence in favour of Dr Waters, though he was brought by the prosecutor for another purpose.

They had two other medical witnesses from London, Dr Lee and Dr Ramsbotham. "There was Dr Lee," said Baron Bramwell, "who was not altogether a satisfactory witness, although, doubtless, a very learned man.[1] Dr Lee was first asked if he had made a personal examination of Mary Whalley. He replied, "I did," and produced a bundle of papers, being notes of examinations he had made of her person, with the fingers and speculum, *six months after delivery*, first along with Mr Dunn, and then, subsequently, with Dr Ramsbotham. These notes he was not allowed to read. He said that he had spent two hours in examining her, and found the parts in the most healthy condition. But, more, he denied that the cervix uteri ever *could* have been ulcerated. Now, Dr Waters had all along stated that the ulceration which he discovered and treated in Whalley two years before Dr Lee examined her was "superficial," and every medical man and medical tyro knows that superficial ulcerations of mucous surfaces, as of the mouth, nose, and eye, did not leave afterwards any marks by which they could be recognised. And for a man to come down the distance that Dr Lee came, to make such a statement in a case where a professional brother's character was involved, was, to say the least of it, painful in the extreme. Dr Lee further declared, that "the speculum had been most grossly abused" by Dr Waters in this case of Whalley. He admitted that the speculum had been very extensively employed all over the world in the exploration of diseases of the uterus. On being cross-interrogated as to whether the use of it would be proper where there was inflammation of the uterus, he declined to venture an opinion on the matter, and, like a Scotsman, requested to be informed what was meant by inflammation, saying he did not understand the term. He was asked whether he understood the meaning of the phrase "inflammation of the orifice of the uterus," and replied that he should not, because the term was not definite. With regard to "irritation," he said that this was a term with no meaning. The counsel then inquired whether he would understand the meaning of "superficial ulceration." He seemed doubtful about it. He (Dr Simpson) had taken up with him, he knew not how, a copy of Dr Lee's "Clinical Reports," and had handed it to Mr Welsby, who now read aloud, from page 113, the following observation regarding the use of the speculum in some uterine affections:—"In some cases of inflammation and superficial ulceration of its orifice [the orifice of the uterus], important information may be obtained from its use." "Are you the Dr Lee who is the author of this book?" asked Mr Welsby. "There is no other Dr Lee but myself," he replied. "Does that passage," inquired Mr Welsby, "convey to you any definite idea?" The perplexed witness avowed in answer that these, his own published words, did not convey to his own mind any very definite idea. As a further reason for not agreeing with the statement which he had published in his own book regarding the use of the speculum in inflammation and superficial ulceration of the os uteri, he argued that the book was written ten years ago. Then followed a conversation between the counsel, Mr Welsby, and him, of which the following is the principal part:—*Mr W.*—"Am I to understand that you explain the unintelligibility of the phrase 'inflammation and superficial ulceration of the orifice of the uterus' by the fact of its being written by you ten years ago?" *Dr L.*—"It *was* written ten years ago." *Mr W.*—"May I ask how old you are?" *Dr L.*—"I am ten years older than when that book was written." *Mr W.*—"That will not convey to the jury or to me any definite idea." *Dr L.*—"You'll have some difficulty in finding out my age." *Mr W.*—"Do you refuse to tell me?" *Dr L.*—"I do not know it. I have as vague a notion of that as I have of the meaning of the term 'superficial ulceration.'" This answer was perhaps as strange as some of Dr Lee's other answers on this occasion; for, in the biography of him published, amongst other medical biographies, in the *Lancet* in 1851, the account of Dr Lee begins with the statement that he was born in 1793, and goes on to give other minutiæ of Dr Lee's early history that scarcely any other one except Dr Lee himself could have supplied to the writer, if indeed he was not the writer himself, most of those lives being little or nothing else than autobiographies. "Will you have the goodness to tell me," continued Mr Welsby, "how long you have studied upon these diseases?" In answer, Dr Lee spoke of his studies in Edinburgh and Paris. "I was two years," he added, "with Prince Woronzow" [what the Prince had to do with uterine disease it was difficult to imagine], "and since my return to London in 1827, I have never left for a holiday." "You have," concluded Mr Welsby, "got a holiday now, at all events."

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[1] In the "verbatim" report published in the *Chester Record*, Judge Bramwell's exact words are given, as follows:—"I make no observation on the evidence of Dr Robert Lee; he is a gentleman of experience beyond doubt, but *not* a very satisfactory witness. He seemed to be in a state of great excitement, and the effect of his evidence was that the treatment was improper."

Dr Ramsbotham next entered the witness-box. His evidence was much more satisfactory. He stated,—"He examined Mary Whalley with the speculum, and discovered no marks of an ulcer having been in the womb. Had there been an ulcer there, there would have been a cicatrix of the wound. Of late years the speculum was very largely used; but he never used it himself on a young unmarried woman, unless she had been the patient of some other practitioner before and had been operated upon. He had great objection to using that instrument unless there was great distress in the womb, when the employment of it would give relief. There were reasons for not using it. In the first place, the mark of virginity disappeared, and, in the next place, before the introduction of the speculum the fingers must be introduced, which was repugnant to the feelings of the woman. If there had been any superficial excoriation, generally it gave way to six or seven applications of caustic, if applied once a-week. Superficial ulceration was very apt to recur, and it was possible that, after a few weeks, another application would be necessary. Caustic could not be applied except by means of the speculum. Supposing, after the using of the speculum, the person, not being previously subject to hysteria, became affected by the use of it, it should be discontinued. From the description he had heard of Mary Whalley's illness in the Isle of Man, he would say that it was a form of hysterical complaint. It might have been cataleptic hysteria, epilepsy, or hysterical coma. As a matter of medical science, he believed it to be decidedly possible for impregnation to take place while the woman was in a state of insensibility, and very distinguished authors, both in England, France, and Germany, had expressed a conviction to that effect." Before passing from this last item in Dr Ramsbotham's evidence, Dr S. said that, theoretically, it seemed possible that impregnation may occur in the female when she is unconscious; but, practically, it was difficult to prove its occurrence in individual instances, as they were almost all liable to many and obvious sources of fallacy. He had seen a case of the kind where, if the strong avowals of the woman could be trusted, impregnation could only have taken place when she was unconscious from chloroform. The case was thus:—

About two years ago, an old patient of his (Dr S.'s) came to him with a woman who had what was said to be an abdominal tumour. He examined the tumour, heard the fœtal heart, and stated that she was pregnant. She averred that, if it was so, she could have got into that condition only when a surgeon, well known to all of them, was operating on her for piles or some other slight disease of the rectum, and had given her chloroform. She most solemnly asseverated that she had never allowed a man to touch her. He thought it right to go to the surgeon and tell him that this accusation had been made with regard to him, that he might be on his guard. He was, Dr S. believed, written to by some lawyers on the subject, but the details of the charge were so vile and truthless, that he was ashamed to speak of them to his friend till Dr Waters' case came up. The surgeon then told him that the case had been dropped long ago, as far as he was concerned. But the later history of the case was instructive. The woman, who kept a large lodging-house, is making two of her lodgers pay for the child, as both of these gentlemen had been cohabiting with her, and they could not settle between themselves which of the two was the real father of the child.

But there was a fourth medical witness—Dr Gully, with whom, as their friend Dr Moir, who was present, could attest, Dr Lee was known to consult professionally. Dr Gully had gone off to France before the trial came on, but was examined by commission; and evidence more absurd and deplorable was never given. It was read as follows in Court, from an official document of which he (Dr S.) had now a copy in his hand:—"In May 1862, I was called in to see Mary Whalley. From what she told me I treated her for tumour of the womb. About 26th June 1862, I went, at the desire of her mistress (Miss Bromwich), to see Mary Whalley. On that occasion I examined the mouth and neck of her womb and the vagina. She gave me every facility for the examination. It was all done at her request. My attention was called to the state of her breasts. Upon pressure the breasts gave out a milky fluid. She called my attention to that also. My attention was also called to a swelling of the belly. I placed my hand upon the belly and kept it there for twenty minutes or more in order to detect any movement, but I found none. I did not discover anything the matter with her liver either at that time or previously, although I examined her with the object of ascertaining that also. I did not discover any trace of ulceration of the womb. I found the mouth of the womb much more open than is usual in pregnancy. It was also swollen and rough on the surface. The neck of the womb was also swollen. These parts presented the appearances of having been operated upon by instruments. I made the examination in consequence of a statement made by Miss Bromwich and by Whalley also, and I looked for signs of pregnancy. I came to the conclusion that she was not pregnant, partly from my own examination, and partly from her statement of the previous medical treatment. I came to the conclusion that she was suffering from tumour of the womb. I treated for tumour till 26th July. Her liver was perfectly healthy as far as I could discover. The effect of my treatment on pregnancy would be to accelerate the birth of a child. The circumstances of the case induced me to examine the parts, and they presented the appearance of those of a woman with small sexual desire. *Cross-examined.*—I first saw her (Whalley) sometime in May. Between that and the 26th July I must have seen her from fifteen to eighteen times. Early in May I examined her belly. I then treated her for tumour of the womb. I made altogether three or four examinations of the belly before the 26th day of June. Her monthly courses had stopped, she told me, from the previous November My examination on the 26th June lasted half an hour quite. Openness of the mouth of the womb is a sign sometimes of excessive sexual connexion. By excessive I mean a woman having connexion with a man once or twice every night, nothing less. Three times a-week would not produce such. The instruments I refer to in my examination in chief are those used in a certain new practice which has sprung up within the last ten or fifteen years of treating affections of the womb locally with instruments, such as speculums, dilators, etc., which are introduced for the purpose of making a visual examination of the mouth of the womb. From my experience of other cases where instruments had been used, I inferred that in this case instruments had been used. I saw her two or three times between the 26th of June and the birth of her child. I still treated her for tumour, and still considered she was not pregnant. The things from which I inferred she was not pregnant, but had a tumour, were her statement that Dr Waters had, fifteen months before, said she had tumour of the womb, and my own examination not discovering any movement in the mass, and that Dr Waters, whom I considered a skilful and able man, had treated the womb as a diseased organ. I administered to her homœopathic medicine for her sickness (vomiting). The sickness proceeded from sympathy with the womb. The appearances from which I inferred that she was a woman of small sexual desire were, that the passage of the womb was narrow, the external parts contracted, whilst in a lascivious woman they would be large and open. The clitoris was very small, and there was apparently no sign of amativeness in the head, which I examined as a phrenologist.

*Re-examined.*—In this particular case I did not consider that the openness of the mouth of the womb could be attributed to excessive sexual intercourse, because the mouth of the womb was rough as well as open. I think the clitoris would not have been so small had she been guilty of sexual intercourse. In my opinion, as a medical man, the use of instruments at all to any unmarried woman under thirty or thirty-five years of age is highly injurious."

It is difficult to comment upon evidence such as that of Dr Gully's, for perhaps in the annals of medicine and medical jurisprudence it is unequalled in its character. It is an extraordinary compound of ludicrous professional pretension, and equally ludicrous professional ignorance. Dr Gully swears that the womb and the neck of the womb "presented the appearance as having been operated on by instruments, and as having been injured by instruments." We all know that in swearing so, there was a pure misstatement (not to use a stronger phrase), because none of the instruments could have left any traces whatsoever, and Dr Lee himself could find none after a prolonged examination. But whilst Dr Gully swore he discovered what every man knew was undiscoverable, and did not exist, he did not, on the other hand, discover what every old woman in the country would have discovered under the circumstances, that the patient was pregnant, though the patient herself, as she stated in the witness-box, suggested to him the possibility of her being so. Whalley told Dr Gully that she had the catamenia suppressed; and that she "felt something moving in her stomach;" there was swelling of the uterus; there was milk in the mammæ; sickness, and every marked symptom of pregnancy; and yet Dr Gully assured Miss Bromwich, to quote again the words of her counsel, Sergeant Shee, "that it was all nervous nonsense, that the tumour had enlarged, was becoming active, and might discharge, and the consequence of its dispersing might be very serious." Would any old petticoated nurse in this country have committed such an unpardonable blunder as not to recognise pregnancy in so well marked a case as the above? This hapless woman was submitted by Dr Lee to an examination extending over two hours. Dr Gully stated that on one occasion he examined her "for half an hour quite," and placed his "hand upon the belly and kept it there for twenty minutes or more!" What an unnecessary act when we have such a simple and speedy means of settling the point, which every educated medical man knows, viz., the use of the stethescope. And yet this same man swears that he knows—what neither he nor any other man could know—signs or marks indicating that instruments for the "visual examination" of the uterus had been employed months before! He appeared to have submitted her himself when in this state of pregnancy to the use of the speculum—a proceeding rarely adopted under such circumstances—and did not actually know what he saw. Every medical man at the present day knows that in the last weeks of utero-gestation the os uteri is generally open more or less, and that its mucous membrane appears usually reddish and rough. Dr Gully sees this natural and common condition of the pregnant os uteri, and what does he infer from it? The language which he uses is not only the language of ignorance, but almost the language of indecency. "My examination," says he, "of the 26th June lasted half an hour quite. Openness of the mouth of the womb is a sign sometimes of excessive sexual connexion. By excessive I mean a woman having connexion with a man once or twice every night—nothing less. Three times a-week would not produce such." What an improper life must this unmarried woman have been living at Malvern, and the Water-cure, if Dr Gully's observation had any truth in it, which, fortunately for the character of Miss Bromwich's household, it has not! Dr Gully states that the vagina and clitoris were not large, and hence that Whalley was a woman of "small sexual desire." He (Dr S.) was not aware that any English physician had made observations on this subject, and if Dr Gully has done so, and the preceding inference is the result, then Dr Gully's observations are utterly opposed to the various high French authorities who have attended to these indelicate points on the prostitutes of Paris, and of other continental cities where this kind of examination of the bodies of licensed prostitutes is, for sanitary purposes, enforced by law. How Dr Gully arrived at the conclusion that he, forsooth, could measure the amount of sexual connexion to which a woman was subjected by the degree of openness of the os uteri, is quite incomprehensible. Whom did he dare to examine and to cross-question for this purpose? Let us hope that it is a mere reckless misstatement, and not an inference from observations attempted to be made. Dr Gully adds further, that he examined Whalley "as a phrenologist," and came to the same conclusion! Besides making this ludicrous diagnosis of her non-amative propensities phrenologically, Dr Gully owns to having treated her hydropathically and homœopathically. It is only a pity for his own sake that he had not recourse to other such means as mesmerism and spirit-rapping, to escape the droll errors of diagnosis and so-called treatment into which he fell.

Dr Simpson stated, that of the medical evidence for the defence he had not heard the examinations of Mr Brittain and Dr Fyfe; but he believed they gave it as their opinion that the medical treatment of the case by Dr Waters was correct, and in accordance with the practice of most physicians of the present day. The following was the evidence of Dr Keiller, as described in short-hand notes sent here to-day from Chester:—"I am a Fellow of the Royal College of Physicians, have had considerable practice in midwifery, and have delivered lectures in the College of Surgeons upon it. Supposing superficial ulceration of the orifice of the uterus to exist, I consider Dr Waters's treatment proper. I do it myself every day. I use the speculum in such cases in my own practice every day. If hysteria exists, as in this case, I have often recommended marriage. If there is hysteria combined with uterine irritation, the recommendation of marriage would be proper in some cases. If irritation only, it would be decidedly improper. Superficial ulceration of the orifice or irritation are likely to recur. Examination with the speculum is quite usual alone,—that is, without the presence of a third person. If two women come together, one usually prefers to retire. *Cross-examined by Mr Serjeant Shee.*—Where there is hysteria combined with uterine irritation, the recommendation of a husband would be proper. And with uterine ulceration it would not be improper in every case to recommend a husband. I have seen cases where impregnation cured prolapsus of the uterus. If the ulceration were slight, it would be proper to recommend marriage. It is only the local irritation of coition which causes any inflammation, not the impregnation." Dr Simpson himself was examined also for the defence, and stated that Dr Waters' treatment appeared to him to be quite proper. The members of the Society were fully aware that in the treatment of superficial ulceration of the cervix uteri, the application of nitrate of silver was a means far more frequently used by the profession than any other. It was the local application most trusted to by medical men for the treatment of superficial ulceration of the os. Indeed, Dr Lee himself, in his "Clinical Reports," says as much; for, after describing the "superficial ulceration" of the os uteri of most modern writers, or the "granular state of the os uteri," as he names this morbid state, he adds as to its treatment, "Gently rubbing the os uteri with lunar caustic, through the speculum, a few times, at long intervals, has appeared to effect all the good which such local treatment can accomplish."—P. 137. The treatment for the "granular" or "ulcerated" state of the os uteri, which thus Dr Lee himself recommends, was the treatment followed by Dr Waters in Whalley's case; and Dr S. felt sure that Dr Ramsbotham had treated many cases of this affection exactly in the same way. But if this disease occurred in an unmarried woman, should it be treated in this, its acknowledged best way, or not? A question to that effect was put by the counsel to Dr S., and he answered as most medical men would answer it, that the profession avoided using the speculum when they could avoid it; but if the disease defied discovery and treatment by all other means, its use might become a matter of necessity. In such a case as Whalley's, where two fingers could be admitted when a vaginal examination was made, there surely could be no strong objection to the use of a speculum to make the diagnosis and cure more certain. And perhaps few or no men in the profession would ever, he believed, dream of using it where the hymen was present or the vaginal orifice very narrow. Dr S. had never used it under such circumstances; and it was certainly in other and many cases had recourse to where its employment was not in the least required, as in cancer of the uterus, polypus, displacement, etc. The information it afforded was indeed extremely limited, for it merely showed more distinctly than the finger sometimes could ascertain, the presence of superficial ulcerations and eruptions upon the os and cervix uteri. The affection in which Dr Waters employed it in Whalley's case was exactly the affection in which its use was looked upon as most generally necessary,—on which again, according to Dr Lee's own work and words, "*important* information may be obtained from its use." (*See* his "Clinical Reports," p. 113.) During the trial the use of the speculum was alluded to by Serjeant Shee or others as a practice in which the patient was placed upon her back, the clothes thrown back, and the person of the patient fully exposed. In Edinburgh no practitioner used the speculum, he believed, in this way. Nearly twenty years ago, Dr Simpson had written a short paper, showing that it could be used equally well, perhaps better, with the patient lying on her side, the instrument introduced below the bed-clothes, and afterwards the clothes wrapped around the lower end of the tube, so that all the medical attendant saw was the os and cervix uteri, or roof of the vagina, at the upper end of the tube. There was thus no exposure whatever of the external person or parts of the patient. But, during the trial, a far graver charge was brought against the use of the speculum. In his opening address to the jury, Serjeant Shee stated that the use of the speculum "produces such an excitement of the person, such a lasciviousness in the woman, by its operation upon the most sensitive part of her person, and often excites the man himself to such a degree, that it is not always safe." When asked in his examination if the speculum was thus liable to excite sexual desires in either of the parties, he (Dr Simpson) answered that he never felt more amazed than when he heard the observations of Serjeant Shee on this point, they were so gross and so untrue. And he felt sure that every member of the Society would join Dr Waters in his statement, that the use of it does certainly not produce such feelings, but most assuredly the very opposite. Of course, Serjeant Shee could not speak from his own experience, nor could he have been instructed to this effect by Dr Taylor, who honestly and properly disavowed his knowledge of it, nor, he felt sure, by Dr Ramsbotham. The merit of this strange instruction must apparently be ascribed to Dr Lee. If such were the results of Dr Lee's experience, he (Dr S.) felt sure that the medical profession as a body would declare that Dr Lee's own experience in this matter must be a rare, if not a solitary exception to the general rule. At the suggestion of the Judge, the counsel for the defendant asked the common duration of pregnancy. The answers were, nine months and a week; and the probable date of delivery in Whalley's case should have been the first week of September, if she were impregnated on the 30th November. But the child, as already stated and sworn to, was born on the 26th July, and was "a fully-developed child." Serjeant Shee asked if the hydropathic treatment to which the patient was submitted, namely, a lamp-bath, etc., might not have produced labour prematurely. Dr Simpson answered, "It might possibly accelerate the labour; but certainly it could not change a premature into a full-sized child." Other questions and cross-questions passed of no immediate importance to the inquiry,—such as, how it was known that the usual duration of pregnancy was really about nine months, or nine months and a week. Dr Simpson answered, from accumulated observation on the human female, and direct experiments on the lower animals. *Serjeant Shee:* "The lower animals in an inquiry of this kind!"—*Dr Simpson:* "Yes; for in this question a cow may be compared to a countess."—*Serjeant Shee:* "A counsel?"—*Dr Simpson:* "No; a countess."

The case, as far as Dr Waters was concerned, was a question almost of life and death. The accusation brought against him by Miss Bromwich was an accusation which not only struck at his moral and professional character—

which not only threatened irretrievable ruin to him and his family; but, as the Judge showed, it amounted virtually to an accusation of rape, and he was liable, if it had been true, to have been tried for his life for that crime. The direct legal evidence against him as to his being the father of Whalley's child, what did it amount to? To nothing whatever, except Whalley's allegations or rather suspicions. And what were her allegations? That she had been given by Dr Waters a draught which immediately made her insensible, and that she remained insensible from two to three hours. But her own witness, Dr Taylor, who is so learned in drugs and poisons, owned, we have already seen, that he knew no drug or poison, nor does any other man know any drug or poison which, when swallowed, could produce immediately an insensibility from which the patient would recover in a few hours. It was proved that Dr Waters gave her no drug; but that Mrs Waters filled out for her a glass of wine; that her alleged insensibility from it was a deliberate and systematic lie; and that, in short, she was never insensible; and all her story about Dr Waters seeing her before she took the wine and when she began to waken up, was a series of groundless and clumsy falsehoods. Besides, the child, which was born on the 26th July, fully sized and developed, must have been begotten not on November 30th but in October; and yet Whalley's only suspicion or accusation against Dr Waters referred to the 30th of November, the day in which she came to his house and went home in a cab, the only day in which she ever used a conveyance. If any one asks *why* did this wretched woman invent the stupid story, the answer is perfectly simple. She knew and felt herself in the family way, suggested her being so to Dr Gully, and at last was confined. Now, mark, she was a very old servant of Miss Bromwich's, but would never have retained her place and support from Miss B. and her friends if she had honestly and even penitently confessed that child to have been begotten as other illegitimate children are begotten. It was necessary, to save her place and character, to "find a hero," as the Judge remarked, and a father for it; and more, it was necessary to find or invent a *very peculiar father in this case*, one, namely, whom she could persuade Miss Bromwich she (Whalley) had no knowledge of, one who must have impregnated her when she was herself utterly insensible. She fixed, in consequence, on Dr Waters, and invented, to cover her own misdeeds, the lying and transparent story of a draught having made her insensible in his house, etc. In order to save her own character she scrupled not to destroy the character of the doctor who had been so kind to her. But to us medical men, a graver chapter follows in this disgraceful trial. Whalley's case must probably have broken down at once, if three great London doctors had not been brought down to appear as witnesses in *her* favour, and, consequently, *against* their persecuted professional brother. It seemed as if it was expected to prove that Dr Waters' moral conduct must have been improper towards Whalley, if they could only once first prove that his medical treatment of her case was improper. Some men in the profession always keep behind the advancing state of medicine. But are such men entitled to give evidence which may brand the moral character of another physician because they happen thus to differ from him on a point of medical opinion or medical treatment? The idea is monstrous. Is it not time yet for the medical profession to wake up and raise their voice against such doings? Dr Lee was the principal and leading medical witness against Dr Waters, and look at what he does. To the dishonour of our profession, judges and lawyers can boast that if three medical men in a point of medical jurisprudence and evidence will swear in one way, they usually can manage to get three to swear the opposite. But in this trial a far more strange phenomenon occurred. Mary Whalley's counsel got Dr Lee to swear against himself,—Dr Lee as the medical witness of 1863 to swear against Dr Lee as the medical author of 1853. And what would have been the effect of this cross-swearing of Dr Lee's if it had unfortunately been believed. It might have sent an innocent and accomplished brother physician to ruin and destruction—if not to death. No thanks to Dr Lee and his medical coadjutors that a jury of Dr Waters' countrymen found him, with the hearty concurrence of the Judge, utterly and totally guiltless.

*Dr Ramsbotham* then rose and stated that he should now answer what Dr Simpson had said. His (Dr R.'s) appearance in that box was not with any intention of running Dr Waters to death, or of maligning him, but quite the contrary. It *did* appear, perhaps, as if he had acted unprofessionally in coming into a witness-box for a party who brought such a charge against a professional brother; but he was led into it accidentally and unintentionally; and when he was in the case, he thought his appearance would be more for Dr Waters' advantage than his detriment, and that his appearance might counteract the very strong opposition which Dr Lee had expressed to the solicitors for the prosecution against the speculum ever being used. He asked to be allowed a few words in reference to the history of what happened to him. A gentleman called on him one day, whose face he knew, but whom he did not recollect, and asked him some questions. "Do you think," he said,—he (Dr R.) began to think from his appearance and manner of speaking that he was a medical man,—"do you think that a woman could go to the full time without knowing it?" He (Dr R.) said "Certainly, yes. A lady came to me lately within a fortnight of her time without knowing it." "Do you think it possible," he then said, "for a woman to be impregnated when unconscious?" "Yes," he (Dr R.) replied, "either from opium or disease, or even sometimes from very heavy sleep in consequence of fatigue." He went away with a "very much obliged to you." In a few days he (Dr R.) received these questions and some others to answer in writing, not on lawyer's paper, nor in form of a legal document, and was asked to see this girl Whalley. He began to suspect, for the first time, that there was some legal question brewing. He went to see her, and found Dr Lee there, of whom he had not heard before in connexion with this case. They saw her together. Dr Lee had seen her some days before, and had examined and questioned her very carefully for two hours. They both concluded that there was no disease *then*, and that there was no cicatrix showing that there had been a deep-rooted ulcer before. This, the lawyers who were present caught hold of, and he (Dr R.) was asked to meet Dr Lee again at a consultation of the counsel. He told the counsel, after having seen the girl, that their case would certainly break down, and that they could not substantiate what he then found for the first time was being brought forward, and he used all the persuasion that he possibly could to both the lawyers, the practitioners, and Miss Bromwich who was in the room. He tried for an hour to make them give up the case, saying that the expense would be great, that they would lose it, and that his evidence would go against them rather than for them. But they had him on matter of fact, not of opinion. "You saw that woman, and saw that her uterus was healthy." In February, six months or more after delivery, when he (Dr R.) first saw her, the uterus to the touch felt quite healthy, and there was no appearance of enlargement or ulceration. Then when he came down, after having tried all he could to persuade them not to carry on the case, he said in the witness-box that there was a distinction between a deep and a superficial ulcer; that if there had been a deep ulcer in the mouth of the womb, the cicatrix would have been observable six months, perhaps a year or two afterwards; but that if there had been merely a superficial ulcer, the mucous membrane had such restorative powers in itself, that it would soon restore itself to its natural and previous state. He (Dr R.) would only tell them this, that the animus with which he took up this case was very different from that with which Dr Simpson had presumed he acted. Mr Serjeant Shee had certainly made the most of the speculum, not certainly from information on his (Dr R.'s) part, but prompted by Dr Lee. He (Dr R.) did not feel that he had at all to be blamed for having appeared; for, though nominally against Dr Waters, he was much in his favour.

*Dr Simpson* said the Society were much obliged to Dr Ramsbotham for the statement he had just made, and he (Dr R.) to him (Dr S.) for giving him an opportunity of making it, for the case was certain to create some noise among the profession in England. Dr Ramsbotham argued that he should not be regarded as an enemy to Dr Waters, or an abettor of the allegations against the propriety of his practice, because he appeared as a witness against him. But, if it were nothing more, the kind of company in which he appeared, viz., among the medical men drawn up to testify for the plaintiff and not the defendant, made the Judge himself take a different view of Dr Ramsbotham's opinion from what Dr Ramsbotham himself now does. For Mr Baron Bramwell had averred, in his summing up, "There was Dr Ramsbotham, who gave his evidence in a very satisfactory manner, and who also expressed his opinion that the treatment was improper."

*Dr Ramsbotham* replied that the Judge must have misunderstood him, or twisted his words when he made him (Dr R.) thus speak of the treatment as being improper. He had said that he had never used the speculum in an unmarried woman unless it had been used before, and Dr Waters said from the first that she was unchaste in his opinion, from his getting two fingers into the vagina. Miss Bromwich was one of the most obstinate old ladies he had ever met with. She had said, as they were at lunch next day in the hotel where they were all living together, "Is it possible that we have no redress? Is this poor girl's character to be taken away by individuals who were suborned to perjure themselves by saying she is not a chaste woman?" The counsel who was there said, "The jury have given their decision, and we can say no more." One of the younger counsel said to him (Dr R.) at the trial, "If I could keep my self-possession in the witness-box as that girl Whalley did, I would be very glad." And it was just the same when she was examined by Dr Lee and himself and all of them. She was, doubtless, on their supposition, an impostor; but she was not only one of the most heartless impostors, but one of the *best* impostors (if he might use the expression) that ever was known. With regard to Dr Taylor, he might state that Miss Bromwich first wanted to make it out that the glass of wine which Whalley drank had been drugged by Dr Waters to throw the girl into insensibility, that he might fulfil his foul desires. The matter was submitted to him (Dr R.). He said that the thing was quite impossible. Dr Taylor said the same thing, and it was to prove this that Dr Taylor was brought down. He (Dr R.) was very glad the case had terminated as it had, and he believed the solicitor for the prosecution was not displeased himself.

*Dr Simpson* remarked that, as to what Dr Ramsbotham had said about Dr Taylor, he (Dr S.) might observe, that what answers a witness came to give were brought out by his evidence in chief. Dr Ramsbotham thought that Dr Taylor was feed by Miss Bromwich to come down and swear that it was impossible that any drug could do what Whalley said the potion administered to her had done. Now, no such question was put during his examination in chief; but the matter was brought out on cross-examination by Dr Waters' counsel. To pay Dr Taylor that he might thus damage fatally her case was not probable; and Dr Taylor would not, of course, take Miss Bromwich's fee and come to Chester with the intention of acting directly against her interests. There was one other point which he (Dr S.) forgot to mention. The question was raised whether hysterical women were not often given to systematic falsehood and imposture. There was evidence brought from Dr Prout's work, and from other books, to show this; and they all knew what thorough impostors hysterical women sometimes were. Dr Ramsbotham in his evidence repudiated this opinion.

*Dr Keiller* asked permission to say a few words. He was glad that Dr Ramsbotham was present to-night, and that he had made a statement to the effect that he never intended to appear as a party against Dr Waters. It could not but be gratifying to this Society to hear Dr Ramsbotham express himself as he had just done; but he (Dr K.) could not help thinking that Dr Ramsbotham, in appearing in the witness-box on the side of Miss Bromwich, had adopted a strange way of exhibiting his sympathy for a professional brother who had been dragged into a court of justice to meet such an obviously trumped up and most injurious accusation. If Dr Ramsbotham appeared at Chester as if against Dr Waters, and yet was for him, such an anomalous position was not at least discoverable during the trial, for not only in matter, but in manner, the evidence of Dr Ramsbotham seemed altogether opposed to the interests of Dr Waters. Had Dr Ramsbotham not now affirmed that he offered no opinion in the witness-box as to the propriety of Dr Waters's treatment of the case, he (Dr K.) would have felt justified in asserting that Dr Ramsbotham did express his opinion of Dr Waters' treatment as improper. As for Dr Lee's appearance at the trial, he (Dr K.) not having personally seen Dr Lee before, but knowing and esteeming him by reputation, at first thought that the wrong man had stepped into the witness-box, for it was difficult for some time to believe that this mistake had not really occurred; his demeanour and answers were so very extraordinary. It would be difficult to describe the ludicrous scene enacted during Dr Lee's examination. The evidence forced from Dr Lee was, to say the least of it, unequivocally one-sided, and, such as it was, singularly biassed. He (Dr Lee) did not in the least degree pretend to mince matters when the subject of the speculum was touched upon; and even after leaving the witness-box, he conspicuously continued to display his disapprobation of the instrument, by publicly exhibiting specula of various shapes and sizes in the body of the Court, before the Judge and Jury, and passing them round for the special inspection of the apparently much-amused rows of barristers. When he (Dr K.) was first asked to appear for the defence, the serious nature of the charge, and the position of the parties interested in its legal determination, naturally induced some anxiety as to the result, more especially after reaching Chester, and there finding three such men as Drs Lee, Ramsbotham, and Taylor to contend with. After, however, hearing the evidence for the prosecution, the result could not for a moment be doubted. He (Dr K.) had no hesitation in saying that a more trashy and transparent case never was before tried in a court of justice, nor one more worthy of receiving a decision which it is to be hoped will prove well calculated to teach the unscrupulous and over-credulous accusers a lesson not to be forgotten.

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## EDINBURGH UNIVERSITY GENERAL COUNCIL.

On the 21st of April, the half-yearly meeting of the General Council of the University of Edinburgh was held in Queen Street Hall—Vice-Chancellor Sir David Brewster presiding. On the platform were Professors Lee, Crawford, Stevenson, Balfour, Lyon Playfair, Fraser, Kelland, Pillans, and Aytoun, and Dr Alexander Wood.

*The Rev. Professor Lee* having opened the meeting with prayer,

The minutes of last meeting were read by the Secretary and approved of.

### PUBLICATION OF PRIZE-LISTS.

*The Rev. K. M. Phin*, Galashiels, then moved,—"That the General Council represent to the University Court that it is desirable to resume the annual publication in the Edinburgh newspapers of the list of honours awarded in the several Faculties and Classes."—He said he was at a loss to imagine any reason why the motion should not be at once unanimously adopted. It was well known that until last year there were regularly published in the newspapers the prizes and honours conferred by the University, and last year was the first year in which this ancient practice was departed from. He was astonished when he observed that the usual list was not to appear in the public prints. They all knew that it was a great stimulus to young men in the prosecution of their studies that at the end of the season their names were to appear in the public papers as having obtained certain honours and prizes. He thought that in that way many students were encouraged in their arduous efforts. He believed, further, that their relatives and friends were very much gratified when they saw their success openly acknowledged to the world. (Hear.) What could have put it into the mind of any one to abandon this most excellent, and, as he thought, most salutary practice, was beyond his imagination. He begged to call the attention of the General Council to the fact that another very important body, the Royal College of Surgeons, was at this moment carrying on that very practice which this University last year thought it right to depart from. It was calculated to discourage students from attending the University, and to make them prefer the classes in connexion with the Royal College of Surgeons, that this practice should be departed from. He had been told that those who had determined that in future the list of prizes and honours should not be published, had been influenced by two considerations. In the first place, he was informed that they would be told that there were no funds to meet the expense of printing the prize-lists. He found from the Ordinances of the University Commissioners, printed in the University Calendar, that "the matriculation, examination, and graduation fees, and the registration fees of members of the General Council, shall be placed to account of a general University fund, which shall also include all the revenues of the University," and this fund was to be spent, among other things, in the expenses connected with the Senatus Academicus, the University Court, the General Councils, the salaries and wages of servants, and class expenses, including prizes. Why, then, should not a portion of this fund be employed in printing the prize-list, when the prizes were distinctly recognised by the University Commissioners as a legitimate part of the expenditure? He would rather that they should make the prizes smaller in value than abandon this excellent practice which had hitherto prevailed. (Hear.) Then he was informed that they might possibly be told that one of the objects of those who had departed from the old practice was to gain a circulation for the Edinburgh University Calendar, and that by printing the lists in the Calendar that publication would be made self-supporting, which it was not at present. He thought this was carrying economy too far; and if the University Calendar was not to be self-supporting except they ceased to print the prize-lists in the newspapers, he would rather that the old practice should be continued, and that the University Calendar drop out of view. He concluded by submitting the motion.

*Dr Andrew Wood* seconded the motion.

*Professor Kelland*, while agreeing with all that had been stated by Mr Phin, could not arrive at the same conclusion. In consequence of deficiency of funds the list of honours could not be announced both in the University Calendar and in the newspapers, and as the Calendar was more permanent, the Senatus preferred that medium of publication. Professor Kelland concluded by moving the previous question.

After some discussion, in which Drs Alexander and Andrew Wood, and Mr Alexander Nicolson, spoke in favour of the motion, and Professors Lyon Playfair, Lee, and Stevenson, supported the amendment, Professor Kelland withdrew his amendment, and Mr Phin's motion was unanimously adopted.

### RIGHT OF ADMISSION TO CEREMONIES OF GRADUATION.

*Mr Phin* then moved the adoption of the following motion:—"That the General Council represent to the University Court that members of the University Council whose annual payments are not in arrear should have a preference over those not connected with the University in the arrangements for admission to any meeting at which the degree of LL.D. is conferred by the Senatus." He said that his object in this motion was simply to assert what he believed to be one of the privileges and prerogatives of the Council. He went on to refer to the arrangements for admission at the recent ceremonial of conferring the degree of Doctor of Laws upon Lord Palmerston, maintaining that members of the General Council ought to be invited to attend on such occasions. He ridiculed the arrangement which provided that the graduates should on that occasion attend in academic costume, and said that, as Messrs Snip and Co. advertised next day that they would furnish the necessary apparel to graduates, it occurred to him that it was possible there might have been some understanding with these gentlemen. (Hissing.) Of course that was a joke, but he thought that such an intimation as to costume was entirely out of place, and that the General Council should have an opportunity of being present on all such occasions. (Applause.)

After some conversation, the motion was carried.

### THE FUNCTIONS, PRIVILEGES, AND RIGHTS OF THE GENERAL COUNCIL.

*Mr Phin* next brought forward the following motion:—"That a committee be appointed to report to the next meeting of the General Council upon the functions, privileges, and funds of the General Council, and upon the present condition of the register." He thought it was of the utmost consequence that they should know what their functions and privileges really were, as contradictory opinions had been expressed in the Council on that subject. They all knew that their distinguished rector had given it as his opinion that they could not petition Parliament, and yet they had since petitioned Parliament, and their petition had been received by Parliament. The powers of the Council related to all matters connected with the wellbeing and prosperity of the University; and he thought that it was of great consequence to have some information as to the extent of their powers in relation to these matters. It was of great consequence also to know how many were on the register, and how many had paid up what was necessary to secure them the privileges of a member of the General Council. It had been asserted that they could not appoint a committee, because one of the Ordinances of the Commissioners laid down the rule that they could not delegate any of their functions to a committee. But to appoint a committee was not to delegate their functions, as the committee would report, and leave the Council quite free to exercise all its functions. It was of great importance, he thought, that the General Council should assert its own prerogative. (Applause.) Their prerogative and independent position had been kept too much in abeyance, and he believed that other University Councils had exerted their prerogatives more than they had done. Other General Councils had a committee to arrange their business, and they had never appointed such a committee. He wished it to be clearly understood that beyond empowering the Vice-Chancellor, Principal, or senior Professor present to take the chair, the Act of Parliament and the ordinances of the University Commissioners took nothing out of the hands of the General Council. There were persons who imagined that they had power to tell them that this or that piece of business was to be done at any particular time; but from the moment the chair was taken everything was in the hands of the University General Council. (Applause.) It was competent to any member, the moment the chair was taken, to move that they should proceed to business, or to move that such an one open the meeting with prayer; and it was in consequence of their not asserting their privileges that the idea got abroad that they were a mere creature of the Senatus, and must take rules for their guidance from the Professors. He had the utmost respect for the Professors. He thought they were highly privileged in having such Professors as they had at present in this University. (Applause.) But he looked upon the Professors here as simply members of the General Council, and nothing more. They might wear a particular dress, or sit in a particular place, but with the exception of the Vice-Chancellor, Principal, and senior Professor, they had no privileges as distinguished from those of any of the other members of the General Council. (Hear, hear, and applause.) He protested against anything occurring which could by possibility lead to the idea that the possession of any such privileges by the Professors was recognised by the General Council. He was happy that it had occurred to him to bring this question before the General Council, in order that they might declare fully and freely what they thought of this matter, and assert their own privileges and prerogative.

*Professor Aytoun* said he could not see the meaning of the attack which Mr Phin seemed to him to have just made upon the Professors, as if they were assuming some peculiar dignity, or claimed in the General Council any special privileges, or were taking upon themselves any function more than was possessed by any other member of the General Council. Since he had been a member of the General Council he had never heard any privilege claimed by any Professor beyond those enjoyed by other members of the General Council. He must say, therefore, that fervid declamation of this sort seemed very unnecessary; it seemed rather to resemble a sort of convention hall, as if one part of the Council were claiming precedence over the other. He maintained that it was not so. He did not think it necessary to meet this motion by an amendment. He did not see why, if the members of the General Council thought it right to be informed of the state of their own body, they should not make the necessary investigation. But he must say that it would be rather beyond their power to inquire into the matter of funds, when they knew distinctly that these funds were by ordinance paid over to the University funds, of which the Senatus had the control and management, subject to the control of the University Court,—a matter he never doubted. If there was any idea or supposition that the members of the Senatus were viciously intromitting with the funds, or squandering them, that being barely stated would lead to an inquiry by the University Court. But he thought that it would be a very ungracious thing for the University Council, when there was no such idea abroad, to have a motion to inquire into the administration of the Senatus Academicus regarding the funds. He thought that his friend Mr Phin might take the reference to the fund out of his motion. He did not see that there was any practical objection to the rest of the motion.

*Professor Lee* moved as an amendment that the motion be not adopted. After some discussion the question was put to the vote, when Mr Phin's motion was carried by an overwhelming majority.

The following gentlemen were then appointed members of committee:—Professor Bennett, Dr Alex. Wood, Dr Andrew Wood, Dr Struthers, Dr Alex. Niven, Alex. Nicolson, Rev. D. Thorburn, Messrs Beatson Bell, J. M'Candlish, D. Stevenson, J. Darling, and W. S. Dalgleish, Dr James Duncan, Mr Macduff of Bonhard.—Rev. K. M. Phin, *Convener*.

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## MEETING OF THE MEDICAL PROFESSION IN CHESTER ON DR WATERS' CASE.

On the 13th ult., an important and highly influential meeting of members of the medical profession of Chester and the surrounding districts was held at the Infirmary, to express sympathy with Dr Waters on the recent action brought against him and decided in his favour. The following gentlemen were present:—Dr Davies, Chester; Dr Williams, Mold; Mr Morris, Marford; Mr Moreton, Tarvin; Dr Braid, Neston; Mr Churton, Chester; Dr Biggs, Kelsall; Dr Fyfe, house-surgeon, Chester; Dr Stolterfoth, Chester; Mr Jones, sen., Ruthin; Mr Bage, Chester; Mr Dickson, Chester; Mr Brittain, Chester; Mr Weaver, sen., Chester; Mr Brierly, Tattenhall; Mr Thelwall, Farndon; Dr Bury, Chester; Dr Jephcott, Chester; Dr Williams, Wrexham; Dr Powell, Chester; Dr Bantock, Chester; Mr Ffoulkes, Banbury; Mr O'Kelly, Chester; Mr John Harrison, Chester; Mr Job Harrison, Chester; Dr Brittain, Chester Infirmary: Dr J. D. Weaver, Chester; Mr Griffiths, Wrexham; Dr J. C. Davies, Holywell; Dr Hill, Hooton; and Mr James Williams, Holywell. Dr Davies was called to the chair, and Mr John Harrison (No. 16 Nicholas Street) acted as Secretary to the meeting.

The *Chairman*, in opening the proceedings, said they had met together on a very important, although very painful occasion. It was most important, inasmuch as the honour and welfare of the profession, as well as the character of an individual member of it, had in some degree been at stake. It was not necessary for him to enter largely into the circumstances of the case. They all knew that a gentleman, an ornament to his profession, and one who had gained the credit and goodwill of a large number of persons, had unfortunately fallen into a position which any of them might have fallen into—that is to say, he was made the victim of a person whose character no doubt had been such as to deceive her intimate friends. She had brought herself into discredit, and evidently wanted to shield herself at the expense of her medical friend. They all knew the satisfactory way in which that charge had been met, and how very handsomely Dr Waters had been treated by all parties in the case, the Judge himself expressing his full conviction that the charge was not borne out by the evidence. However, the matter would not be allowed to rest there; they had a duty which they owed to themselves and to the profession. Their profession was one peculiarly liable to such charges, and whenever a brother fell under an imputation of the kind, it behoved them all to rally round him, and endeavour by every possible means to defend him from these imputations. They had met there with that view. A somewhat analogous case had lately occurred in London, in which Dr Adams was made the victim; but that gentleman got triumphantly through a very painful lawsuit, and met with the sympathy of almost every medical man in the profession. He (the chairman) only hoped the same warm sympathy would be manifested on this occasion. Although Dr Waters was placed in a trying position, he (the chairman) hoped that the difficulties under which he had been labouring would only serve to bring more fully into view the many good qualities he possessed, and prove him more worthy and deserving even than ever of the regard and esteem of his medical brethren. (Applause.)

The *Secretary* then read letters from the following medical gentlemen expressive of the great interest they felt in the case, and regretting their inability to attend the meeting:—Dr Hughes, Mold; Dr Cumming, Malpas; Mr J. C. Davies, Holywell; Dr Proudlove. Tattenhall; Mr Walter Wm. Jones, Ruthin; Dr Dixon, Northwich; Mr Williamson and Dr Henry Williamson, Nantwich; Dr Dobie, Chester; Mr Edward G. Clarke, Mold; Mr T. E. Jones, Wrexham; Dr Seller, Tarporley; Dr Dixon, Surgeon, Flintshire Militia. The Secretary referred particularly to a letter received from Dr Bennett of Edinburgh, which stated, that, considering the great benefit which Dr Waters' manly and straightforward conduct in the recent action was likely to bestow on the medical profession, nothing it appeared to the writer would be more appropriate for them than to express their opinion in some tangible shape. He suggested that a general subscription might be entered into that might suffice not only to express their sense of his merit, but their desire, by means of union, to put down the odious calumnies and injuries to which they were all exposed. He thought, however, that the movement should originate amongst the medical profession in Chester, who might jointly form themselves into a committee for this purpose. The writer continued, that he would be glad to take charge of the Edinburgh subscriptions, to which he would gladly contribute, and recommended that the testimonial should be presented to Dr Waters at a banquet to be held in Chester, say early in August. The Secretary said he thought this letter of Dr Bennett's very important indeed; and although that meeting had been nominally called for the purpose of congratulating Dr Waters on the issue of the recent trial, the letter was valuable as showing the feeling amongst the profession in distant places on the subject. (Hear, hear.)

*Mr H. Churton* then moved the following resolution:—That a subscription be entered into by the medical profession generally for the purpose of assisting to defray Dr Waters' expenses in the late trial, and for presenting him with such a testimonial as may be agreed upon by the subscribers.

*Mr Ffoulkes* seconded the resolution, which was carried unanimously.

*Mr Griffith*, Wrexham, said that looking at all the circumstances of the case, they must come to the conclusion that Dr Waters had not only come forward as a defender of his own case, but also in some measure as the defender and champion of the honour and rights of the profession at large. (Hear, hear.) There was not a gentlemen there present who might not soon be dragged into the same difficulty. Character and circumstances were no protection, and if they would hope for that sympathy and assistance which under similar circumstances they might themselves expect, they should now show their willingness to extend not sympathy merely, but the more solid advantages of pecuniary aid. (Hear, hear.) The expenses of the action to Dr Waters must be something very considerable and fall heavily upon him, and the mere verdict in his favour was not a sufficient protection against the expenditure incurred. He was very happy to attend there and express sympathy on the part of some gentlemen at Wrexham, who were unable to come to the meeting, but who had deputed him to speak for them. Several of those gentlemen had made arrangements to give their attendance, but circumstances had afterwards occurred which prevented them from doing so; and they desired him to say that they were ready to co-operate with a committee in any manner to testify to Dr Waters the deep sympathy which they felt for him under the painful circumstances under which he was placed, and their admiration of the bold, English way in which he had confronted the charge. (Hear, hear.) He thought they ought not to separate without some remark—it might be a delicate matter to do so—but he did think the time was come when some remark should be made respecting those professional gentlemen who had come down from London in the case, darkening the character and blasting the reputation of a fellow member of their profession. (Hear and loud applause.) On looking at the trial he could not conceive that either Dr Lee or Dr Ramsbotham—for he had no delicacy about names—could form an *ex post facto* opinion and venture to say the treatment adopted by Dr Waters was wrong. (Hear, hear.) Not one of them—and although they did not bear the high names of the two gentlemen he mentioned, still they possessed common sense, they possessed a knowledge of their profession, and most of them had a good deal of experience—would venture to say in such a case that the treatment was improper. It might have been all very well if it had been the case of an ignorant person, who might have adopted wrong treatment, and then in the interests of the profession and the public he should be held responsible; but there was no such evidence here. What was known of the necessities of the case at the time it came under Dr Waters' care? (Hear.) Something was due to the reputation and experience and skill of Dr Waters in his diagnosis of the case, and applying proper treatment to it. He (Mr Griffith) believed that Drs Lee and Ramsbotham had no adequate foundation for condemning the treatment of Dr Waters in this case, and guided their statements on preconceived ideas as to a certain point of practice in the treatment of these diseases. As to the speculum, they all knew that Dr Lee had for a long time been waging a crusade against its use, and he thought there was a point on which the meeting were bound to express an opinion, namely, to discourage men from being made the tools of any party, to fill up gaps in evidence, sacrifice their reputation, to disregard those feelings of delicacy and honour which should exist between one man and another in the profession; and the time had come when they should generally express their disapprobation of such a course of proceeding. (Applause.)

In answer to an inquiry made by a gentleman present, the *Chairman* said the subscription to be entered into would be strictly confined to the medical profession, inasmuch as there would be a meeting the same evening of a more public character, at which it was intended to have another subscription. In referring to Dr Bennett's letter, he said his (Dr Bennett's) suggestion was what the meeting intended itself to have proposed.

*Mr Harrison* (55 Nicholas Street) said there had been a preliminary meeting at which he had been asked to take upon himself the duty of drawing up an address to Dr Waters. This address at first contained some remarks respecting the course taken by the two medical gentleman who had come down from London, but, on reconsideration, it was thought better to omit these portions. Mr Harrison then read the address proposed to be adopted, as follows:—

*To Edward Waters, Esq., M.D., F.R.C.P. Ed., etc., etc.*

DEAR SIR,—We, the undersigned, medical practitioners in the city and county of Chester and the adjacent counties, assembled at the Chester Infirmary, April 13, 1863, beg to tender you our hearty and most cordial congratulations on the issue of the recent trial which has just occurred in this city. The rumours circulated during the past winter in connexion with your name caused us great pain, and it was with an anxiety scarcely inferior to that which you yourself must have felt that we awaited the time when an opportunity should be afforded you of vindicating your character from the aspersions that had been cast upon it. The result has amply proved the correctness of our anticipations; and we now sincerely rejoice that a member of the profession, whose name has been hitherto held in the highest esteem amongst us, has passed unscathed through the painful ordeal to which you have been exposed. Having carefully watched the progress of the trial, we beg to express to you our great satisfaction at the verdict, and to offer you the united testimony of our unabated confidence in your professional judgment and skill, as well as in your honour and integrity as a man; and we should be glad if we could believe that this slight tribute on our part could afford any consolation to you for the great suffering which you and the other members of your family must have endured for so long a period, and while passing through the painful and appalling trial to which you have been subjected. We beg to subscribe ourselves, dear sir, your faithful friends.

*The Address was signed by all the Medical Gentlemen present*

*Mr Churton* said it would be well to form a committee to devise the best mode of proceeding in the matter. He thought the bare presentation of a testimonial—say a piece of plate—would not be sufficient; Dr Waters had been at a great outlay in getting up his defence, and he ought to have something in money. (Hear.)

*Mr Brittain* asked, would the subscription amongst themselves interfere with that to be entered into in the evening?

The *Chairman* replied that it would not.

*Mr Brittain*—Because if the other subscription goes to pay the law expenses, ours might be appropriated to the testimonial.

*Mr Harrison* then moved the adoption of the address to be presented at a proper time to Dr Waters.

*Mr Moreton* (Tarvin) cordially seconded the proposition, and observed that they would be justified in expressing their opinion to Dr Waters on the conduct of Drs Lee and Ramsbotham.

The *Secretary*—Don't you think it would be better to leave that to the medical press? Otherwise I think it would be taking it out of good hands.

*Mr Brittain*—I believe there are several medical gentlemen in Manchester and Liverpool who have expressed a wish to join us in the subscription.

The *Secretary* said that he thought it would be better to have separate meetings in those large towns, as it would have much more effect than having one common meeting in Chester.

The *Chairman*—Would it not be well that we should advertise the resolutions in the Medical Journal?

The *Secretary* remarked that it would be desirable to have a complete account of the present meeting drawn up and sent to the Medical Journal in a formal manner, with a request that the whole proceedings should be inserted.

The adoption of the address having been put to the meeting it was carried unanimously.

A committee was then appointed to carry out the various arrangements, and a vote of thanks having been passed to the chairman the proceedings concluded.

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## THE CAUSE OF ENTERIC (COMMONLY CALLED TYPHOID) FEVER.

(*To the Editor of the Edinburgh Medical Journal.*)

Clifton, 6th April 1863.

SIR,—In a review of Dr Murchison's work on Continued Fevers, in your current Number, the writer does me the honour to refer to some views of mine on the etiology of enteric fever.

The discussion of these views concludes with the following remark:—

"But as there is evidence that enteric fever does originate *de novo*, and as Dr Budd's theory does not explain how this takes place, we are forced to seek for a more general mode of causation, and this we find in the action of the gases emanating from sewers."

In connexion with this remark, I venture to suggest that your Reviewer would make his position much more clear if he would state in your next

Number, in precise and definite terms, what evidence there is to prove that enteric fever originates *de novo*, which would not prove the same of small-pox; also, or, to take a still stronger case, which would not, by exact parity of reasoning, prove that all animals and plants which propagate by minute and impalpable germs are constantly being generated *de novo*, too.—I have the honour to be, Sir, your obedient servant, WILLIAM BUDD.

[In reference to Dr Budd's observation we have merely to remark, that in Dr Murchison's book and in the writings of other authors we have accounts of outbreaks of enteric fever manifesting themselves under circumstances where there seems to have been no possibility of its having been imputed, and where there could have been no materies morbi derived from individuals already affected. We do not maintain that it is absolutely proved that gases emanating from sewers generate typhoid fever, though this has been rendered highly probable; and as this doctrine is more general than that of Dr Budd, and is applicable to the explanation of cases where his theory is inadmissible, we feel ourselves constrained to give it the preference.—THE REVIEWER.]

## PUBLICATIONS RECEIVED.

Baines,—Excessive Infant Mortality. By M. A. Baines. London, 1863.
Bruns,—Supplement by Dr Victor v. Bruns. Tubingen, 1863.
China,—Report of the Medical Missionary Society in China. Hongkong, 1863.
Cobbold,—Remarks on all the Human Entozoa. By T. Spencer Cobbold.
Farina,—Menton. Par le Dr J. F. Farina. Paris, 1863.
Hassall,—The Urine in Health and Disease. By A. Hill Hassall, M.D. Second Edition. London, 1863.
Hayward's Heath, Fourth Annual Report of Lunatic Asylum at. 1862.
Imbecile Youth in Scotland, Report of the Society for the Education of. 1863.
Insane,—Reports of the Officers of the Retreat for the, at Hartford, Conn., U.S.—1860–61–62.
New Sydenham Society,—Kramer's Aural Surgery. Translated by H. Power, F.R.C.S., etc. 1863.
New Sydenham Society,—Year-book of Medicine, Surgery, etc., for 1862.
Rodrigues-Barraut,—Cholera. Par A. R.-Barraut, M.D., Edin. Mauritius, 1863.
Smith,—On Human Entozoa. By W. Abbotts Smith, M.D., etc. London, 1863.
Symonds,—Waste. By John A. Symonds, M.D., etc. London, 1863.
Tanner,—Cancer of the Female Sexual Organs. By Thomas Hawkes Tanner, M.D., etc. London, 1863.

## PERIODICALS RECEIVED.

Boston Medical and Surgical Journal,—March 5, 12, 19, 26.
British and Foreign Medico-Chirurgical Review,—April. London, 1863.
British Medical Journal,—April 4, 11, 18, 25.
Canstatt's Jahresbericht,—Index for 1861. Wurzburg, 1862.
Dublin Medical Press,—April 1, 8, 15, 22.
Gazette des Hôpitaux,—March 24, 26, 28, 31, April 2, 4, 7, 9, 11, 14, 16, 18. Paris, 1863.
Gazette Hebdomadaire de Médecine, etc.—March 27, April 3, 10, 17, 24. Paris, 1863.
Gazette Médicale d'Orient,—March. Constantinople, 1863.
Gazette Médicale de Paris,—March 28, April 4, 11, 18, 25, 1863.
Glasgow Medical Journal,—April, 1863.
Health and Meteorology of Manchester,—February 1863.
Journal de Médecine et de Chirurgie,—April. Paris, 1863.
Journal für Kinderkrankheiten, Nov. and Dec. and Jan. and Feb. Erlangen, 1862-63.
Journal de la Physiologie (Brown-Séquard's),—July, October. Paris, 1862.
Journal of Mental Science,—April. London, 1863.
Madras Quarterly Journal of Medical Science, No. 11, January, 1863.
Medical Critic and Psychological Journal,—No. 10, April. London, 1863.
Medical Times and Gazette,—April 4, 11, 18, 25. London, 1863.
Revue de Thérapeutique Medico-Chirurgicale,—April 1, 15. Paris, 1863.
Social Science Review,—April 18. London, 1863.
Vierteljahrschrift für die praktische Heilkunde,—Vol. 1. Prague, 1863.
Virchow's Archiv,—Nos. 1 to 6, vol. 26. Berlin, 1862-63.

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ERRATUM.—We regret that, in the physiological Periscope in the March Number of this Journal, Dr Beale was inadvertently stated to be the author of the researches "On the Properties of the Saliva of the Parotid and Submaxillary Glands." The author should have been stated to be Professor Eckhard of Giessen, his researches having been published in Dr Beale's Archives.—ED. EDIN. MEDICAL JOURNAL.

# Part First.

## ORIGINAL COMMUNICATIONS.

### ARTICLE I.—*On the Diagnostic Value of an Accentuated Cardiac Second Sound.* By J. WARBURTON BEGBIE, M.D., Physician to the Royal Infirmary.

(*Communicated to the Hunterian Medical Society, March* 27, 1863.)

IT is now universally admitted that the second sound of the heart is produced during the act of closure of the semilunar valves in the orifices of the aorta and pulmonary artery. The sudden tension of the membranous structure of which these valves are composed is a sufficient, possibly the *only*, cause of the sound. It is, however, probable that, as generally held, the recoil of the blood against the valves contributes to its production.

Careful clinical observation has materially aided the direct experiments which, at a former period, were made regarding the heart's sounds. As respects the second sound, it may indeed be concluded that, by the former means of research, much left unfulfilled by the latter has been supplied. For example, I may refer to one or two particulars of importance, which have a special bearing on the subject of this paper. When the second sound is entirely replaced over the base of the heart by a murmur, it is not audible over the ventricles, and is not to be detected at the apex of the organ. In other words, when the murmur of aortic regurgitation is so loud as to drown all normal second sound, preventing, by its very loudness, the recognition of the pulmonary second sound over the pulmonary valves, or in their immediate neighbourhood, where the second sound originating there is most readily heard, it cannot be detected even in small part over any other portion of the heart. Here, however, there are some points worthy of special observation; they concern what may be called exceptional cases of aortic insufficiency. A diastolic murmur may largely obscure, but not obliterate, the second sound over the aortic valves, a portion of it remains; and in such cases, the pulmonary second sound being either readily appreciable or at least audible, the murmur diminishing in loudness as the stethoscope is placed over the ventricles, the normal second sound, pulmonary in origin, or the portion of aortic second sound which remains, is with greater readiness discovered there, or even at the left apex, than at the base. In endeavouring to account for this circumstance, it must be held in remembrance that the murmur of aortic regurgitation is not conducted with anything like the same distinctness over the ventricles as it is down the course of the sternum to the very limit, in some instances, of the ensiform cartilage itself. I have found not unfrequently that the second sound, greatly obscured by murmur at the base, and having precisely the same character at the end of the sternum, has been partially unclouded at the left apex, and over the ventricles a little less so. In such instances, it has not been difficult to determine that the more ready recognition of the sound in the latter situations has been due to the loss in distinctness sustained by the murmur.

Dr Walshe has noticed "a distinct sound at the left apex in more than one case, while at the aortic base the ordinary regurgitant murmur alone existed."[1] So also in cases in which the second sound at the base is only feebly heard—no murmur existing—there may be, if not a loud, at all events a more readily recognised second sound near or at the apex. To such instances Skoda has directed attention, and they have likewise been fully considered by Dr Walshe. In explanation of their occurrence it may be, as Skoda has suggested, and Dr Walshe is disposed to allow, that some of the phenomena occurring during the diastolic action of the ventricles, which are properly, or rather naturally, soundless, become attended by sound,—in other words, produce a second sound of their own; or it is equally conceivable—although perhaps not fully established—that certain diseased states in existence may determine a sound bearing a resemblance, more or less exact, to the normal diastolic sound of the heart.

These few observations I have made by way of preface to the statement of great practical value which I now wish to consider,—That an accentuated condition of the heart's second sound is heard in connexion with one or other of two conditions of disease,—aneurism of the aorta, or dilatation of the aorta. It is hardly necessary to say, that no account is taken here of the by no means uncommon phenomenon of an accentuated pulmonary second sound. All careful auscultators know of how much value that phenomenon is in relation to the condition of mitral valve constriction. In describing the heart's second sound as accentuated in the instances of aortic aneurism and aortic dilatation upon which the present observations are based, it is perhaps necessary to explain that the expressions, intensified, or greatly pronounced, would equally well indicate the character of the sound which has been found to exist. When occurring under the circumstances referred to, the accentuation of the second sound is always well marked. I have frequently observed that early auscultators have, unaided, noticed the peculiarity, while very rarely indeed has there been any difficulty attending its recognition by such, when their attention has been called to the subject. In a case of aortic aneurism under my care last summer, there existed so accentuated a second sound over the base of the heart as to arrest the notice of all who examined the patient by auscultation. Several students, merely tyros in the art, readily recognised the *booming* character of the sound.

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[1] Diseases of the Heart, 3d edition, page 65.

When the accentuated second sound occurs in connexion with aortic aneurism or aortic dilatation, it may be presumed that the semilunar valves are competent. Their insufficiency and the occurrence of an accentuated second sound are inconsistent; if the former lesion be in existence, a diastolic murmur is the necessary result. The influence of valvular disease in the production of murmurs in cases of aortic aneurism is a point of the greatest importance for consideration. There may be, of course, associated mitral valve disease, or tricuspid disease, and, if so, murmurs may be thus originated; but such association is to be regarded rather in the light of an accidental coincidence, and not by any means of the same importance as the occurrence of disease of the aortic valves. Judging from cases of aneurism which have come under my own observation in hospital, I conclude that it is very common to find aortic valve insufficiency in connexion with aortic aneurism; while in such cases the diastolic murmur, usually a very loud or at least very distinct one, so characteristic of the former lesion, is the most prominent auscultatory phenomenon. The cases now referred to are very evidently not cases of valvular disease in the first instance, and subsequently of aneurism; in none has there been any foregoing attack of rheumatism, in none any distinct rheumatic history. Neither are they examples of a mere accidental association. The relation of the valvular imperfection to the aneurism is, I believe, of the greatest interest and importance, and in all, its occurrence has been subsequent to the disease of the vessel. If an aortic aneurism attain to any considerable dimensions, and affect the ascending portion of the arch, the aortic valves are rendered incompetent; and being so we shall find the auscultatory phenomena connected with the latter lesion in existence, and likewise the other physical signs which afford such evidence, especially the well-known peculiarities in the pulses, as pointed out by Dr Corrigan and Dr Henderson. I never remember to have seen an instance of aneurism of the aortic arch within the pericardium of any considerable size unattended by diastolic murmur,—the diastolic murmur of aortic valve insufficiency. On the other hand, the prominent physical signs of aortic insufficiency have led me—and no doubt the same error has been committed by others—to overlook the existence of aneurism altogether. Dr Haldane has in his possession a preparation of a large aortic aneurism, removed from a patient who had been under my own observation, as well as at different times under the care of three hospital physicians. In this case the signs of insufficiency of the semilunar valves were of unusual distinctness, and so, during the patient's life, the existence of that lesion was recognised, while the aneurism escaped detection till after death. The fatal event occurred suddenly, not from rupture of the aneurism, but after the mode in which a very sudden termination not unfrequently takes place in cases of aortic insufficiency. With these facts before us, how necessary is it to make a careful use of the other means of diagnosis, in addition to auscultation, which we possess.

There is no diseased condition within the chest which gives rise to so many and different auscultatory signs as aneurism. I have no intention at present of making any detailed reference to these. My remarks will be limited now to one peculiarity,—the *accentuated* second sound. Here I purposely avoid making any mention of the systolic cardiac sound. Of course, in all cases, it is of importance to determine its true state, whether pure, or itself accentuated, or attended by murmur; any of these it may be, while the accentuated character of the second sound prevails. Now, as the result of careful observation and continued attention, I have found that, excluding the accentuated pulmonary second sound, and the intensified aortic second sound in some cases of hypertrophy and dilatation of the left ventricle, the accentuated second sound in the aorta is an indication of aortic aneurism, or of dilatation of the aorta associated with atheromatous degeneration. If it be the former, the aneurism probably does not arise within the pericardium, and probably does not affect the ascending portion of the arch, but has most likely its seat in the transverse portion; it may, however, arise at an earlier part of the aorta, as was found in the following case:—

### *Aneurism of the Aorta, pointing externally, bursting through the Lung into the Left Pleura.*

S. M., æt. 36, under my care in the Infirmary, Ward V., during August and September 1862. Between the second and third left ribs, near their cartilages, a pulsating tumour was detected on the patient's admission. On auscultation a soft bruit was audible over the tumour; and at the base of heart, as well as over the upper bone of sternum, a very loud ringing second sound. The latter phenomenon never varied during the patient's six weeks' residence in hospital. He died suddenly, after expectorating a little blood. On examination of the body after death, serous fluid and coagulated blood, to the amount of more than half a gallon, were found in the cavity of the left pleura; the heart was pushed downwards and backwards; it weighed fourteen ounces. *The valves were perfectly healthy.* An aneurism was found commencing abruptly an inch and a half above the semilunar valves,—the whole vessel suddenly dilating to a point immediately beyond the origin of the left carotid, where the dilatation as suddenly ceased. The pouch so formed was six inches in length; it passed behind and was applied to the back of the manubrium sterni, and made its appearance externally between the second and third left ribs. The left extremity of the sac was intimately united to the left lung, the edge of which had become thinned by pressure, and the pleura having then given way, allowed the escape of the aneurismal contents into the pleural sac.[1]

In the foregoing case the peculiarity of the second sound was of comparatively little value in leading to the recognition of the aneurism, other and still more distinctive signs, especially the visible pulsating tumour, of that condition being in existence; but the accentuated sound led to the diagnosis of the competency of the semilunar valves, which post-mortem examination confirmed. In the following case the accentuated second sound was the earliest noted reliable sign of aortic aneurism.

W. M'A., æt. 35, a hawker, was first seen by me in March 1862, complaining of slight chest symptoms, particularly cough and expectoration of a little phlegm. Had not been a sober man.

*Condition on first examination.*—Has a slight bronchitic affection. Heart's second sound markedly accentuated over the aortic valves. No other auscultatory phenomenon connected with heart or great vessels.

I had frequent opportunities of seeing and examining this man up to November 6th, when he entered the Infirmary, becoming a patient in Ward IV. During this time his general health had failed considerably; he had become thinner, feebler, less able for his occupation, though still moving about and doing something as a traveller.

On 6th November the following notes of his condition were made. Has been suffering from dyspnœa, which has seized him on a few occasions suddenly, and without any previous effort or exertion having been made. Cough is somewhat clanging in character. Has some pain and peculiar sense of weight in region of sternum. Over the left portion of manubrium there is visible pulsation,—the latter readily distinguished on pulsation. Left radial pulse is feebler than right. Murmur of soft blowing character accompanies first sound over the seat of pulsation, and is heard less distinctly over the base of heart. The second sound at base is of a loud *booming* character. Respiratory sounds in upper part of left lung, feeble. Posteriorly there is a little bronchial stridor.

This man, so far as I know, survives: he left the Infirmary about eight weeks since. With such signs as those detailed, the existence of aneurism becomes unquestionable, they have become gradually developed in succession to the accentuated second sound, the earliest noticed of all.

Of this kind I might furnish other examples, several are known to me; and the opportunity has occurred for directing the attention of students to these, in the ordinary course of clinical instruction.

I have further to remark, that a similar condition of the second cardiac sound may be caused by dilatation of the aorta, associated with more or less of atheromatous degeneration. To distinguish between the two,—in other words, to know when the accentuated second sound is due to aneurism and when to dilatation of the aorta, is not always easy. Reliance is chiefly to be placed on the associated physical signs in the former case, more particularly prominence, pulsation, extended percussion dulness, and the signs of internal pressure. If atheromatous dilatation exist, and that is the special condition, independent of aneurism, which gives rise to the accentuated second sound, there will probably be more or less pulsation in jugular fossa, atheromatous condition of superficial pulses (radials, temporal arteries, etc.) noticeable, and probably the arcus senilis.

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[1] The post-mortem examination was performed by Dr Haldane, at that time Pathologist to the Royal Infirmary, and the account given above has been abbreviated from his record of dissections.

The following points appear to me to be of importance in endeavouring to explain the mechanism of an accentuated second sound, under the circumstances now considered:—

1. The condition of the vessel, both in cases of aneurism and of dilatation with atheromatous degeneration, being such as greatly to diminish, if not to destroy, the support given to the circulation by the artery, there results an increased recoil of blood on the closing or closed valves.

2. It is possible that a morbid condition of the valvular apparatus itself heightens or intensifies the sound. The valves are not incompetent, but in such cases they are sometimes found thickened, and even presenting a hard surface at parts.

3. Something may, I conceive, be due to the increased calibre of the vessel, in connexion with the altered condition of its internal tunic, in causing the peculiarity of sound.

But in whatever way the phenomenon is to be correctly explained, there can be no doubt of its existence being entitled to very considerable value as a clinical fact. I have noticed that the accentuated second sound is most readily appreciable over the aortic valves in both conditions. In the cases of dilatation of the aorta it has, however, been more decided in character over the manubrium sterni than in aneurismal cases. I may add, that in the majority of cases observed by myself, in which the accentuated second sound has existed under the circumstances now detailed, the expression, *booming* second sound, or second sound with *ringing boom*, has best described the acoustic character of the sound itself. I have known the booming sound continue for many weeks, and in one remarkable case of aneurism lately observed (Walker, in Ward V., and afterwards in Ward IV.), for months, and thereafter become at first obscured, and ultimately entirely replaced by a loud diastolic murmur, telling plainly that the semi-lunar valves had become insufficient owing to the extension of the disease towards the heart.

# ARTICLE II.—*Report of Clinical Cases treated in the Surgical Wards of the Royal Infirmary, under the care of* MR SPENCE, *during the Session* 1861-62. By FREDERICK STEELL, M.D., late Resident Surgeon.

(*Concluded from page* 988.)

## TUMOURS.

### *Tumours of the Neck.*

1. E. F., æt. 27. Hæmatocele of the thyroid gland, of five years' standing. This tumour, which was tense and elastic as if containing fluid, although of very considerable size, caused no inconvenience to the patient, who applied for surgical interference merely to be rid of the deformity. On puncture by a trochar and canula, a quantity of bloody serum was evacuated; but as little subsidence of the swelling followed, a free incision was made into the cyst, which was found filled with clots of blood. These were turned out by the finger, and the cavity stuffed with lint. Suppuration and granulation of the cavity followed, and some thickening which remained yielded to blistering and painting with iodine.

2. P. C. Loose, circumscribed, fibro-cystic tumour, about the size of a walnut, and situated over the sterno-mastoid muscle. Excised. Cured.

3. R. J. Fibrous tumour over the parotid gland, about the size of a small orange. Excised. Cured.

4. M. M. Adenoid tumour, occupying the whole of the digastric space. Excised. Cured.

5. J. C. Fatty tumour, of six years' growth, about the size of a goose-egg, situated between the shoulders, over the spines of the vertebræ, and closely adherent to the skin. Attended with considerable uneasiness. Excised. Cured.

6. A. J., æt. 11 years, came to the hospital on the 10th of June, with a tense elastic tumour occupying the supra-sternal hollow at the root of the neck, and extending upwards to the isthmus of the thyroid gland. This, first noticed about two months before, when it was of small size and caused little uneasiness, had increased considerably of late, and was now materially interfering with respiration, from its pressure on the trachea. The voice was considerably affected, the respiration was stridulous, and at intervals there occurred paroxysms of dyspnœa threatening suffocation. After two days' observation, as the patient's symptoms became more severe, Mr Spence made a longitudinal incision through the integuments in the middle line of the neck, and carefully dissected down to the tumour, which proved to be an abscess in the midst of glandular tissue, extending downwards and backwards behind the manubrium. Evacuation of the matter gave immediate relief to the patient, who left the hospital well a few days after.

7. J. D. Cystic tumour over spine of scapula. This tumour, present for three years, had suppurated and burst, shortly before admission. Nitrate of silver was freely applied to the remainder of the cyst, and poultices applied to the sore. After the cyst had suppurated out, several carious portions of the spine of the scapula required removal by the gouge.

### *Breast and Axilla.*

1. C. M., æt. 62. Scirrhus of the mamma, of a year's growth. No glandular enlargement. Excision. Died.

2. J. B., æt. 60. Scirrhus of mamma; first noticed five months before admission. No glandular enlargement. Excision. Recovered.

3. M. H., æt. 50. Scirrhus of mamma; first noticed six months before admission. No glandular enlargement. Excision. Recovered.

4. J. D., æt. 52. Scirrhus of mamma, of four months' duration; rapidly enlarging. Excision. Died.

5. J. G., æt. 61. Scirrhus of mamma. Evidence of a mediastinal tumour. No operation.

6. M. B., æt. 32. Scirrhus of mamma. Extremely cachectic appearance; slight glandular enlargement. No operation.

7. E. W., æt. 36. Fibro-cystic tumour of mamma, of four years' growth; size of small orange. Tumour excised. Recovered.

8. A. D., æt. 18. Fibrous tumour of mamma. Tumour excised. Recovered.

9. J. S., æt. 28. Adenoid tumour, about the size of a hen's egg, in anterior wall of the axilla, and partially covered by the great pectoral muscle; present for two years. Tumour excised. Recovered.

10. M. C., æt. 21. Cystic tumour of mamma; first noticed twelve months before admission. Tumour excised. Recovered.

11. M. D. Flat, circumscribed, fibrous tumour, about two inches in diameter, and loose in the cellular tissue of axilla; of eight years' growth, and giving rise to great pain. Tumour excised. Recovered.

### *Epithelial Growths.*

1. A. K. Epithelioma of the lower lip. Excised. Cured.

2. J. J. Epithelioma of lower lip. Excised. Cured.

3. G. M. Epithelial cancer of the tongue. Copious hæmorrhage; stopped by the application of a solution of perchloride of iron. No operation.

4. D. M. Epithelial growth on the side of the nose, of three months' growth. Tumour freely excised. Recovered.

5. J. S. Large epithelial growth of the lip and cheek. Cervical glands extensively affected. No operation.

6. F. M. Extensive malignant growth of lower lip; unfit for operation.

7. P. M. Cancer of the penis. Amputation. Recovered.

Several other individuals affected with epithelial growths were treated as out-patients.

## *Erectile Tumours.*

There were many erectile tumours, most of which were treated as out-cases, and all successfully. The case of the child W. I., which required amputation of the forearm, has been mentioned among the secondary amputations.

## *Tumours of Bone.*

1. J. M., æt. 24. Pedunculated exostosis immediately above the internal condyle of the femur, of four years' growth. For the last eight months it had given rise to considerable pain, especially after active exercise. The tumour was exposed by a free incision, and removed by the bone-forceps; and the resulting wound was allowed to heal from the bottom by granulation. Recovered.
2. J. C., æt. 17. Fibro-vascular tumour, about the size of a walnut, closely adherent to the outer third of the orbital plate of the frontal bone. Exposed by a free longitudinal incision and removed. Cured.
3. D. B., æt. 13. Medullary tumour of superior maxilla, causing great protrusion of the eyeball to the outer side. Left nostril completely occluded; tumour to be felt at posterior nares; repeated hæmorrhages. Nothing done.
4. J. M. Exostosis on outer side of the head of the tibia, causing considerable uneasiness. Excised. Cured.

*Remarks.*—The cases of tumour of the mamma afford instances of the principal forms of morbid growth occurring in that organ, with the exception of the medullary sarcoma, which is rare. Of the ten cases included in the report, four were cases of simple tumour, and of these there was one fibrous, one adenoid, one cystic, and one fibro-cystic; the remaining six cases were scirrhous. The former presented no peculiarity worthy of notice. In two of the latter, the great enlargement of the glands in the axilla, and the markedly cachectic appearance presented by the patients, showed too plainly that the disease had so far developed itself as seriously to affect the general system; and, accordingly, it was not considered advisable to hazard the risks of operation against an amelioration of condition which at best would be very temporary. In all the cases of scirrhus, the patients, with one exception, were above fifty years of age, the tumours had grown rapidly, and the constitution was more or less seriously impaired. These peculiarities are characteristic of malignant growths, and, in the absence of positive signs, may materially aid us in diagnosis. For example, in the case of M. C., there was a hard tumour deeply embedded in the glandular substance, which was not circumscribed, and which certainly did not present unequivocal signs of a cyst. But the tumour was of long standing, and it was the source of little or no uneasiness to the patient, who was young, fresh-looking, and in the enjoyment of excellent health. Here, these considerations strengthened the likelihood of its being cystic, by rendering the idea of scirrhus highly improbable. When punctured by a trochar, a little lardaceous-looking matter escaped from the canula, and this settled the point.

The epithelial growths were of the ordinary kind, and, except the case of G. M., which illustrates the risks of hæmorrhage and the excellence of perchloride of iron as a styptic, none call for special remark.

The most interesting of the cases of erectile tumour, that of the child W. I., has been detailed among the amputations. The numerous other cases were treated by ligature, the seton in the form of a thread saturated with perchloride of iron, and the injection of a solution of perchloride of iron, according to circumstances, such as the size, site, prominence, and circumscription of the tumour.

Of the three cases in which the thyroid gland was involved, and which alone call for remark among the tumours occurring in the cervical region, one, namely, the case of hydrocele, has been recorded under the head of tracheotomy.

E. F.'s case is an instance of hæmatocele in an unusual situation. The tumour closely resembled a hydrocele, and was treated like a hæmatocele in its ordinary site. In A. I.'s case the abscess seemed to be situated in the glandular texture connecting the thymus and thyroid, and this uncommon seat, together with the length of time the tumour had existed, rendered the diagnosis somewhat obscure. The patients operated on for exostosis were both young men. The tumours, which occupied their usual site, were distinctly pedunculated, and their removal, which was readily effected, was not followed by any of the bad consequences which not uncommonly attend such operations.

There were two cases of cystic tumour of bone, occurring in outpatients, one of the upper and the other of the lower jaw, both of which were successfully treated by removal of a portion of the wall of the cyst. There were also numerous instances of cystic tumour of the scalp and eyelids, one large wen of sixty years' growth being successfully removed from the scalp of an old woman at the advanced age of eighty-two.

## Genito-Urinary Cases.

### *Lithotomy.*

1. W. B., æt. 55. Stone in the bladder, removed by ordinary lateral operation. Recovered.
2. J. D., æt. 52. Stone in the bladder. Lateral operation. Recovered.
3. W. G., æt. 6. Stone in the bladder. Lateral operation. Recovered.

### *Stricture.*

1. M. M. Stricture of the urethra, with perineal abscess. Abscess freely incised. Cured by dilatation.

2. S. G., æt. 45, who had suffered from stricture for several years, was admitted labouring under complete retention of urine, and in a state of violent delirium. A large abscess, found in the perineum, was at once freely opened, and an attempt made to pass a small-sized catheter into the bladder, but unsuccessfully. After considerable difficulty, the smallest sized grooved instrument was passed through the stricture into the bladder, and the stricture being divided from without, a catheter was passed and retained for two days. After the operation the patient progressed favourably, and was dismissed cured.

3. J. C., æt. 28. For the last ten months this patient had suffered from a tight stricture, and on admission the difficulty of micturition was so great that the urine passed off merely in drops. The smallest sized steel bougie was the only instrument that could then be passed, and even at the end of several weeks the stricture resisted the passage of any larger instrument. Accordingly, the stricture was now divided, by cutting from without on the smallest sized grooved staff, and a full-sized gum-elastic catheter, passed through the penis into the bladder, was retained for two days. Patient made a rapid recovery, a full-sized instrument being occasionally passed; and he was dismissed cured.

4. R. J., æt. 58. Patient stated that a stricture, from which he had suffered for twenty years, had been several times successfully treated by dilatation, but had always recurred. On examination, an abscess with several fistulous openings was found in the perineum. After a grooved staff had been passed into the bladder, a free incision was made into the abscess, and the stricture divided at the same time. As usual, a full-sized gum-elastic catheter was passed and retained for a few days. Cured.

5. W. P., æt. 53, admitted with extreme difficulty of micturition, caused by a stricture which had yielded to dilatation two years previously, but had recurred from the patient's neglecting the occasional passage of instruments. Examination of the perineum likewise disclosed a hard painful swelling, evidently threatening abscess. This was freely incised, and the stricture at the same time divided by the ordinary method. Patient made an excellent recovery.

6. J. G. was admitted labouring under complete retention of urine, and likewise complaining of severe pain in the perineum. As the smallest sized grooved staff was the only instrument that could be passed, urethrotomy was immediately performed in the usual manner, and patient recovered without a single bad symptom.

### *Extravasation of Urine.*

1. J. G., æt. 44. Patient stated that, ten days before admission, after exposure to cold and wet, he experienced a severe rigor, fol-

lowed by painful and difficult micturition, which had continued ever since. On admission, there was a large sloughing abscess found in the perineum, the pulse was feeble and rapid, and his countenance had a sunken anxious expression. A free incision was made in the perineum, and a considerable quantity of fetid pus mixed with urine was evacuated, after which a gum-elastic catheter was passed into the bladder and retained. Poultices were meanwhile applied to remove the sloughs, and stimulants freely administered to maintain the strength of the patient. The wound gradually contracted and healed, and the patient left the hospital with his general health much improved.

2. J. M., æt. 13. Six days previous to his admission, this patient, while attempting to walk along an iron railing, fell astride it and severely injured his perineum. Although he complained of great pain immediately after the accident, he was not only able to walk home, but passed water freely four hours afterwards. Next morning, however, he was unable to do so, and the urine then drawn off by a catheter was observed to be bloody. Up to the morning of the 21st, when he suddenly felt his water running down his legs, he did not again complain of difficulty of micturition or pain in the perineum. On admission, on the evening of the same day, the perineum and posterior part of the scrotum were found to be in a sloughing condition, with several fistulous openings. This condition was attended by very little constitutional disturbance. The openings in the perineum having been enlarged, a gum-elastic catheter was, with some difficulty, passed into the bladder, and poultices applied to the perineum. During the cicatrization of the wound, bougies were occasionally passed to prevent the formation of stricture at the ruptured portion. Cured.

## DISEASES OF THE TESTICLE.

1. D. J., æt. 27. Scrofulous abscess of testicle, followed by several sinuses. Cured by freely laying open the sinuses, and the administration of cod-liver oil and the iodide of potassium.

2. W. T. Hæmatocele. Cured by a free incision into the cavity, and the removal of a portion of the cyst.

3. W. B. Syphilitic sarcocele. Rest, strapping, and the iodide of potassium. Cured.

4. W. K. Syphilitic fungous testicle. Cured by paring the edges of the scrotal sore, dissecting back the integuments, and securing them by silver suture.

5. P. D. Fungus of the testicle. Treated successfully by strapping and the application of sulphate of copper to the sore.

6. D. R. Subacute cystitis, following a retrocedent gonorrhœa, which had suddenly ceased after an injection of a strong solution of nitrate of silver. Fatal termination by peritonitis, following rupture of the bladder, caused by ulceration of its coats.

7. R. P., æt. 3. Admitted with complete retention of urine, having had considerable pain and difficulty of micturition for some time previously. A catheter was with some difficulty introduced, and on the urine being drawn off, it was found to contain a large quantity of pus and blood. At the same time it was observed that the prostate was of very large size, and of dense consistence. Ordered syrup of the iodide of iron; urine to be drawn off night and morning. Dismissed in *statu quo*, at request of friends.

### *Cases requiring Castration.*

1. M. T., æt. 9. Scrofulous testicle in a strumous-looking boy, first noticed eight months before admission. On examination, there was found a tumour of the left testicle of an oval shape, and about the size of an orange, hard at the upper part, but soft and fluctuating at its base, having the overlying integuments tense and red, but giving rise to little or no pain. An incision made into its base evacuated a considerable quantity of curdy scrofulous pus. Cod-liver oil and vinum ferri, with nourishing diet, were now administered. After a time, as the general health of the patient was fast failing, the tumour was removed, along with an elliptical portion of the integument over it. The general health of the patient now rapidly improved, and he was dismissed cured.

2. R. L. Six months before admission, patient had his left testicle crushed between two wooden beams, and extensive suppuration followed the injury. There were now several sinuses running through the gland in all directions, and discharging copiously. These were first freely laid open, and the patient ordered tonics and nourishing diet. New abscesses, however, continued to form, and as the extensive suppuration had almost entirely destroyed the gland, while the patient's constitution was evidently suffering from the profuse discharge and long confinement, it was found necessary to have recourse to castration. After the operation patient made a rapid recovery.

3. J. S., æt. 29. Medullary tumour of testicle. Six months ago, patient first noticed a swelling of the left testicle, accompanied by a sense of weight and lancinating pain along the course of the spermatic cord, and this he attributed to a crush received shortly before. Since that time the tumour has steadily increased in bulk, and latterly has given rise to intense pain, especially at night. On admission, there was found a large ovoid tumour, about the size of a small coco-nut; heavy, solid, and elastic to the touch, smooth on the surface, except at the base, where it was slightly nodulated, and having the integuments covering it tense and of a somewhat darker hue than natural, with great enlargement of the veins. Slight icteric hue of countenance, but general health apparently good; no glandular enlargement. Tumour removed by two semi-lunar incisions, including a considerable portion of skin. Cured.

*Remarks.*—Three cases for lithotomy presented themselves during the session, and in all the ordinary lateral operation was successfully performed. In the case first mentioned in the report, the stone was composed of two masses, one the size of a field-bean, and the other the size of a walnut, the former being connected with the latter by an elongated slender pedicle, so as to constitute an irregularly dumb-bell form. The smaller of these masses had probably been originally formed in the prostate, as it is difficult otherwise to account for the peculiar shape of the stone. In J. D.'s case, the progress of the deep incision was arrested by the prostatic calculus, the removal of which necessitated the withdrawal of the staff; and the necessary division of the prostate was completed by passing the finger into the wound, and carefully enlarging the incision with a probe-pointed bistoury guided along the point of the finger.

Besides the case of M. M., a large number of cases of stricture were successfully treated by dilatation, and the occasional retention of a catheter in the bladder for twenty-four or forty-eight hours in some of the more obstinate. The five cases mentioned in the report in which urethrotomy was performed, are examples of the admirable results of that operation in properly selected cases.

J. G. is an example of a somewhat rare class of cases occurring in debilitated subjects in which the primary mischief appears to be acute inflammation of the cellular tissue of the pelvis, which rapidly goes on to sloughing of all the textures of the perineum, and in which the urethra is sometimes secondarily affected. In such circumstances the local mischief is attended by severe irritative fever. In evidence of there being no primary affection of the urethra, it should have been stated that the patient had not the slightest difficulty in making water prior to the rigor, and that immediately after the abscess was opened, a full-sized catheter could be passed into the bladder.

The case of the boy J. M. is an ordinary instance of extravasation of urine following upon ruptured urethra, and is remarkable for the absence of severe constitutional symptoms which almost invariably characterize these cases.

## TABULAR VIEW OF FRACTURES.

### *Upper Jaw.*

T. A., admitted July 8. Cured July 25.
T. R., admitted Sept. 19. Cured Oct. 18.
J. H., admitted Oct. 13. Cured Nov. 16.

### *Lower Jaw.*

J. W., admitted March 25. Cured May 12.
J. R., admitted April 4. Died April 13.

### *Pelvis.*

D. M., admitted May 20. Cured July 8.
J. S., admitted June 7. Cured June 27.
A. J., admitted Sept. 16. Died Oct. 15. (Compound.)

### *Femur.*

D. C., admitted Oct. 22. Cured Jan. 15.
J. J., admitted Nov. 12. Cured Jan. 6.
A. L., admitted Dec. 8. Cured Jan. 31.
P. C., admitted March 4. Cured.
R. F., admitted March 31. Died April 18. Age of patient, 82. (Fracture of neck.)
J. R., admitted April 8. Cured March 18.
M. M., admitted April 10. Cured June 5. (Double fracture.)
J. L., admitted May 8. Dismissed May 30.
G. B., admitted May 12. Cured June 28.
J. W., admitted July 22. Cured Sept. 8.
J. C., admitted Sept. 13. Cured Nov. 7.

### *Femur—Compound.*

W. S., admitted Jan. 23. Cured May 17.
S. C., admitted July 15. Died Sept. 18. (Secondary amputation.)

### *Both Bones of Leg—Simple.*

E. J., admitted Jan. 25. Cured April 6.
A. E., admitted April 11. Cured May 13.
C. B., admitted April 29. Cured May 11.
J. H., admitted April 30. Cured May 29.
W. M., admitted May 10. Cured June 8.
W. R., admitted May 27. Cured July 8.
M. B., admitted June 5. Cured July 14.
J. M., admitted July 5. Cured July 28.
J. M., admitted Aug. 9. Cured Sept. 18.
A. R., admitted Sept. 13. (Both legs.)
M. C., admitted Oct. 25. Cured Dec. 1.

### *Both Bones of Leg—Comminuted.*

J. B., admitted Nov. 9. Cured Dec. 19.
R. W., admitted Nov. 12. Cured Dec. 24.
A. M., admitted Nov. 23. Cured Jan. 8.
J. D., admitted Dec. 30. Cured Feb. 10.
P. M., admitted Sept. 15. Cured Oct. 31.
J. M., admitted Oct. 1. Died Nov. 14.
A. E., admitted Oct. 13. Cured Dec. 10.

### *Both Bones of Leg—Compound.*

G. B., admitted Jan. 4. Cured March 6.
T. M., admitted March 20. Died April 1. (Amputation.)
J. N., admitted June 5. Died June 9. (Amputation.)
C. A., admitted July 13. Died July 29. (Amputation.)
R. B., admitted Sept. 9. Died Sept. 10. (Amputation.)
M. F., admitted Oct. 7. Cured Nov. 25.
M. C., admitted Oct. 13. Cured Jan. 24. (Amputation.)

### *Tibia—Simple.*

R. H., admitted Dec. 17. Cured Jan. 9.
A. M., admitted Jan. 22. Cured March 3.
J. S., admitted Jan. 30. Cured March 6.
A. M., admitted March 15. Cured April 4.

### *Fibula—Simple.*

J. M., admitted Nov. 15. Cured 27.
P. L., admitted Nov. 12. Cured Dec. 5.
J. Y., admitted Dec. 20. Cured Jan. 21.
W. H., admitted Jan. 20. Cured March 17.

J. C., admitted Feb. 27. Cured March 17.
J. M., admitted April 25. Cured May 7. (Dislocation of foot backwards.)
J. S., admitted June 10. Cured July 1.

*Foot—Compound.*

R. S., admitted May 15. Cured Aug. 2. (Amputation.)
J. B., admitted April 8. Died April 23. (Amputation.)
H. Y., admitted June 26. Cured Aug. 31. (Amputation.)
T. S., admitted June 9. Cured Aug. 7. (Amputation.)
T. W., admitted Oct. 17. Cured Dec. 15. (Amputation.)

*Ribs.*

J. L., admitted Jan. 15. Cured March 27.
M. F., admitted April 29. Cured May 16.
E. N., admitted May 4. Cured May 6.
J. L., admitted June 6. Cured July 8.
G. J., admitted Aug. 10. Cured Sept. 1.
J. H., admitted Sept. 13. Died Sept. 11.

*Scapula.*

R. M., admitted Jan. 21. Died Feb. 5.
P. G., admitted April 16. Cured April 28.

*Humerus—Compound.*

B. R., admitted July 22. Cured Oct. 10.

*Humerus—Simple.*

P. M., admitted Oct. 31. (Fracture of external condyle and dislocation of radius.)
J. J., admitted Nov. 12. Cured Jan. 6. (Comminuted.)
W. E., admitted Nov. 12. Cured Dec. 4.
M. B., admitted Jan. 22. Cured Jan. 23.
J. A., admitted April 10. Cured April 21. (Dislocation.)

*Note.*—Besides these cases of fracture which were treated in the hospital, there were numerous cases of fractures and dislocations of the upper extremity treated at the hospital as out-patients, which furnished examples of almost every form of these injuries for clinical instruction.

*Remarks.*—As the various plans of treatment adopted in the different forms of fracture have been explained by Mr Spence in former reports, we shall here merely give one or two of the more interesting cases somewhat in detail. The first case we shall select is that of R. M., a man about forty years of age, who applied for advice at the Royal Infirmary on the 17th of January, on account of an injury of the shoulder caused by his falling when in a state of intoxication.

On placing the hand upon the injured shoulder, before removal of his dress, there was no feeling of flattening, but on raising the arm slightly to take off his shirt to examine him more fully, the limb fell towards the axilla, causing a great projection of the acromion and flattening of the deltoid, with all the symptoms of luxation of the humerus downwards in an extreme degree, but attended with the distinct crepitus of fracture. By extending and drawing the arm out from the side and then raising the whole limb, the natural contour of the shoulder was restored, crepitus again being heard as the broken surfaces came into contact. When the support was even slightly removed from the elbow, the limb had the tendency to fall and be again displaced. Under these circumstances, Mr Spence stated to the students present that the case was undoubtedly an example of that rare form of injury, fracture through the neck of the scapula, the separated glenoid cavity and the humerus being displaced together into the axilla, partly by the weight of the arm, and partly by the great pectoral muscle, latissimus dorsi, and teres major, drawing the arm towards the chest.

A pad was placed in the axilla, and the arm, slung by the elbow, bandaged to the side; and as the man had also received an injury over the orbit, laying bare the bone, he was strongly advised to remain in hospital. This, however, he refused to do, but promised to return next day. Nothing more was heard of him till the 20th of January, when he returned and agreed to remain in hospital. During the interval which had elapsed he had been drinking, but the bandages had not been much disturbed. The apparatus was readjusted, the patient kept in bed, and warm-water dressing was applied to the wound over the orbit, which had begun to suppurate. Everything seemed to go on well for about ten days; he suffered from no feverishness, and complained of no pain till the evening of the first of February, when he said his eye felt painful. Warm-water dressing was applied over the eye and the orbital wound, and a purgative was given. Next morning, he suddenly became insensible; the wound looked dry, and its edges were erysipelatous. The erysipelas spread rapidly, and the semicomatose state changed to violent delirium, under which the patient sank. The cause of death was meningitis, resulting from the injury of the frontal bone; and a suppurative clot was found blocking up the longitudinal sinus. This unfortunate result gave an opportunity of examining and determining the exact nature of the injury of the shoulder. The fracture was found to pass obliquely from below, upwards and forwards, commencing about half an inch behind the origin of the long head of the triceps, and separating the neck and four-fifths of the lower part of the glenoid cavity from the scapula. The long head of the biceps and the whole of the glenoid ligament had also been torn from the upper remaining fragment of the glenoid cavity, and carried along with the displaced portion. Fracture of the neck of the scapula is admitted by all to be a very rare accident, and by many the possibility of its occurrence has been doubted. Mr Pirrie mentions three cases where the symptoms were apparently well marked; Sir Astley Cooper also describes a case in which he was consulted; but, on the other hand, Mr South, in his translation of Chelius's Surgery, states that there is no preparation illustrative of such an injury in any museum in London. One dissection of this injury by Duverney is, however, mentioned in Cooper's Surgical Dictionary.

The principal value of this case consists in the fact, that, by its fatal issue, a proof of the existence of such a fracture was established, and an opportunity was afforded for examining its exact anatomical nature, and the connexion between it and the symptoms observed shortly after the occurrence of the accident. It should be noticed that the fracture did not pass from the scapular notch directly through the neck separating the glenoid cavity and coracoid process from the body of the scapula, as described by Sir A. Cooper, but obliquely through the neck, detaching all but one-fifth of the glenoid cavity, the whole of the glenoid ligament, and the long tendon of the biceps, so that the whole of the articular structures were detached from the scapula, and were readily dragged downwards with the broken fragment by the weight of the limb and muscular displacing causes.

As another instance of an uncommon injury may be mentioned the case of J. A., in which there was fracture of the neck of the humerus, and dislocation of the separated head of the bone downwards into the axilla. The patient, who was fifty-three years of age, had, while cleaning a stair, slipped her foot, and, after falling with her whole weight on the right shoulder, rolled down the stair. She came to the hospital immediately after the accident, and then the right shoulder presented the usual appearances of dislocation downwards into the axilla. On further examination, however, there was found to be a fracture of the neck of the humerus, and on placing the hand in the axilla, the head of the bone could be felt lying loose, and not moving along with the shaft in rotation. The patient was placed under chloroform, and numerous unsuccessful efforts were made at reduction, but it was not until all attempts were about to be given up that the head of the bone was reduced, and then apparently spontaneously. A pad was placed in the axilla, and the ordinary retentive apparatus for fracture of the neck of the humerus applied. As it is scarcely conceivable that both injuries could have occurred at the same time, it is probable that she dislocated her shoulder by the fall, and subsequently fractured her humerus while rolling down the stair.

A case of dislocation of the acromial end of the clavicle, occurring in an out-patient, also deserves mention as a somewhat rare injury.

## HERNIA.

1. J. E. Strangulated inguinal hernia; nineteen hours down: reduced by taxis.

2. J. R. Strangulated inguinal hernia; reduced by taxis.

3. M. M., æt. 37. Patient had been troubled with a femoral hernia for twelve months, which, however, had been always easily reduced by herself. On the 12th of October, patient, however, found that she could not reduce the hernia, which had come down after a violent fit of coughing. Towards evening she began to suffer from nausea, and severe vomiting occurred during the night,

while tumour became very painful. Next morning, she was seen by Mr Spence, who, failing to reduce the hernia by the taxis, while she was under chloroform, at once performed the usual operation. The stricture was found to be very tight, and the bowel dark and congested. Patient recovered without a single bad symptom.

4. C. F. Femoral hernia, four days' strangulated. In addition to the ordinary signs of strangulated hernia, patient on admission was likewise suffering from peritonitis, as evinced by great anxiety of the countenance, intense abdominal tenderness, and a small, rapid, wiry pulse. Operation. The bowel, being gangrenous, was not returned. Calomel and opium were administered, and a blister applied to the abdomen, but the patient died on the third day from general peritonitis, as shown by post-mortem examination.

5. W. B., æt. 15. Strangulated inguinal hernia. Strangulated eight hours. Taxis failing, operation performed. Cured.

## MISCELLANEOUS CASES.

1. F. Y., admitted with general emphysema to an intense degree, and a wound passing obliquely downwards and to the left side, from the lower edge of the thyroid cartilage, from which bubbles of air escaped at each respiration. Patient had stabbed herself with a penknife during a fit of religious mania. When the wound had been extended to about two inches, it was found to have punctured the right side of the larynx, between the thyroid and cricoid cartilages. Several incisions were made into the cellular tissue of the neck and upper part of the trunk, and opiates were administered to soothe the patient who was very noisy and restless. The wound was allowed to heal from the bottom, and the emphysema gradually disappeared; but, as the maniacal condition of the patient continued, she was sent to an asylum.

2. I. P., æt. 28. Admitted with a large pendulous tumour about the size and shape of an ostrich's egg, soft, and evidently full of fluid, and situated in front of the right knee, the skin covering it being of a natural colour. Blistering and painting with iodine having failed to benefit the patient, the bursa was laid open, and found to contain a large collection of fibro-cartilaginous bodies about the size of millet-seeds, with imperfect septa passing across the cavity. The interior of the cyst was painted with iodine, and poultices were applied to promote suppuration of its walls. Patient dismissed cured.

A. N., æt. 36, was admitted on July the 8th, with an aneurism occupying the lower part of the popliteal space, which, judging from the patient's statements, had probably existed for four months. The patient also suffered from extensive cardiac disease, having a double aortic and a mitral regurgitant murmur, with hypertrophy, especially of the left ventricle. There was general visibility of the pulses, and the radials were slightly corded. Treatment by flexion was adopted, the limb being gently flexed both at the knee and hip-joints, and placed on its outer side on an air pillow. This position was maintained by a strap stretching from the heel of a slipper to a figure-of-eight bandage passed round the thigh and abdomen; while perfect rest was enjoined, with a nourishing but non-stimulant diet. A certain degree of pain which at first attended this treatment soon passed off, and the case seemed to be progressing most favourably, as the tumour had become much smaller and harder, and the articular arteries were distinctly enlarged around the knee. Suddenly, however, on the morning of the 18th of August, the patient experienced a sensation as if something had given way behind the knee-joint, and immediately complained of intense pain in the limb. On examination, it was evident that the aneurism had ruptured, and accordingly manual pressure was applied over the femoral artery to prevent further extravasation of blood. After consultation, it was determined to ligature the femoral artery, which was accordingly done, the limb being wrapped up in wadding immediately after the operation. On the 16th of August, being the seventh day after the rupture of the aneurism and ligature of the femoral, gangrene of the limb having set in, amputation at the lower third of the thigh was performed by the long anterior flap. During the operation unusually little hæmorrhage occurred, and the tissues were observed to be infiltrated with serum. For two days the state of the patient continued satisfactory, but he suddenly expired on the 19th. The propriety of ligaturing the femoral artery may be questioned, but by arresting the circulation, it afforded a reasonable hope that the rent in the sac might have become occluded, and the blood extravasated into the tissues, which was not very large in amount, absorbed, while even if gangrene did supervene, amputation could have been performed before the attending constitutional symptoms had developed themselves.

A case of axillary and abdominal aneurism also occurred, which proved fatal two days after admission, by rupture of the abdominal tumour into the lung.

## ARTICLE III.—*On the Relative Weight of the Viscera on the Two Sides of the Body; and on the Consequent Position of the Centre of Gravity to the Right Side.* By JOHN STRUTHERS, M.D., F.R.C.S., Lecturer on Anatomy in the Edinburgh School of Medicine.

(*Read before the Edinburgh Medico-Chirurgical Society, May* 6, 1863.)

IN demonstrating the position of the liver, I have been in the habit of remarking that the great weight of that viscus on the right side, does not appear to be balanced by the viscera on the left side. The object of this paper is to show that this supposition is correct, and consequently, that the centre of gravity lies to the right side of the middle line of the body. The fact would be sufficiently interesting in relation to animal symmetry and equipoise, to visceral development, and to the mechanism of the erect posture; but it has lately assumed a new importance from the theory that a greater weight of one side of the body will determine the greater strength and use of the limbs of that side, and thus furnish a physical explanation of the use of the right hand in preference to the left.

This theory has lately been propounded by Professor Andrew Buchanan, in a most interesting and philosophical paper,[1] which does not as yet appear to have attracted the notice which it deserves. Dr Buchanan's theory is not founded on the supposition that the viscera are naturally heavier on one side than on the other, in the passive condition. On the contrary, he remarks (p. 13) that, "in a child from one to two years of age, in whom the liver has diminished in size, and assumed the relations to the other viscera that it is afterwards to retain, but in whom the locomotive system of the right side has not yet acquired a predominancy, it is probable that the two halves of the body, separated by a vertical section from before to behind, differ very little in weight from each other; and in that case the centre of gravity must be situated in the middle plane." But he supposes that the effect of the inspiratory act will be to push the liver farther to the right side, giving a temporary preponderance, and also a muscular advantage, to the right side, which he thinks will be sufficient to determine the above result.

The phenomenon to be explained is not why each individual uses one hand in preference to the other, the reason of which is evident, but why all nations and tribes of mankind yet known to us should prefer the same hand. It will be admitted that any theory which professes to explain this remarkable phenomenon—which has ceased to attract the notice of physiologists only because it has baffled satisfactory explanation—deserves examination, especially when it supposes the cause to be a physical one acting within each individual. The question which meets us at the threshold of Dr Buchanan's theory, is whether the foundation on which he has rested it is correct; and even granting a likelihood that the descent of the diaphragm will disturb a supposed equipoise of the viscera, it is in vain to expect the acceptance of a theory the starting point of which is involved in doubt. My object, therefore, is to inquire whether there are not unquestionable anatomical grounds on which the theory, if it is correct, might be rested. With this view, I have been engaged for some time back, as opportunity offered, in making a series of experiments and special examinations of the viscera.

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[1] Mechanical Theory of the Predominance of the Right Hand over the Left, or, more generally, of the Limbs of the Right Side over those of the Left Side of the Body. By Andrew Buchanan, M.D., Professor of Physiology in the University of Glasgow. 1862.

In proceeding to ascertain the facts, it was necessary, *First*, to determine the exact usual position of the several abdominal and thoracic viscera in relation to the middle line of the body. *Second*, to determine the relative weight of the parts of those viscera which lie on either side of the middle line, as well as to consider the absolute weight of each viscus. In regard to the absolute weight of each viscus as a whole, abundant data are supplied by the well-known observations of John Reid, Sims, Clendinning, Hutchinson, Peacock, and others. The results of the very numerous weighings of the various viscera, by these authors, will be found fully given in the last edition of Quain's "Elements of Anatomy." The correctness of the averages which I shall take will be seen by referring to the results established by these authors. I shall take the average weight (avoirdupois) as presented in the male, as the proportions in one sex must be kept to, it does not matter which. For the relative weight of the several parts of those organs which cross the middle line I shall refer to weighings of my own. Farther, as it may be supposed that the greater or less distance of an organ, or part of it, from the middle line, will increase or diminish the influence of its weight, I have taken this into consideration, and have subdivided each side into an outer and an inner half, as not merely the most simple subdivision which could be made, but the one which, on the whole, best suits the arrangement of the viscera. These subdivisions may be termed the right external and right internal—and the left external and left internal regions.[1] This distinction may not be necessary to the correctness of the general result, and has considerably increased the labour of the inquiry, but I have attended to it in order to anticipate any suggestions of fallacy. Finally, as the question, in so far as it can bear on the cause of the preference of the right hand, must turn on the weight and position of the viscera in the child at the period when the predominance of the right hand is being gradually developed, in the second and third years and afterwards, it is necessary to make the calculation from the facts as presented in children. The result, however, we shall find, will be not to affect materially the calculations founded on the adult. The most convenient method of proceeding will be first to calculate the relative weight of the two sides in the adult, and then to make such allowance as may be necessary for the differences in the child. For this I shall found on what has already been published regarding the viscera of the child, and on the observations I have been able to make on the exact position and weight of the viscera in children.

I shall now proceed to consider each of the viscera in relation to these questions.

LIVER.—(A) *Relative Weight of the Right and Left Lobes.*—The average weight of the adult liver, in the male, may be taken at 52 ounces. The proportion of the left to the right lobe has been variously estimated as one to four, five, or six. It no doubt varies in different instances, and something may depend on the exactness with which the section has been made. My own weighings, after careful section, give the first of the above estimates as that which is nearest the truth. At a proportion of about one to four, 10 ounces would belong to the left lobe, and 42 to the right.

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[1] See the Table at the end of the Paper.

The left lobe is relatively somewhat larger in children of the age in question than in the adult. Especially in the early fœtus the liver is of enormous size, and the left lobe is described as then being equal, or nearly equal, in "size" to the right. Diminishing relatively, the proportion between the right and left lobes, just before birth, is said to be as 1 to 1·6. After birth, a rapid diminution takes place of the whole liver relatively to the body, and of the left lobe relatively to the right; so that, even at a month after birth, the proportion has been stated to be as one to three. By the fifth or sixth year the liver has been said to have reached that proportion to the body which it has in the adult. Dr Reid's third table would seem, however, to show that the liver of the child and adolescent is heavier in relation to the body than in the adult; that from the ages of from one to five years up to from twenty to thirty years, the proportion, omitting fractions, falls from 1 to 21 to 1 to 29, while the proportion between thirty and sixty years of age is 1 to 35½. This greater size in the child and adolescent applies, however, to the whole liver, the proportion between its right and left lobes being a separate question. The diminution to the proportion of 1 to 3 at a month after birth is very rapid, if correctly stated, but it is of no consequence to the present question to determine the proportion of the left to the right lobe until the age when the child generally begins to walk. The weighings which I have made in children between the ages of one and a half and three years give a proportion of between 1 to 3 and 1 to 4. Taking the proportion at 1 to 3½ for the child, but retaining the adult weights for the calculation in order to avoid confusion, this would give, say, 11½ oz. for the left, and 40½ for the right. A proportion of 1 to 3 would give 13 oz. for the left, and 39 for the right. For the calculation we are safe, therefore, in order to give full influence to any difference in the liver of the child at and after the age in question as compared with the adult, to allow 12 oz. for the left, and 40 oz. for the right lobe.

(B) *Extent to which the Left Lobe lies to the right of the Middle Line.*—The information in the books as to the exact position and retention of the liver is indefinite and not to be much relied on in this inquiry, the question of the relation of the viscera to the middle line not having, apparently, been present to the mind of any of the writers; and it has cost me much more labour to arrive at satisfactory conclusions on this subject than I anticipated. In proceeding to determine how far the division into right and left lobes corresponds to the middle line of the body, two questions present themselves:—How far the attachment of the falciform ligament above, corresponds to the line of separation between the right and left lobes by the longitudinal fissure below; and, What the true position of the falciform ligament is in relation to the middle line.

(1.) *Correspondence of the Falciform Ligament to the Longitudinal Fissure.*—As both the falciform ligament and the longitudinal fissure have relation to the umbilical vein, now converted into the round ligament, we should expect them to correspond in front, but not necessarily in their course backwards. I find that the hepatic attachment of the falciform ligament may pass straight back, or may curve a little with the concavity to the left, or may undulate slightly. Again, the longitudinal fissure may not be straight, the umbilical part and the part for the ductus venosus both inclining slightly towards the right at the portal fissure, giving a slight concavity to the left. And the fissure may be half-an-inch broad, at least when the liver is lying flat on its back, so that its exact middle must be taken. The liver substance, too, is soft and easily moulded, and care must be taken not to press it on one side more than the other. Avoiding these sources of fallacy, I find there is generally an exact correspondence between the line of the ligament and the fissure, except it may be for a very short distance at the farthest back part. This is shown by passing a series of needles vertically through from the attachment of the ligament, or from the middle line of the fissure. If not emerging precisely in the middle, they appear on alternate sides, or through some part, of the fissure; or, above, on alternate sides of a slightly wavy attachment of the ligament. Or, again, by slicing fairly along the middle of the fissure, by the sweep of a long amputating knife, the correspondence is so close, that I have seen the edge of the knife split in between the layers of the ligament. It was necessary to be certain of this correspondence before taking the falciform ligament as an index to the position of the right and left lobes, in examining the liver as it lies in the abdominal cavity.

(2.) *Position of the Falciform Ligament in relation to the Middle Line.*—The falciform ligament may have been in the middle in early fœtal life, but I find that after birth it always lies to the right side. This is the case both with the upper (diaphragmatic) part and the lower (rectal) part of the ligament, although the latter begins at the middle line at the umbilicus. It must be the familiar experience of the dissector, when he has slit up the abdominal wall through the linea alba, that the falciform ligament goes with the right flap. The hepatic attachment of the ligament is seen to be considerably to the right in any recently opened healthy abdomen. In order to meet the suggestion of fallacy in the possibility of this being partly the result of displacement from opening the cavity, and to determine the exact extent to which it departs from the middle, I adopted the plan of passing in needles in the middle line, and then opening the cavity to see where they pierced the liver or other viscera, and their distance from the ligament. I refer at present to the recumbent posture. My numerous experiments show that the ligament is to the right side in two ways,—that it is not merely to the right side at its parietal attachment but itself slopes to the right side, so that the hepatic attachment is still farther removed to the right. Opposite the fore part of the liver the parietal attachment may be ¾ inch to the right side, while the hepatic attachment may be 1½ inch or more. I have seen it 3 inches, but this is unusual. Making allowance for proportion, it is as great in the child. In a nineteen months' child the parietal attachment was ¼ to ⅓ inch, the hepatic attachment fully an inch, to the right side of the needles. In a child of two years the hepatic attachment was as much as 1¾ inch to the right side. The ligament and fissure, however, as they pass backwards may approach nearer to the middle line, and even reach quite to it at the very back. In the child of two years, in which the ligament and fissure were, midway back, 1¾ inch to the right, they were at the very back transfixed by the median needle.

We shall probably be within the truth by reckoning on an average, in the adult, and in the recumbent posture, an inch of the breadth, straight along, of the left lobe as situated on the right side.

As the portion of the left lobe next the longitudinal fissure is its thickest part, this must considerably lessen the weight of the liver on the left side; for, although the left lobe is from 3 to 4 inches in breadth, the outer half is thin and shelving, and in weight generally less than a third of the entire left lobe. To determine exactly how much should be allowed, I sliced off carefully and weighed the internal inch of the left lobe. It may be equal to a half, more generally to over a third, of the weight of the entire left lobe. A proportionate section in the child gives very nearly, if not exactly, the same result, and we are, therefore, probably within the mark in carrying one-third (4 oz.) of the weight of the left lobe to the right side. Thus, instead of 12 and 40, which the division into left and right lobes alone would give, the division by the middle line of the body gives 8 oz. of liver on the left and 44 on the right side.

*The Movements of the Liver, and the Functions of the Falciform and other Ligaments of the Liver.*—Although accounted for developmentally by the presence and necessary position of the umbilical vein, the *falciform ligament* does not the less serve as a retaining structure to the liver. Of course, this so-called suspensory ligament does not suspend the liver, which is naturally in contact with the diaphragm, but merely checks farther motion in certain directions. The following conclusions may be adopted regarding the uses of the falciform ligament:—(*a*) *Antero-posteriorly*, it checks both forward and backward motion. (1.) In the recumbent posture, farther backward motion is prevented by the lower or rectal portion, and by the round ligament within its free margin, more especially by the round ligament. (2.) In the erect posture, farther forward motion is prevented by the upper or diaphragmatic portion of the ligament, the rectal part and the round ligament being then quite loose. The different antero-posterior function of the two parts of the ligament may be easily demonstrated. The change is seen to correspond to a line drawn from the anterior notch of the liver to the ensiform cartilage. The two parts of the ligament are supplied with blood from different sources. (*b*) *Laterally*. It checks extreme lateral motion in either direction, allowing the liver to swing a certain distance from side to side. It allows free lateral motion in front, gradually diminishing backwards as the ligament becomes narrower, and as the liver is comparatively fixed along its posterior border by the coronary ligament and by the vena cava. However, I find that the liver has, notwithstanding, considerable lateral, or sliding, motion on the diaphragm, besides its motion with the diaphragm. The space, nearly an inch broad, between the layers of the *coronary ligament* is, along the thick posterior border of the right lobe, filled with very loose areolar tissue, and, besides, corresponds mostly to the right limb of the tendon of the diaphragm, on which it moves pretty freely in the lateral direction. The extent of the *left lateral ligament*, forming a free triangular fold, extending a greater or less distance behind the left lobe, may be explained by the shrinking of a formerly larger lobe, but is not the less a structure allowing not only of lateral motion but of forward motion as when the front of the liver swings to the right side, and contrasts with the always small and sometimes scarcely present *right lateral ligament*, as if no provision were required for allowing and then checking a swinging motion of the liver to the left side.

But the great lateral motion of the liver appears to be of a swinging nature, as if with the vena cava for an axis, the displacement being greater the farther forwards, as indicated and regulated by the forward broadening of the falciform ligament. The area over which the line of division between the lobes travels would thus be indicated by a triangle, the apex behind, the base in front, three or four inches in breadth, being twice the breadth of the fore part of the ligament. This triangular area lies more to the right side than to the left, on account of the displacement of the parietal attachment of the ligament, which would correspond to the middle line of the area.

As the falciform ligament is already sloped to the right side, it must check right lateral displacement before the motion has gone to an extent equal to the breadth of the ligament, probably for about an inch; while left lateral displacement, if performed at all, may take place to an extent greater than the breadth of the ligament, for about three inches. The liver may be pushed to the left side with the hand until the falciform ligament checks, and this amount of left displacement may possibly take place when we lie on the left side. We have seen that the natural position of the falciform ligament, in the recumbent posture, is that of sloping to the right side, probably to the extent of about an inch from its parietal attachment. The effect of the erect posture will probably be to carry it an inch farther to the right side, until it checks the motion. It appears reasonable to suppose that the slant of the falciform ligament and the displacement of its parietal attachment to the right side result from the dragging of the liver on that side, as the great weight and sloping position of its right lobe must give it a constant tendency to slip farther to the right side, especially in the erect posture.

*Relative Weight of the Parts of the Liver which lie in the Internal and External Subdivisions of the Right Side.*—If the right lobe, lying on its back on the table, be divided in the middle of its breadth, it will be found that sometimes the outer or right half, sometimes the inner half, is the heavier, whether in the adult or child. I have found each as much as five ounces heavier than the other, and have also found them exactly equal. The rounding of the angles, and the occasional shelving in front, reduces the weight of the outer half, while its greater thickness near the back helps to compensate. The actual thickness of various parts of the right lobe, as ascertained by passing needles through it, varies a good deal in different livers. We may conclude that, on an average, the two half-breadths of the right lobe are of nearly equal weight. But as the right lobe naturally lies sloping outwards and downwards, being indeed almost vertical at its outer part, much more than half of the breadth of the flattened liver must lie bent down in the outer half of the right side of the body. The diameters alone will show this,—5 to 6 inches is a good half diameter across this part of the costo-abdominal cavity, while the breadth of the right lobe, out of the body, is commonly 6 to 7 inches, and it begins fully an inch to the right of the middle line. In a nineteen months' child in which I took these dimensions carefully, the half transverse diameter of the cavity here was 3 inches; an inch of the left lobe lay to the right side, and the right lobe itself, out of the body, measured 4½ inches. The right lobe weighed 7¾ oz., over 4 oz. of which belonged to its outer half. Thus, 5½ inches of breadth of liver was placed in 3 inches breadth of cavity, by bending down of the outer part, so that a median subdivision of the right side of the cavity would necessarily leave a much greater weight of liver in the external than in the internal subdivision. The outer part went down as far as the lower end of the kidney for an inch below the last rib, and to within an inch of the iliac crest. The height of the bent down part of the lobe was 3½ inches, measured up to the right of the gall-bladder, and 5 inches to the upper surface so as to include the thickness.

The left lobe was 2½ inches in breadth, weighed over 2 oz., and the ½ inch of it next the fissure weighed ½ oz. and 45 grains.

Besides, the weight of the part in the inner right region is rather diminished by the inch of the left lobe forming part of it, so that the outer region gains over the inner even more than the four ounces which the whole right side gains by an inch of the left lobe lying to the right side. If it is of any importance, then, to distinguish between the two, we must allow a greater weight to the part in the right external than in the right internal region; and we will probably be quite within the mark if, of the 44 ounces of the liver which lie on the right side, we give 24 to the outer and 20 to the inner region.

*Effect of Posture on the Position of the Liver.*—Vertically and antero-posteriorly, the change in the position of the liver when we assume the erect posture is probably of much the same kind as that effected by the active descent of the diaphragm in inspiration, the downward and forward change of the anterior part being due to general descent and flattening, while the comparative fixity of the posterior part prevents what would otherwise be the natural tendency of the liver rather to slip downwards and backwards, from the greater weight and lower position of the posterior part. As already remarked in treating of the ligaments and motions of the liver, the natural effect of the gravitation of the right lobe, called into play by the erect posture, should be to make the liver slip farther to the right side. The cumulative evidence from the study of the ligaments, position, and connections of the liver points to that conclusion; and we have seen that the falciform ligament, in its natural position, already slanted to the right side, will probably allow the liver to swing an inch farther in that direction. The erect posture should thus have the effect of causing a still greater preponderance of the liver on the right side; but as this is only a probability, however strong, I shall not found on it in the calculation, although it is worthy of remark that it is in this posture that the limbs are exercised.

*Supposed Effect of Inspiration in sending the Liver to the right side.*—Dr Buchanan supposes equipoise to be disturbed by inspiration, partly by the pushing of the liver to the right side, partly by greater expansion of the right side of the chest,—that "the right side of the chest bulges out more than the left in inspiration, as is manifest to the eye; and the lower ribs, as they recede from the middle plane, carry with them the liver, which is firmly bound to them on the right and posterior aspects by its short and strong coronary ligament, while its other ligaments, being long and lax, offer no impediment to its outward motion;" and that "as to the stomach and spleen, they go with the liver, to which they are attached by veins and by the omentum minus, rather than follow the left ribs, with which they have no structural connection."

In addition to the vertical and antero-posterior effect of the erect posture, the active descent of the diaphragm (probably for half-an-inch in an ordinary, and from one to two inches in a forced, inspiration) will still farther flatten and depress the liver, and considerably advance it. Whether it will also push it farther to the right side is a question. The general change in the curvature of each half of the diaphragm is to be considered, and, besides the tendency of each half to descend in the axis of its dome, there is the modelling influence of the lung above. The circumferential attachment of the diaphragm is no doubt widened; but, as inspiration advances, the lower wedge of the lung pushes down in between the ribs and the diaphragm, and, by pressing the liver inwards, may at least counteract the gain from the greater width. Anatomically, too, we would expect the stomach and spleen to remain on the left side, even although the liver did go a little to the right in a forced inspiration. The hypothesis, if not altogether untenable, is so doubtful as to form too slippery a foundation on which to build, and Dr Buchanan will perhaps view this conclusion with the more complacency when I have given him, instead, a secure and definite anatomical basis on which he may rest his theory. Dr Buchanan farther identifies his theory with the inspiratory act, in the view that the preferential use of the right limbs is determined, not by their independent motions, as when the child is carried, but by the combined action of the trunk and limbs, for which deep inspiratory efforts are required, and is accordingly developed after the child begins to balance itself and walk. Putting aside the idea of any special influence of the inspiratory act, there still remains the essence of one of the most interesting parts of Dr Buchanan's paper, in which he considers the actions of the limbs as associated with and influenced by the motions of the body.

SPLEEN.—The prevalent weight of the spleen being from 5 to 7 ounces, we may take 6 ounces as the average, the whole of which will fall to be placed in the left external region. Although, relatively to the body, it is smaller in the fœtus, it increases rapidly after birth to the full adult proportion.

PANCREAS.—The pancreas weighs usually from 2¼ to 3½ ounces. Three ounces may be taken as the average. It is usually from 6 to 8 inches in length. By the introduction of needles, and by marking where it crosses the aorta, I find that a third of the length of the pancreas lies to the right of the middle line. Being thicker and vertically enlarged, the duodenal third always weighs considerably more than the middle third, while the extreme or splenic third weighs still less, not much more, and sometimes less, than half of the middle third. From the weighings which I have made they may be arranged thus, as the nearest approximation,—right third, 1¼ oz., in the right internal region; middle third, 1 oz., in the left internal region; splenic third, ¾ oz., in the left external region. This estimate rather understates the weight of the part which lies to the right side.

STOMACH.—The stomach may weigh an ounce more or an ounce less, but the estimate of Clendinning at 4½ oz. may be taken as an average. The extent to which its narrow end passes to the right side is not so easily determined, as the stomach varies in size and in proportionate length in different cases, and as its position varies with its condition, coming more downwards and to the right side when distended. It is loosely described in the books as passing to, or even some way into the right hypochondrium; but, perhaps, no two anatomists or physicians are agreed on an exact distinguishing line between the epigastric and hypochondriac regions. I have seen it go three inches to the right of the middle line, and, when filled, it no doubt comes more out of the left hypochondrium, and will reach some way across the middle. But I do not think that it does so in the child, in which the stomach still retains more of its original vertical position. The median needles go near the pylorus, and I have seen one go exactly through the pylorus. When distended in the child it may cross the middle a little, but it will be safest to reckon it as entirely on the left side. We may reckon 2½ oz. in the left external, and 2 oz. in the left internal region.

This relates to the empty stomach. The additional weight to be allowed on account of the varying contents of the stomach will be considered separately.

KIDNEYS.—The left kidney is generally heavier than the right, by ⅙ oz. according to Rayer, by ¼ oz. according to John Reid. As to position, this would fall to be placed more in the external than in the internal subdivision of the left side.

LUNGS.—The right lung in the male, according to Reid, weighs 24 oz., the left 21, giving a preponderance of 3 oz. in favour of the right. As a small part of the anterior border of the right lung may cross the middle line, and as the lungs in children are probably less developed relatively to the body, the pulmonic preponderance of the right side may be estimated at 2 instead of 3 oz., and may be considered as belonging to the internal right region, as it is the internal left region in which the left lung is excavated by the heart.

HEART.—The average weight of the heart in the male may be taken at 11 ounces. Although it is relatively larger in the fœtus, there is no reason to believe that it is so in the child; on the contrary, the weight of the heart increases with age.

As to position, the heart is divided unequally by the middle line, and the part to the left side is not only the larger but the thicker. Long needles introduced through the middle of the sternum in the child pass quite close by the right side of the mediastinum, and, opposite the heart, pierce the right ventricle, leaving most of it to the right side. If a long sharp amputating knife be introduced below the ensiform cartilage, pushed up behind the middle of the sternum, and a swift straight cut be made through the middle of the chest, the heart will be found to be split so that the right section will be equal to one-third of the entire weight. The part to the right of the section includes, besides the auricle, the right ventricle, except nearly an inch of the breadth of its anterior wall; a slice of the upper part of the septum ventriculorum, and as much of the base of the left ventricle as suffices to carry the origin of the aorta with it.

Either the auricles or the ventricles are more or less full of blood. The auricles would balance each other, while the blood, so long as it is in the ventricles, would preponderate a little on the left side, but so slightly that this need hardly be taken into the calculation.

My experiments seem to entitle the right side to a full third of the weight of the heart, and we shall be within the mark in dividing the 11 oz. into 3½ for the right and 7½ for the left.

THE GREAT BLOODVESSELS.—A great bloodvessel placed to one side of the middle line must be considered as a special weight, as it is always more or less full. To the left side, there are the second and third stages of the aortic arch, and the descending dorsal aorta, at first entirely and then partially to that side, as it reaches to the middle line at the diaphragm. To the right side, there are the ascending part of the aortic arch, or most of it, the vena cava superior, and the ascending cava from where it leaves the aorta to diverge to the right side to pierce the diaphragm. The abdominal cava, besides being larger than the corresponding aorta, lies more to the right than the aorta does to the left on the lumbar vertebræ. The right innominate vein and last inch of the left may be put against the rest of the left innominate vein. The great bloodvessels must therefore be regarded as of almost equal weight on the two sides, the preponderance, if any, not being in favour of the left.

THE INTESTINES.—Everything being considered in connection with the intestines—their position, their mobility, their size, their varying contents—they cannot be reckoned as preponderating on either side. The sigmoid flexure of the colon, lying either in the left iliac fossa, or partially in the left pelvis, according to the state of the bladder, is often enough partially loaded with more or less solid contents; but it may be considered as counterpoised by the cœcum and commencement of the ascending colon, also an accumulating part of the canal, and the cœcum and ascending colon are more capacious in diameter than the descending or the sigmoid colon. The duodenum is on the right side, and its dilated curve from the vertical to the third stage, below the bile entrance, is an accumulating part. The jejunum is heavier than the same length of ileum, but only a few inches of the beginning of the jejunum are fixed on the left side, and a few inches of the end of the ileum on the right. As to intestinal contents, therefore, the preponderance cannot be more frequent or more extensive on one side than on the other in the erect posture. The gall-bladder may contain half an ounce or more of bile, but more or less of this has probably been included in most weighings of the right lobe of the liver, and does not require separate notice.

GREATER WEIGHT OF THE RIGHT LIMBS AND RIGHT WALL OF TRUNK.—If the greater size of the right limbs and muscles on the right side of the trunk be regarded as the result, not the cause, of the preferential use of the right limbs, it will not concern an inquiry into the cause. It will also prevent the experiment of median section of the entire body being a fair one to determine the amount of preponderance on the right side, except in early childhood, and even then the experiment would not be an easy one to perform with accuracy, so that the viscera alone can be trusted to. As a fact, however, its establishment adds to the greater weight of the right side, bringing the centre of gravity still further to the right side than the greater weight of the right viscera alone would have done.

AMOUNT OF INFLUENCE EXERTED BY THE CONTENTS OF THE STOMACH IN INCREASING THE WEIGHT OF THE LEFT SIDE.—The influence of the contents of the stomach in tending to some extent to balance the preponderance of the viscera on the right side must not be overlooked. Room is made for distention of the stomach, partly by pressing up the diaphragm on the left lung, but chiefly by protrusion of the abdominal wall, the transverse colon and small intestines being at the same time pushed downwards and to the right side, while the liver is probably pushed a little further to the right. The stomach itself may, at least when distended, cross the middle line to some small extent, but, for reasons already given, it will probably be most correct to reckon the whole as on the left side; and as the stomach partly comes forth from the hypochondrium when full, we may reckon nearly as much of it in the internal as in the external subdivision of the left side.

A large meal will, as long as it remains in the stomach, bring up the weight of the left side, so as quite to balance the right. But as the weight is variable and temporary, we must calculate not only the amount introduced but the time during which the whole or part of the contents remain in the stomach. It will be necessary to consider the solid food and the fluids separately.

(1.) *Amount of the Food and the length of time which it remains in the Stomach.*—From the dietary of the navy, army, and various public institutions, the conclusion may be drawn "that a daily amount of food, varying in quantity between 35 and 25 ounces, is sufficient to maintain health.[1] This applies to healthy active adults. But, as most persons eat more than the system requires, and as the child requires proportionally a greater amount than the adult, having to grow as well as to renew its tissues (although what is required for growth is very small compared with what is required for renewal), instead of the average of 30 oz., or even the maximum of the above estimate, it will be safe to allow say 40 oz. as the amount of solid food introduced into the stomach in the course of the day. Considerations as to the habits of eating should, in this inquiry, be such as are applicable to the generality of mankind, but in reality it comes to the same in the end whether the meals are few and large, or, as especially with children, more frequent and smaller. For the calculation, let us assume four meals a-day of 10 oz. of solid food each.

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[1] Todd and Bowman, Physiological Anatomy and Physiology of Man, vol. ii. p. 157.

Now, as to the length of time during which this weight remains in the stomach. Facts as to the rate of stomach digestion[1] show that, in health, a full meal, consisting of animal and vegetable food, may be nearly chymified in about an hour, and entirely gone from the stomach in two hours and a half. A meal consisting chiefly of animal food may take from three to four hours to disappear from the stomach. For a calculation, we may allow each meal an average of three hours, reckoning the whole quantity as present for half the time, and, on an average, half the quantity for the rest. The four digestions would thus occupy six hours with the full weight, and six with half the weight, equal to nine hours of the whole 10 oz. Or the same result may be obtained by supposing the 10 oz. to exert its whole weight for two hours and a quarter. One of the four digestions, however, falls fairly to be put against the eight hours period of sleep, leaving the full weight of 10 oz. during, say, seven of the remaining sixteen hours. Thus the visceral preponderance on the right side falls, on account of the food, to be diminished by 10 ounces during seven hours, and remains undiminished during the remaining nine hours. It may be observed, however, that as there is a natural tendency to rest after a full meal, the part of the sixteen hours spent in the erect posture, so as to call forth the play of the limbs, is least likely to include the time when the stomach is loaded.

As a greater weight for a part of the time will, in this case, have the same effect as a smaller weight acting permanently, it will simplify the calculation to consider the influence of the food as equal to that of a permanent weight in the stomach of about 4½, say 4¾ oz.

(2.) *Amount of Fluid, and the length of time which it remains in the Stomach.*—It is not so easy to determine the usual quantity of fluid daily introduced into the stomach in health. The cases of excessive drinkers of whatever fluid (water, tea, or alcoholic fluids), are not to be reckoned, as the preferential use of the right hand was determined before the drinking habit, and as we must keep in view no less the uncivilized tribes and the generality of mankind. The best method will be to estimate the quantity of fluid daily given off by the body. For determining this, many careful experiments have been made of the amount of water given off by the kidneys, skin, and lungs.

*The amount given off by the skin,* extremely variable under different conditions, and even sometimes in the same person under similar external conditions, has been very variously estimated. By Funke, at from less than 1 oz. to over 25 oz. Seguin's estimate would give about 28 oz. on an average; and Valentin's goes as high as 40 oz. a-day. To form an estimate here, we would require to know the quantity of urine which was secreted during the experiments which give so high an average for the amount of water thrown off by the skin. In the present state of our knowledge, we may fix on 25 oz. as an average quantity for a general calculation.

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[1] See in Todd and Bowman, op. cit., p. 209. Also, in Kirkes' Handbook of Physiology, 4th edition, 1860, p. 221.

*The amount of the pulmonary exhalation* has also been variously estimated. By Vierordt, at 11 oz.; by Valentin, at 14 oz.; by Seguin, at about 16 oz.; while Todd and Bowman, founding on the experiments of Valentin, Brunner, and Moleschott, give 12 to 20 oz. as the range in temperate climates. We may assume 15 oz. as the average.

The ordinary estimate of *the amount thrown off by the kidneys* of an adult healthy man, has been 30 oz. in summer, and 40 in winter, giving an average of 35. The more recent experiments of Thudicum, on two men, would raise the average to from 61 to 69 oz.; while those of Bischoff and Vogel would make it less, indicating a normal range of from 40 to 70 oz.[1] It is difficult to fix an average for either the renal or cutaneous watery secretions, as each is influenced by the amount of the other, and by various other circumstances, but, as an approximation, we may assume 50 oz. as the average amount thrown off by the kidneys.

This would give a total of 90 ounces, equal to four pints and a half, as the amount of fluid introduced into the stomach daily. If it is supposed that I have understated the amount secreted by the kidneys and skin, though I do not think so, it may be recollected that it is a generally accepted doctrine in physiology that the quantity of water which passes from the body is greater than that which enters it, the estimate of M. Barral being that from $\frac{1}{8}$th to $\frac{1}{6}$th of what passes out is formed within the body. But I will not make any deduction on this account.

Now, as to the time during which this weight operates in the stomach. Fluids disappear from the stomach with great rapidity. Thus, in the words of Dr Kirkes (op. cit., p. 221), "Those portions of food which are liquid when taken into the stomach, or which are easily soluble in the fluids therein, are probably at once absorbed by the bloodvessels in the mucous membrane of the stomach. Majendie's experiments, and better still, those of Dr Beaumont, have proved this quick absorption of water, wine, weak saline solutions, and the like; that they are absorbed without manifest change by the digestive fluid; and that, generally, the water of such liquid food as soups is absorbed at once." We can, therefore, allow but a very short time for the disappearance of ordinary quantities of fluids in the ordinary conditions of health. The whole quantity will be there but for a very short time, as its disappearance

[1] Day's Chemistry in its Relations to Physiology and Medicine. 1860.

will commence almost immediately, and the weight will be a gradually and rapidly diminishing one. If we allow for an average a weight equal to the whole quantity for half-an-hour, we shall probably be within the truth. We have to suppose, then, that 90 oz. of fluid pass into the stomach of an average adult daily, and that the stomach retains that weight for half-an-hour. This falls to be distributed over only sixteen of the twenty-four hours, as we must deduct the period of sleep.

Although a large part of the drink is taken along with the meals, we cannot with even an approximation to accuracy say how many times a-day fluid is introduced, as in the case of food; and it will come to the same result if we distribute it equally over the sixteen hours. Thus 90 oz., divided by the thirty-two half hours, will give under 3 oz., say 3 oz. as a permanent weight, representing the influence exerted by the fluid contents of the stomach in increasing the weight of the left side.

Thus, the temporary increase of the weight of the left side, occasioned by the contents of the stomach, solid and fluid together, may be estimated as equal to a permanent weight of 7¾ oz. Forty oz. of food remaining for two and a half hours is equal to 100 oz. remaining for one hour; while 90 oz. of fluid remaining for half an hour is equal to 45 oz. for an hour, giving equal to a total of 145 oz. for an hour. But as 10 of the 40 oz. of food fall to be reckoned against the period of sleep, the amount of food to be placed against the remaining sixteen hours will be equal to a weight of 75 oz. for an hour, being a total weight of food and fluid equal to 120 oz., or 7½ pounds, remaining in the stomach for one hour. Divided by sixteen, for the sixteen hours, we arrive at the estimate of 7½ oz., which I have taken at 7¾ in order to be within the mark. Thus, we may say that, while a weight equal to more than that of two livers passes through the stomach daily, it exerts an influence somewhat more than equal to that of the permanent presence of a second spleen, and we will be able to realize this if we imagine a body like a full-sized spleen to be constantly carried in the stomach.

Symmetry and Equipoise of the Right and Left Sides Zoologically and Developmentally Considered.—When we examine the instances of unequal development on the two sides, among animals constructed on the symmetrical plan, it does not appear that there is any preference in nature for one side more frequently than for the other. Among the mammalia, we observe that it is the left tusk of the narwhal which is normally so greatly developed. In birds, the ovary and oviduct which are developed are almost always the left. In serpents, the lung which is so much developed, compared with the other, is the right. Among fishes, the pleuronectidæ, or flat fishes, present a remarkable asymmetry in the twisting of both orbits, and partly of the mouth, to one side, that which they present to the light. In the sole it is the right side, in the turbot it is the left side, to which the eyes are turned; but it is not uncommon to find this reversed, and in the flounder the eyes are found nearly as often on the left as on the right side.

The question of anatomical symmetry is, however, a different one from that of equipoise. It is more especially the single organs—the alimentary viscera, the spleen, and the heart—which affect the equipoise. This question does not appear to have attracted much notice, except in respect to birds, in which the mesial position of the heart, and the nearly equal size of the two lobes of the liver on each side of the middle line, have evident reference to the necessity of equipoise for locomotion in the air. Equipoise would appear to be almost as necessary for animals which locomote in the water; but less so in the case of animals which tread upon the resisting ground.

It is interesting to notice how far this equipoise is preserved or departed from in the development and ultimate localization of the single viscera, in quadrupeds and in man. The great bloodvessels are originally developed symmetrically, and the heart mesially, and it is the subsequent turning of the heart to the left which renders the lungs of unequal weight. In the abdomen, the stomach, intestines, pancreas, liver, and spleen, were originally developed mesially. The front of the stomach and pancreas was originally their left lateral surface. Their originally posterior end has been turned to the left, carrying with them the spleen, while the liver has become a right organ mainly by the diminution of its originally equal-sized left lobe. The side to which they go is sometimes reversed, constituting transposition of the viscera, which is complete when it occurs, and which makes no difference to the health and comfort of the individual. The object of their going to one or other side is evidently the convenience of package, whether we regard it from the final cause point of view, or as merely a matter of mechanical adaptation under the pressure of growth. The result of these changes, while they necessarily end, by mere package, in a kind of general balance of the two sides, is to endanger the exact equipoise. Now, while the viscera of the quadruped have the same general lateralized position, as in man, there is a reason why this should be carried to a greater extent in man than in the quadruped, owing to the much greater lateral development of the chest and abdomen of the human figure, in order to adapt it to the erect posture, as contrasted with the great lateral flattening of the trunk in quadrupeds. The equipoise is, therefore, more disturbed in man than in the quadruped; and it may be observed that the same consideration applies to the child, in whom the chest is at first narrow, and in whom it undergoes rapid increase of breadth when the child begins to walk, the period at which, according to the gravitation theory, the predominance of the right hand should begin to be developed.

To this reason why the position of the centre of gravity to the right side should have more influence in man than in the quadruped, may be added the fact of the erect posture enabling the gravitating influence to operate at once on the whole of one side of the body. The fact that, in ordinary quadrupedal locomotion, the animal rests on the fore-foot of one side and the hind-foot of the other, should not prevent the influence of the greater weight of the viscera of the right side from telling on the limbs of that side; but the influence must be less in the quadruped, for the reason above given. Whether the limbs on the right side of the quadruped possess, apart from training by man, some little additional strength compared with those of the left side is not well ascertained. My shoemaker informs me that the right side of the hide is generally thicker than the left; and on farther inquiry at a leather-merchant who deals largely in the hide of the calf and ox, I am informed that the above is a well-known fact in the trade, the right being known as the "lying" side of the skin. The quadruped would appear at any rate to lie more on the right than on the left side. It would seem as if this must be during sleep, for one may count the ruminants, with their paunchful of grass, resting about as numerously on the left as on the right side; but the direction of the wind and sun, and the slope of the ground, with the direction in which the animal was standing, may influence this, and there should be a tendency to alternate the sides for muscular relief.

The great difference between man and the lower animals in regard to the use of the limbs is, of course, the absence in the latter of anything like training or skilled labour, which in man carries to such a high degree the predominance of the right limbs, previously determined by some cause for which we are in search; just as the difference between the two hands is much greater than the difference between the feet.

Conclusion as to the Relative Weight of the Viscera on the Right and Left Sides.—The accompanying table shows the weight of the several organs and parts of organs on the two sides of the body, placed in their respective regions. For the reason already mentioned I have attended to the distance of each organ, or its parts, from the middle line. Had distance from the middle line increased the influence of the weight of an organ it would have added still more to the preponderance of the right side; but this would be confounding leverage with weight, as the absolute weight remains the same at whatever distance from the centre. Nor is it of importance in this respect, or to the general result, to distinguish the two modes in which the greater weight of the right side will be transmitted to the lower limb, viz., partly by the spine, and partly directly down through the viscera below the liver. The general result is that, as far as the viscera alone are concerned, the right side is at least 22¾ ounces heavier than the left, and that this is reduced 7¾ ounces by the influence of the contents of the stomach, leaving a clear preponderance of at least 15 ounces in favour of the right side. This amount would bear reduction, and still leave sufficient to disturb the equipoise of the body. If any one will take the trouble to repeat my experiments, he may perhaps differ from me slightly here and there, but will, I believe, find that I have rather understated the general result, for, in taking the estimate of each viscus, I have kept within the mark so as to leave no element of doubt. The preponderance of the right side is probably considerably greater than 15 ounces, and it is probably still greater in the erect posture. This, it will be remembered, refers to the adult weights, but, although the difference is less absolutely in the child, it is proportionally the same, as I have made the calculations on the proportions in the child; and I have allowed nothing for the greater weight of the limbs on the right side.

| REGIONS. | Right External. | Right Internal. | Left Internal. | Left External. |
|---|---|---|---|---|
| Spleen, 6 oz. . . . . . . . . | ... | ... | ... | 6 |
| Pancreas, 3 oz. . . . . . . . | ... | 1¼ | 1 | ¾ |
| Stomach, 4½ oz. . . . . . . . | ... | ... | 2 | 2½ |
| Left Kidney . . . . . . . . | ... | ... | ... | ¼ |
| Right Lung . . . . . . . . | ... | 2 | ... | ... |
| Heart, 11 oz. . . . . . . . . | ... | 3½ | 7½ | ... |
| Liver, 52 oz. . . . . . . . . | 24 | 20 | 8 | ... |
| Total, . . . . . | 24 | 26¾ | 18½ | 9½ |
| Contents of Stomach, 7¾ oz. . . | ... | ... | 3¾ | 4 |
| Total, . . . . . | 24 | 26¾ | 22¼ | 13½ |

| | |
|---|---|
| Total of Viscera on right side . . . . . . . . . | 50¾ oz. |
| 〃 〃 left side . . . . . . . . . . | 28 |
| Visceral preponderance on right side . . . . . . . | 22¾ oz. |
| Deduct for contents of stomach . . . . . . . . . | 7¾ |
| Total preponderance on right side . . . . . . . . | 15 oz. |

I hope on another occasion to be able to enter on an examination of the theories which have been proposed in explanation of the preference of the right hand by all nations of mankind. Before doing so I am anxious to obtain from those who are personally acquainted with them information regarding the phenomena and the early history of the occasional cases of true preference of the left hand, as distinguished from the more frequent case of ambidexterity acquired by practice. Meanwhile, I content myself with having shown that, from the arrangement of the viscera, the body is considerably heavier on the right side than on the left, with the consequent position of the centre of gravity to the right side of the middle line, whatever the result of this fact may be. As a physical agent constantly in operation in the erect posture, it cannot but exert an influence on the attitudes and movements of the body and limbs, and on the muscles concerned in them.

## ARTICLE IV.—*Cases of Lithotomy, with Clinical Remarks.* By PATRICK HERON WATSON, M.D., F.R.C.S., Surgeon to the Royal Infirmary, Lecturer on Surgery, Edinburgh.

CASE 1.—In January 1858, I was consulted about J. M., æt. 3 years. He was stated to have suffered from urinary symptoms, more or less marked, since birth. For these, various remedies had been employed, affording him relief for a time, but the uneasiness had always recurred when the medicines were omitted. His symptoms consisted in severe pain during and after micturition, so that he screamed and twisted himself about in agony. This pain was referred to the lower part of the abdomen and penis. At night he generally wet the bed. The foreskin was long, and his right forefinger and thumb were sodden from constantly milking at his penis to relieve the pain.

Having, with the assistance of Dr Alex. Cowan, put him under chloroform, I passed a sound and examined his bladder, and almost immediately struck sonorously against a calculus. Passing the finger up the rectum, I was able to recognise the size and form of the stone caught between the finger and the sound. It appeared to be about the size of a garden bean.

Having communicated to the friends the result of the examination, they readily assented to the performance of lithotomy.

A few days afterwards, accompanied by Professor Miller, Mr Spence, Dr A. Cowan, Dr James Miller, and several of my pupils, I went prepared to operate, but, upon passing the staff into the bladder, I could detect no calculus. Professor Miller and Mr Spence also examined the bladder, but with a like unsuccessful result. On withdrawing the staff, and passing the sound, still no stone could be detected. Accordingly, though morally convinced that the stone was there, I decided that it was imprudent to proceed with the operation.

As the urine was acid and contained lithates in quantity, I ordered liquor potassæ to be taken along with a simple farinaceous but nutritious diet. Under this course of treatment the symptoms of stone gradually abated. After this, although I sounded the patient on three different occasions, with about a week's interval between them, I could not again detect a calculus. From that time, although I occasionally heard of the patient, I did not again see him till December 1862. I was then requested to visit him, on account of a great aggravation in his urinary symptoms. I found that for more than a year past he had been attending school, but that latterly he had been ashamed to go, as the frequent calls to make water obliged him to ask out nearly every quarter of an hour. On passing the sound I at once came in contact with the stone, and, on introducing the finger up the bowel, again felt it between the finger and the sound, as on the first occasion. The urine, when allowed to stand, deposited a large quantity of gelatinous mucus and phosphates, which served to account for the recent increase in the severity of his symptoms. He was somewhat emaciated, but sprightly; his digestion and circulation were perfectly normal.

On the 20th December 1862, with the kind assistance of Professor Miller, Mr Spence, and several pupils, I removed the calculus by the usual lateral operation of lithotomy. There was almost no bleeding, and after an opiate the patient, although he did not sleep, remained quiet and easy for the first forty-eight hours after the operation, passing water copiously through the tube, and by the wound. On the 22d, when seen in the evening, he was found to have passed an uneasy, restless day. His tongue was coated, his face suffused, and he complained of sore throat. The wound looked healthy, and the urine flowed freely. On the morning of the 23d, his bowels acted freely and normally after a dose of castor oil. He had, however, passed a restless, feverish night, with slight delirium. His pulse had risen to 120, and his expression was indicative of a restless anxiety. The sore throat was gone, but his tongue was dry; the cutaneous surface had a slight mottled suffusion. The tube was removed. In the evening the pulse had risen to about 160; his thirst was extreme; he was very restless; the edges of the wound were ash-grey; the perinæum tumid, and suffused with a dusky redness. The urinary secretion flowed through the wound, but was diminished in quantity. Poultices were applied to the perinæum, and he was ordered aq. acet. ammoniæ, with orange-juice and soda-water for a drink.

24*th*, He passed a restless night, with no sleep and occasional delirium. His pulse still continued above 120; the wound was more tumid and sloughy, while, from the diffuse redness, it almost seemed as if sloughing phagedæna were about to attack the whole perinæum. Professor Miller, who saw him with me, recommended that the incision should be extended forwards through the middle of the tumid perinæum, and that chlorate of potash should be given internally. To-day, when his bowels acted, the feces passed through the wound. This state of matters continued with little or no modification for about a week, then the feverish symptoms began to abate, and his appetite to return, while the secretion of urine became more copious and less fetid than it had been previously. The sloughy margins of the wound, which were limited to about one-eighth of an inch in thickness, began to separate, but the wound still gaped, and showed no indication of contracting.

At the end of a fortnight, as the feces still continued to pass through the wound, I examined the condition of the parts by introducing the finger up the rectum. At about an inch and a half from the verge of the anus I found an aperture through which the finger readily passed into the perineal wound. Accordingly, inserting a probe-pointed bistoury through the wound, I divided the septum which existed between them.

From this period onwards, for the next six weeks, with careful nursing and tonic treatment, he improved so far that he was able to be up all day upon the sofa, the wound had completely cicatrized, there was no longer any remains of urinary irritability, his appetite was good, though capricious, and he slept soundly at night. His urine was perfectly normal in every respect. He was able to walk about the room, and was apparently in a fair way to be speedily restored.

On the 20th of February 1863, the sixty-second day from the time of the operation, and more than a fortnight after the healing of the wound, I was hurriedly sent for to see J. M., who was said to have been taken suddenly ill, and to be dying. On reaching the house I found him with a pale, clammy face, and livid lips, sitting in an arm-chair, gasping for breath, and complaining of a sensation of inexpressible anguish in the præcordial region. His pulse was very frequent, uncountable, and irregular; his heart's action tumultuous and fluttering; his respiration forced, but apparently unaccompanied with any mechanical obstacle to the entrance of air, for though the respiratory sound was very harsh, and there was some submucous rattling at both lower bases, there was neither dulness on percussion, nor silence over any part of the chest. He was ordered brandy and ammonia, and to have hot poultices sprinkled with mustard, applied to the chest. These afforded some alleviation to his symptoms, and he rallied so far for a time as to be able to lie back upon the pillows: the deadly pallor and purple suffusion of the lips diminished, but although he expressed himself as feeling easier, he gradually sank and died about midnight.

The calculus removed in this case was of a flattened oval form, about the size of an almond, weighing 35 grains; it was 15-16ths of an inch in length, 11-16ths of an inch in breadth, 4-16ths of an inch in thickness. It consisted of two different strata. One of these, exposed around the margins and constituting the mass of the calculus, of a brownish yellow colour, and smooth, almost polished surface, with slightly elevated tuberculations, was obviously composed of uric acid; the other, coating the flattened surfaces on either side, except in the very centre where the uric acid basis was exposed, was of a dark ruddy-brown colour, of a porous structure, with a thin surface-coating of white shining crystals; the former portion consisting of phosphate of lime, the latter of the triple phosphate of magnesia and ammonia.

*Sectio Cadaveris.*—I examined the body of this patient fifteen hours after death, opening all the cavities, and inspecting all the contained organs. On removing the heart, the cavities on the right side were found uncontracted and firm, whilst those on the left side were fully contracted. On opening the right auricle and ventricle, they were found occupied, but not filled, by a colourless adherent coagulum, surrounded by fluid blood and coloured coagulum. This colourless clot extended through the pulmonary orifice without fully occupying it, or being adherent to its valves or inner coat, and was traced through all the ramifications of the artery; and on afterwards making sections of the lungs, the minute pulmonary arterial branches could at once be recognised by these unadherent firm coagula lying in their channels. There was no congestion of the lungs; they were throughout crepitant, with marginal emphysema, and only here and there lobular collapse was found scattered through their substance. The larger bronchial tubes contained copious frothy and adhesive mucus. The coagulum in the right side of the heart occupied the appendix of the auricle, but did not extend into either the superior or inferior vena cava. The kidneys, ureters, and bladder were perfectly normal,—the last-mentioned organ being unusually capacious for a boy of seven years of age. The situation of the prostatic wound could scarcely be recognised, and, on cutting through the substance of the gland and its capsule, the prostatic veins and sinuses seemed perfectly normal; the iliac veins and vena cava inferior were laid open, and only contained a normal-coloured coagulum of moderate size, unadherent and unobstructive. The perineal wound and the incision which divided the septum between it and the rectum were perfectly healed. The other organs were quite healthy.

*Remarks.*—This case presents several features of interest to the practical surgeon and to the pathologist:—

1. The difficulty experienced in finding the stone.
2. The febrile affection which followed the operation.
3. The complication produced by the spontaneous formation of a rectal fistula from sloughing.
4. The sudden and unexpected death of the patient, after his recovery from the operation, and the associated post-mortem appearances.

1. *The Difficulty experienced in Detecting the Stone.*—The patient, when first I saw him, had the rational symptoms of calculus so well marked that, before sounding him, I was quite confident he must have a stone in the bladder. The ready detection of the presence of the calculus on introducing the sound, the facility with which I was able to determine its form and recognise its size from the rectum, confirmed this diagnosis, and, further, led me to anticipate no difficulty in the operation. On proceeding with it, however, a few days afterwards, the inability to detect the calculus occurring not only to myself but also to Professor Miller and Mr Spence, the conclusion was inevitable to every one but myself that I had been mistaken in supposing that I had heard the click of the instrument against the stone, and felt it through the rectum. The absence of this essential preliminary to the operation, that, namely, of convincing those who were to assist me of the presence of the calculus in the bladder, of course at once negatived any operative interference, though I was confident that the calculus was in the bladder, and that

it was of a size too large to admit of its spontaneous elimination by the urethra. But as the negative results of the repeated examinations, coupled with the satisfactory effects of the administration of internal remedies, made the existence of the stone still more problematical, I was led to the abandonment, for the time being, of further attempts to detect its presence; for it is a fact recognised by all practical surgeons, that repeated attempts to detect the presence of a calculus by means of the sound, may, and sometimes does, induce fatal cystitis.

In most cases of stone, the detection of its presence by means of the sound is a matter of very great ease, especially when the patient is under the influence of chloroform, and the bladder contains a moderate quantity of fluid; but in some cases very great difficulty may occur. When the stone is small, and the bladder capacious or much fasciculated, or the prostate considerably enlarged, this need excite no astonishment. I had occasion, not very long since, to attend a patient on his deathbed, who for the greater part of his life had been the subject of vesical uneasiness, for which he had been under the care of Mr Liston when he was in Edinburgh, and of many other surgeons since. During the whole of that period, extending over more than thirty years, he had undoubtedly had an enlarged prostate, and although he believed himself to be suffering from calculus in the bladder, and had on many occasions been sounded, none had ever been detected. For years past, furthermore, he had required to pass the catheter twice or thrice a-day to relieve himself, but he had never felt the stone. His weakened condition prevented him from any longer relieving the retention by means of the catheter, and for this purpose I was called to see him. The very first time I introduced an instrument to withdraw his urine, as the bladder gradually emptied itself, the beak of the catheter, turning to one side, came in contact with a calculus. After death, lying behind the prostate, which was much enlarged, there were two very considerable sized phosphatic calculi, and an innumerable collection of smaller ones.

While such causes, then, as already mentioned, do certainly create difficulties of a mechanical kind in the detection of calculi, it does seem strange that a stone of some size in a child's bladder should first have been so easily detected, and then so frequently have eluded a carefully conducted examination. But such cases are not unprecedented. Mr Coulson[1] mentions no fewer than three such occurring in his own experience; and in Civiale's *Traité de l'Affection Calculeuse, Paris* 1838, an extended list will be found of examples of this kind gleaned from the works of the older surgeons.

It might, perhaps, be presumed that the early date at which the symptoms commenced, afforded a presumption in favour of the non-existence of a calculus. This, however, should not be the case, for symptoms of stone sometimes commence from the first few days after birth,—the infant having suffered more or less constantly, and obviously from uneasiness connected with micturition, till relieved by the operation. Langenbeck of Berlin met with a small uric-acid calculus in a male fœtus at the sixth month. Stahl found a calculus of the size of a peach-kernel in an infant of three weeks old, who had suffered from uneasiness in passing urine from its birth. I had occasion, some years ago, to operate on a boy, aged two and a half, who had suffered from symptoms of vesical irritation ever since his birth.

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[1] On the Diseases of the Bladder and Prostate Gland. London: 1857.

Neither did the complete subsidence of the symptoms, when the acid condition of the urine had been neutralized by alkalies, militate against the existence of a calculus. Such cases have been observed frequently enough. In one such, which came under my notice several years ago, a patient of the late Dr Richard Mackenzie, the symptoms of stone came on after the subsidence of an attack of gonorrhœa, which had commenced only a few weeks before, and the calculus, when removed, was found to be of very moderate size, consisting principally of oxalates, with a slight and recent coating of phosphates. The operation was performed upon the patient when seventeen years of age; and in infancy he had suffered from symptoms of a calculus in the bladder, which had been detected by the late Mr Liston, but left unremoved, as the parents objected to the performance of any operation. In this case the symptoms had spontaneously subsided, and no further uneasiness had been experienced till the attack of gonorrhœa renewed the irritation which had so long continued in abeyance.

2. *The Febrile Attack which followed the Operation.*—This was undoubtedly due to the occurrence of scarlatina, which, although accompanied by no characteristic eruption, and with a very evanescent sore throat, apparently expended itself upon the lithotomy wound, producing phagedena of its surface,—a result that I have seen occur more than once in cases where tracheotomy was performed for scarlatinous laryngitis or laryngeal obstruction, occurring during diphtheria. That the fever was really due to scarlatina is rendered more probable when I mention that his elder brother took ill immediately after with undoubted scarlatina, followed by dropsy and otorrhæa; and further, that he had been in his sister's house, where a nephew had died of scarlatina, within a few days of the operation. Nay, as I afterwards learned, the very crib and bedding on which he lay after the operation were brought from the infected house.

3. *The Spontaneous Formation of a Fistulous Communication between the Rectum and Perineal Wound.*—This may occur as a direct result of the operation of lithotomy, the bowel being wounded by the knife of the operator. That such an accident had not occurred in this case was evident from the bowels acting for the first time in the natural way without any discharge of feculent matter occurring through the wound, while the fistulous communication became

established only during the period when the wound presented a phagedenic appearance. The situation of the fistulous communication was about an inch and a quarter up the bowel from the verge of the anus. Now, in most instances of accidental wound of the rectum during the operation, the part of the bowel in close proximity to the prostate is the part most liable to suffer from the knife of the operator. I believe, therefore, that the fistula was produced by the phagedenic ulceration which attacked the wound as an accompaniment of the scarlatina. I am aware that this sloughing condition of the wound, accompanied by febrile excitement, might be attributed to extravasation of urine into the cellular tissue of the pelvis. In such cases, however, it is the textures between the neck of the bladder and around the rectum, upon the level of the prostate, which suffer, and not the parts occupying the superficial portion of the wound. Furthermore, the prostate was in this operation divided to a very limited extent, while the external wound was free, and the urinary escape throughout copious; all which militate against the supposition that the symptoms, local and constitutional, were produced by urinary infiltration.

4. *The Sudden and Unexpected Death of the Patient after Recovering from the Operation, with the Post-mortem Appearances.*—That his death was due to the formation of a fibrinous coagulum, which occupied the right side of the heart and the ramifications of the pulmonary artery, is, I think, indisputable. The difficulty which exists in my mind attaches to the source of this coagulum, and the period at which its formation commenced.

Now, it is well, in considering this point, to keep in mind that the only part of the fibrinous coagulum which was attached was the portion within the right auricle and ventricle; that portion which branched through the pulmonary arteries was completely unattached to the serous surface of the vascular coats, as I determined by carefully slitting up all the larger ramifications, and examining the relations of the smaller ones in the sections made of the pulmonary substance. I am confident, therefore, that the coagulum was not the result of embolism of the pulmonary arteries, as, had this been the case, unless affecting all the smaller ramifications, no such extensive result could have occurred. The probable source of such embolic coagula, were this view to be adopted, might naturally be presumed to be the veins around the prostate and within the pelvis; but during the inspection all these vessels were examined, and no trace of any plugging having existed could be detected. I believe, therefore, that this coagulum formed during life, commencing in the right auricle, where it was firmly adherent, extending thence into the ventricle, and thence into the pulmonary artery and its ramifications; that its formation was due to the hyperinotic condition of the blood following scarlatina, aided materially by the enfeebled state of the circulation, induced by the weakening effects upon the system of this attack of scarlatina.

CASE 2.—I was consulted by J. B. on the 12th of April of this year, on account of symptoms of vesical uneasiness. He was a hale, florid, labouring man of sixty-two, a farm-servant, who all his life had enjoyed uninterrupted good health till the end of 1858, when he began to suffer from uneasiness in making water, frequent desire to micturate, and a sluggish flow; shortly after this, retention occurred, requiring the use of the catheter. Till January 1862 this necessity for medical assistance did not again occur; but at that time he was again under treatment for a short period, until, in fact, his bladder regained its tone, after suffering from distention.

From that time he always managed to relieve himself without assistance, and had no great uneasiness until about a fortnight ago, when he began to experience severe pain, especially after making water, and particularly aggravated when walking, or riding in a cart. This pain was referred to the lower part of the abdomen, the point of the penis, and the back of his calves and feet, especially the right heel. On passing a sound, and moving it first on the one side and then on the other, from the fundus to the neck, no foreign body could be detected. On turning the beak of the instrument round behind the prostate, towards the rectum, it immediately came in contact with a calculus; and on passing the forefinger up the rectum, and elevating the neck of the bladder, the presence of the stone was readily recognised.

I recommended that he should submit to an operation for the removal of the stone, and should enter the Infirmary for that purpose. To this he readily acceded.

On the 15th of April, I accordingly operated upon this patient by the ordinary lateral operation for lithotomy. The bladder was easily reached; but, on passing the finger along the wound, only a small portion of the upper surface of the calculus could be felt projecting over the enlarged prostate. In this situation the forceps could not grasp it; but by introducing the scoop below it, and raising it from the cavity in which it lay, I at once, with the greatest facility, removed the stone from the bladder. There was little bleeding, and the urine flowed freely through the tube which was introduced The patient was conveyed to bed. The calculus, which was of a trigonous form, consisted of uric acid, coated with a thin crust of shining phosphates. It weighed three drachms and a half, and measured nearly an inch and a quarter in every direction.

The patient passed the first four days after the operation in his usual health, his tongue clean, his pulse quiet, no thirst, without complaint of pain, and with an abundant secretion of urine. On the afternoon of the third day the tube was removed, as he complained of its making him uncomfortable. On the evening of the fourth day, as he had passed no water either by the wound or by the urethra, Dr Jolly passed the catheter and drew off about twenty ounces of urine, containing a copious deposit of mucus.

From that period till the 26th of April (eleventh day) he required the use of the catheter twice or thrice a-day, not a drop of water ever escaping through the wound. On the 26th of April, as his tongue was dry, his pulse accelerated, and he complained of thirst and considerable vesical irritation, he was ordered aq. acet. ammoniæ, alkalies, and a dose of laxative medicine. When his bowels were being evacuated, about a tablespoonful of pus escaped from the urethra, and he made water freely by the natural channel.

From this date, under the use of mineral acids, the urinary secretion became daily more normal, containing less and less mucopurulent fluid, while, with the exception of one day, he did not again require the use of the catheter.

10*th May*.—The external wound, which has hitherto remained open, is now completely cicatrized. For a week he has been out of bed every day, and for the last day or two he has been able to be out of doors.

14*th May*.—Discharged perfectly well.

*Remarks*.—In this patient, as in the former case, a difficulty was experienced in finding the stone; but here the difficulty arose from a mechanical cause, overcome by turning round the beak of the sound behind the enlarged prostate. Had I not, however, resorted to this proceeding successfully before, in examining a patient with calculus in which the stone could alone be detected in this way, the enlargement of the prostate might easily have been overlooked, for to the finger passed up the rectum there was no sign of any unusual condition of the prostate to lead me to anticipate that this means of manipulation would be requisite. I believe, however, that in many cases the form of partial enlargement of the gland, in which it projects not towards the rectum but into the interior of the bladder, can alone be estimated by the facility with which the sound, when the beak and curve have been completely passed into the vesical cavity, can be turned round without encountering any obstacle. At the same time, the introduction of the finger up the bowel, to support the neck of the bladder and regulate the movements of the sound, when examining this part of the viscus, should never be neglected, as, in the case of a small or oval and very flat stone, without the support given by the finger, the short beak of an ordinary sound may turn round and round, and miss the stone altogether.

The striking feature in this operation was the inefficiency of the forceps as a means of seizing the stone. Before commencing the operation, I anticipated that there would be a difficulty in laying hold of the calculus, believing, as I did, that its size was not larger than a walnut, and apprehending that, being lodged behind the enlarged gland, it would not come forward into the neck of the bladder in the gush of urine. This I found to be the case, for, as I kept my finger in the wound, passing the forceps over it, I felt the stone remain unmoved. Each time I attempted to seize the stone in their grasp, the edges of the blades merely rasped over the surface without securing it. I did not find the curved forceps more efficacious. I therefore introduced the scoop, and, sweeping its spoon behind the prostate and beneath the stone, lifted it to the level of the prostatic incision, and, holding it between the scoop and the finger, extracted it with the greatest facility.

In cases of lithotomy where no tube is employed, immediate adhesion of the wound, followed by retention of urine, or by the patient retaining his urine for some hours, and when the bladder becomes distended, voiding it by the urethra, sometimes takes place. Such union, however, is rarely permanent. Either it is broken up by the surgeon passing his finger through the wound, so as to relieve the retention which is present, or to prevent the occurrence of urinary extravasation into the parts around the wound, or when inflammation of the wound becomes established, the urine again finds its way through the perineal incision.

In the present case, however, union not of the whole wound, but only of the prostatic and urethral portion of it, took place within a few hours after the withdrawal of the tube, and this union was so complete, that not a drop of water afterwards escaped by the wound.

I am inclined to believe that the condensed textures of the enlarged prostate tended in no small degree to produce this result, as upon them the presence of the tube would of course act less powerfully in the way of effecting gaping of the deep portion of the wound than in a healthy gland. No doubt the small incision, and the entire absence of any forcible measures in extracting the stone, materially aided in this result. The discharge of pus along the urethra, preceded by some vesical irritation, must, I think, have been due to the formation and collection of pus in connexion with the deeper part of the wound, which made its way inwards instead of outwards through the superficial portion of the incision, which was nearly healed. Another explanation is, however, possible. The patient, on the occasion when the pus escaped along the urethra, had his bowels moved for the first time upon the night-stool, while before he had always employed the bed-pan, and thus the mucopurulent matter which had collected behind the prostate gravitated forwards, and during his efforts at defecation was expelled along the urethra before the urine which immediately followed. This, however, would scarcely satisfactorily account for the rapid improvement by the abatement of the pre-existing febrile disturbance of the system which immediately ensued.

To some, the rapid healing of the deeper part of the wound in this patient's case, might seem to indicate the propriety of attempting to secure this result in lithotomy generally, by doing away with the use of the tube. This were no novel proceeding; indeed, it is what has generally been adopted by all non-professional lithotomists, and, from their example, imitated by some surgeons whose success in the operation has been unrivalled. There is but one objection to disuse of the tube, and that is the risk of urinary extravasation. No doubt, in most instances, the larger portion of the urine flows not along, but by the side of the tube; still, by its presence, the tube secures the wound from partially uniting, and were this to occur externally instead of in its deeper part, as took place in the case under consideration, it would in all probability be fraught with serious local results, if not by disastrous consequences implicating the patient's life.

No doubt, urinary infiltration around the neck of the bladder and rectum does sometimes occur where the tube has been employed, but in such circumstances the tube is not in fault; the prostate has been too freely divided, and the pelvic fascia at its base cut through, either from the size of the stone requiring this, or from the operator having entered his knife through the prostate instead of the membranous portion of the urethra, or from not having been upon his guard to avoid too extensive a use of his knife as he withdrew it from the bladder. The occurrence of such accidents should be considered as affording an argument not against the use of the tube, but rather in its favour; for the very object of using the tube is to prevent immediate union of the wound, and while the wound keeps open, the risk of infiltration from any cause must be acknowledged to be less than where it has united.

## ARTICLE V.—*Note on the Calabar Bean.* By D. ARGYLL ROBERTSON, M.D., F.R.C.S.E., Lecturer on Diseases of the Eye, Edinburgh.

IN my paper on the Calabar Bean as a new agent in Ophthalmic Medicine, published in the March Number of this Journal, I recommended the use of solutions, of various strengths, of the extract of the Calabar bean, prepared by the addition of water to the residue of the evaporated spirituous extract. The solutions, or more strictly speaking the suspensions, thus formed, at first answered very well, but I found that with keeping they became decomposed and fetid, and when applied to the conjunctiva occasioned considerable irritation. In their physiological effects they did not, however, appear to be much impaired. With the view of preventing decomposition and favouring suspension, I substituted glycerine for water, which fulfilled these conditions, but it appeared to produce a much greater degree of irritation, attended with considerable vascular injection and increased secretion of tears and mucus, than followed when the watery solutions were employed; consequently, I now employ simple syrup as a suspensory medicine, which, as far as I have yet had opportunity of judging, seems to prove effectual as regards suspension, and to produce less local irritation than glycerine. Regarding its preservative qualities I am not yet in a position to speak. From a private communication I have received from Mr Hugh Neill of Liverpool, I learn that he also was independently led by the occurrence of putrefaction to the use of glycerine as a suspensory fluid. I may mention, that I have made much stronger suspensions than those I formerly employed, by means of which I have been enabled to overcome the effects of more powerful mydriatic solutions. The capsule of the bean has in my hands proved quite inert as regards action on the eye.

The only other effect following this local application I have noticed, in addition to those I have already detailed, is a slight spasmodic twitching of the eyelids, which I find to occur about the time the affection of accommodation commences. This can only be accounted for by supposing that it stimulates the orbicularis branch of the facial nerve.

I have further found the Calabar bean effectual in counteracting the effects produced by the application of daturine to the conjunctiva. I was led several months ago to test the action of daturine by a statement in a letter of the Parisian correspondent of the *Lancet* (published in the Lancet of 30th August 1862), to the effect that M. Jobert de Lamballe had been employing for some time a simple solution of daturine as a mydriatic, in preference to belladonna or atropine, as he had found that it was three times as strong as atropine or its salts, that its introduction produced no pain, and that vision was not dimmed. My experience of its actions does not altogether tally with M. Jobert's. I am rather disposed to believe that daturine acts somewhat more on the pupil and less on the accommodation than atropine; but that it acts on both in precisely the same manner as atropine I have fully convinced myself. Daturine, moreover, appeared to me to be considerably weaker than atropine. A solution of 4 grs. to the oz., however, produced fully dilated pupils, and a convex glass of eleven inches focus was required ere the finest print (No. 1 of Snellen's types) could be read at eight inches distance from the eye. An interesting point noticed in the experiments with daturine and the ordeal bean, and one which serves to refute the notion still held by some, that dimness of vision in these cases merely depends upon the dilated state of the pupil, was, that soon after the use of the Calabar bean to counteract the effects of the daturine, the affection of accommodation was completely altered (to such an extent that a condition of myopia was induced requiring a pretty strong concave glass for its correction), while the pupil remained fully dilated.

In the *Medical Times and Gazette* of May 9th, under the head of Provincial Correspondence, is a report of a case of dilated pupil with confused vision, the result of a blow on the eye with a stone, the details of which Mr Neill had laid before the members of the Liverpool Medical Society, in which the Calabar bean had proved most serviceable in causing contraction of the pupil and improvement of vision, and this after veratria had been employed without producing any benefit. Although it is not mentioned, the dimness of sight in this case in all probability depended on paralysis of the ciliary muscle, which condition being corrected by the use of the Calabar bean, distinctness of vision returned. This is the more likely to be the case, as, in almost all cases of simple dilated pupil, affection of the accommodation due to paralysis of the ciliary muscle is found to be present.

In a letter published in the same number of the *Medical Times and Gazette*, Mr Woolcott testifies to the efficacy of the ordeal bean as a myositic or contractor of the pupil.

Mr Soelberg Wells has also, I believe, had successful results from the use of the Calabar bean in a case of paralysis of the ciliary muscle and sphincter pupillæ, the details of which, I am informed, are to appear in a future number of the *Medical Times and Gazette*.

From these facts I am confirmed in my opinion of the great value the Calabar bean will be found to possess as an ophthalmic agent, and I doubt not, that when tried in cases of recent prolapsus iridis at the circumference of the cornea, as well as to counteract the influence of atropine, and as a remedial agent in cases, like the above, of paralysis of the ciliary muscle and sphincter pupillæ, it will not be found wanting in beneficial effect. That such a remedy was needed and urgently sought after is evident from the fact that Professor von Graefe, at the recent International Congress of Ophthalmology at Paris, detailed his experiments with morphia as a stimulant to the contractor pupillæ, and ciliary muscles. One case of simple mydriasis, with paralysis of the ciliary muscle, which I saw at Berlin during the winter session of 1860–61, treated by the subcutaneous injection of morphia, certainly did exhibit temporary improvement under its use; but the remedy required to be pushed until the whole system was narcotized, and as the effects of the remedy on the constitution passed off the pupil dilated, and the accommodation returned to its former condition. In the Calabar bean, however, we have a remedy of much greater value as regards these points, as by it we can stimulate the muscle of accommodation and cause contraction of the pupil with greater certainty and to a higher degree, and that, too, without in any way affecting the system generally.

I gladly avail myself of this opportunity to acknowledge my obligations to Professor Balfour, for the very liberal manner in which he supplied me with the materials for prosecuting my researches, and to Dr Grainger Stewart and Mr Burns Thomson, for having procured for me the Calabar beans with which I commenced my experiments.

15*th May*.

[Since writing the above, I have read with great interest Mr Soelberg Wells's paper in the *Medical Times and Gazette* of 16th May, which contains an account of the sensations experienced by Mr Bowman after the introduction of a minute quantity of the extract of the ordeal bean into one of his eyes, and the details of the case of paralysis of the ciliary muscle and sphincter pupillæ, in which Mr Wells employed the Calabar bean. It is very gratifying to find so accurate an observer as Mr Bowman confirming my views regarding the properties and action of this remedy when applied to the eye.

In Mr Wells's patient the paralysis was only present in one eye, and thus it afforded opportunity for comparing the effects produced by the bean on the healthy and on the paralyzed muscles. The chief points of difference noticed were, that the effects produced were less marked and of shorter duration in the paralyzed than in the healthy eye, and that the paralyzed pupil did not act under the influence of light as the healthy one did.

A very interesting condition, noted by Mr Bowman, was that of astigmatism, causing indistinctness or blurring of horizontal lines, while vertical lines appeared sharp and distinct. This was remedied by a concave cylindrical glass of fourteen inches focus suitably placed. In connexion with this point, and more particularly with a suggestion I may throw out as to its probable causation, I would draw attention to the heart-shape of the pupil noted by Mr Wells at one period during the action of the bean on his patient's sound eye. Now, we must consider the action of the bean, in these cases, as purely local, as the agent does not act by being absorbed into the blood, and thus through a general affection of the system, as is the case when subcutaneous injection of morphia is employed for the same purpose. The amount of the bean, sufficient to produce myopia and myosis upon local application to the conjunctiva, would produce no effects at all if swallowed or injected below the skin. Hence, it is not difficult to conceive that the astigmatism (due to an uneven curvature of the lens) and the heart-shaped pupil may both arise from the greater stimulant effect produced by the agent upon that part of the eye on which the drop is placed. The base of the heart-shaped pupil, Mr Wells informs us, was above; this is just what we would expect upon the above theory, as eye-drops are generally placed upon the surface of the lower eyelid, or inferior portion of the eyeball, which would cause a greater contraction of the lower portion of the sphincter pupillæ than the upper. In like manner we might explain the astigmatism by an increased action of the lower part of the ciliary muscle. This theory I merely bring forward as a suggestion to explain the above phenomena.

Mr Wells concludes his able report by recommending periodic applications of a very weak solution of the bean in cases of paralysis of the pupil, and accommodation so as to "excite the sphincter pupillæ and ciliary muscle to gentle contraction without fatiguing and weakening them by over stimulation." In this I thoroughly concur, viewing the action of the bean in such cases, as analogous to electricity applied to paralyzed limbs, except that, in the former case, the effect is produced through the nerves; whereas, in the latter, it appears to be by stimulation of the inherent property of contractility in muscle. In either case one great advantage to be looked for from their use is the prevention of the muscular degeneration which inevitably occurs in muscular tissue that from paralysis or other causes is not employed. Care must be taken, as Mr Wells points out, not to overstimulate, which would fatigue and weaken, as I myself can testify with regard to my left eye, which has not yet perfectly recovered from the action of solutions of atropine, daturine, and the Calabar bean.

I perceive that Mr Neill's remarks on the Calabar bean are given more in detail in the *British Medical Journal* of 14th May, in which he refers to the use of glycerine for suspending the spirituous extract.]

4 MAITLAND STREET, 18*th May* 1863.

# Part Second.

## REVIEWS.

*Consumption: its Early and Remediable Stages.* By EDWARD SMITH, M.D., LL.B., F.R.S., Assistant-physician to the Hospital of Consumption and Diseases of the Chest, Brompton, etc. London: Walton and Maberly: 1862.

DR SMITH has fairly earned the reputation of being one of the most careful and laborious physiological observers of the present day; and some time ago we took occasion to introduce him in this capacity to the notice of our readers.[1] But Dr Smith is not merely a physiologist, he is also a practical physician; and at the Brompton Hospital his attention has been specially directed to the subject of consumption. Accordingly, he now comes before us as the author of a work on this disease,—a disease which, from its prevalence and its character, is, more than any other, worthy of the best attention of the medical practitioner. Dr Smith's object in the present volume has been to produce a practical work, which, founded on the inductive method, and on the improved physiology and pathology of the present day, should convey a faithful picture of cases of consumption seen at an early period, while the disease is still remediable, should enforce the principles upon which a successful treatment must be based, and should inform his readers of the means by which that treatment is to be carried out. We have read the book with much interest, and have no hesitation in saying that the object of the author has been fully attained.

Dr Smith's work consists essentially of two parts,—the first treating of the nature and evidences of the early stage of phthisis; the second, of the treatment which is to be opposed to it. These two divisions constitute the great bulk of the volume; but they are preceded by a section containing an historical sketch of the views entertained at different times regarding the nature and treatment of the disease, and are followed by a short chapter on the circumstances which modify the prognosis in particular cases. Like all Dr Smith's writings, the volume contains abundant evidence of careful research and patient investigation; the results of observation are then subjected to a rigorous scrutiny; and if we do not always concur in the conclusions arrived at, this is not so much owing to any distrust in the logical faculties of the author, as to the circumstance that, the evidence on many points being still incomplete, it is calculated to impress different minds in a different manner. The work is one which we can strongly recommend to our readers, more particularly as, from its entering largely into details, it would be impossible for us, in any reasonable limits, to follow the author through the path he has chalked out for himself.

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[1] See review of his work on "Health and Disease, as Influenced by the Daily, Seasonal, and other Cyclical Changes in the Human System," in the Number of this Journal for June 1862.

The first section of the work is, as already stated, introductory and historical; and we may quote the last four of the fifteen statements or propositions, in which Dr Smith endeavours to sum up the differences between our present knowledge of consumption, and the opinions entertained regarding it in former times.

> "12. There is an impression that, within a few years, the effect of modern treatment has been to prolong life during the attack of the disease; and our distinguished colleague, Dr Williams, has felt himself justified in saying how long it is increased. There are also grounds for believing that a cure is sometimes effected; but the mortality referred to, and the general feeling of professional and non-professional people alike, is that the disease is a fatal one.
>
> "13. The general principles of treatment and the methods employed are still the same as in former ages (due allowance being made for the separation of the particular class of cases which we are now able to effect), except that now bleeding is not resorted to, and the connexion with catarrh is not necessarily admitted, although we fully admit the influence of a cold in developing the disease. That there has been a change in the aspect of the disease at various æras is highly probable, according to the habits and prevailing constitution of the age, and hence the former practice and views might then have been as little incorrect as ours are now.
>
> "14. There has been but little variation in the remedies employed in all ages, as we have seen that milk, cream, eggs, meat, vegetables, exercise, open air, sea voyaging, change of climate, and adaptation of temperature and dryness of the air, steel, bark, olive oil, mutton suet, etc., have been heretofore employed. There have been the selection and rejection of minor remedies as emetics, digitalis, hydrocyanic acid, etc., with every age, and the leading treatment of this day has been anticipated, although not in the precise form of cod liver oil.
>
> "15. Hence as a final expression we may state that, whilst we have a selected class of cases, with an improved diagnosis and pathology, and these improved views are so largely shared by all practitioners, that the treatment is now similar in all parts of the kingdom, we are not agreed as to the essential nature of the disease, have no unfailing mode of treatment, and the disease is still essentially and almost as universally a fatal one as it has been in all ages."—Pp. 29, 30.

In these conclusions, Dr Smith, in our opinion, expresses too unfavourable a view regarding the incurability of phthisis, and the inefficiency of treatment. That cures not unfrequently take place cannot be doubted; that these cures are now more frequent than formerly is in the highest degree probable, although, from the obscurity which, till the discovery of Laennec, enveloped the diagnosis of phthisis, the matter does not admit of direct proof; and if this be so, it is illogical to conclude that treatment has nothing to do with the more favourable results. But neither do we agree with Dr Smith that "there has been but little variation in the remedies employed in all ages." The *supporting* treatment of consumption is, as a general plan of treatment, peculiar to modern times; there is no doubt that there have at all times been physicians who have recommended and practised this system, but these have been the exceptions; and though nourishing diet in the later stages of the disease has generally been allowed, it has not been until the supposed inflammatory symptoms of the early period had been combated by bleeding, purging, and other antiphlogistic remedies. It would be easy to accumulate testimony in support of this statement; but we shall content ourselves by adducing extracts from the writings of two of the most philosophic physicians of this country,—Sydenham and Alison.

In the *Processus Integri*, when speaking of the description and cure of a decline, Sydenham says:—"The treatment of the disease is, in my mind, as follows:—Bleed in the first instance, then purge for three days running, either with the *pil. coch. maj.*, or with the lenitive decoction already noticed. On the third night, give half-an-ounce of the syrup of poppies. After an interval of two or three days, according to circumstances, repeat the purge, and do this as often as is necessary, *i.e.*, until the symptoms either wholly or partially disappear. After each purge, give two drops of opobalsam on a large lump of sugar;"[1] and so on. It is true that riding on horseback is strongly recommended, but not until bleeding and purgation have been premised.

Dr Alison, in summing-up the indications for the treatment of phthisis, commences with the following recommendation:—"Repeated but cautious use of the antiphlogistic remedies, small bleedings, purgatives, blisters, and other counter-irritants, when the symptoms indicate attacks of 'intercurrent inflammation.'"[2]

Another point in this preliminary sketch is worthy of notice. Till lately, phthisis was regarded as a disease which commenced by the deposition of tubercle in the lungs, and it was believed that all the symptoms of the malady were due to the presence of this substance, and to the progressive changes which it underwent. Lately, however, an opinion has gained ground that conditions exist anterior to this deposit which have such a connexion with phthisis, that, if continued, the deposition in the lungs commonly appears. Some look upon these conditions as necessarily pre-existent to the deposit, and hence regard them as constituting a definite stage of the disease; others regard them as predisposing causes, and as indicating that there is a tendency to tubercular deposit. Dr Smith's own view is that "there is a stage of the disease which exists before the deposition of tubercle, and that its evidences are to be found both in the lungs and the general system."

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[1] Sydenham's Works; Syd. Soc. Ed., vol. ii. p. 298.

[2] Outlines of Pathology and Practice of Medicine, 1844, p. 289.

Now, that a peculiar train of symptoms does generally precede the local manifestations of tubercle, we do not doubt; the question is, whether or not this stage is specific, that is to say, whether it is preliminary to tubercle and to nothing else. If we look upon this stage as specific, we must suppose that it depends upon some peculiar lesion of nutrition, which we should naturally look upon as located in the blood. But Dr Smith himself admits (p. 157) "that there is no evidence of any known condition of the blood necessarily or even usually found in phthisis." If the stage is specific, and yet does not depend upon a change in the blood, in what can it consist? For our own part we should regard this pretubercular stage as non-specific, that is, as not necessarily preceding tubercle, but as a condition of generally impaired nutrition and constitutional weakness, which is not unlikely to be followed by that deposit. Tubercle we do not believe to be directly deposited from the blood, but all causes of local or constitutional debility predispose to its formation. What is called the tubercular dyscrasia is not established until tubercle has been deposited; and we would not call this the first stage of phthisis, because we do not believe that it necessarily precedes, or is inevitably followed by, the development of that disease. The question is of more importance pathologically than practically, for almost all modern observers are agreed that confirmed phthisis is generally preceded by a period during which various symptoms, mostly of debility, are present, without there being any distinct evidence of disease of the lung, and the treatment will be the same whether this period be regarded as specific or not. Those who look upon it as the first stage of phthisis, and those who do not, both regard it with anxiety, especially if there be any hereditary predisposition, as likely to be followed by the manifestation of the disease.

Having discussed these preliminary matters, Dr Smith passes on to the consideration of the nature and evidences of the early stage of consumption. The author first gives the result of his investigations regarding the kind of constitution most liable to be affected, as indicated by the temperament, the colour of the eyes and hair, the complexion, and the habit of body. Though the inquiry has been conducted on a large scale, embracing a thousand individuals, no very definite results are arrived at. Dr Smith's own conclusion is expressed in the following terms:—"As a general expression of the whole, it may be stated that there is the greatest diversity in all the points of inquiry in phthisical persons in this country, but there is a preponderance of persons with grey eyes, florid complexion, fleshy habit of body, and excitability of temperament. Whether, however, they differ in these respects from the community at large has not yet been determined." It may, however, be stated that, in regard to the hair, instances of extreme colour (black on the one hand, flaxen on the other) were few, and that in the case of the eyes, while 74 per cent. had grey, and 23 per cent. had hazel, not 1 per cent. had black eyes.

The same system of minute inquiry is carried out in regard to the performance of the different functions of the body, by persons in this early stage of the disease. As was to be expected, the general result is, that at this period the various functions are carried on less perfectly than by individuals in a state of health. We quote a few of Dr Smith's results—"The whole of the processes concerned in the function of alimentation are commonly lessened in vigour." "The amount of food taken is commonly somewhat lessened." "The weight and bulk of the body are almost universally lessened." "The fixation of fluid in the body is lessened, and the elimination of it increased." "The action of the skin is commonly increased, either absolutely or relatively to the vital transformation." "Perspirations are common in the early, as in the later stages of phthisis, and oftentimes have a sour odour." "There is a general tendency to defect of the temperature of the body." "The muscular power is commonly lessened." "The circulation is commonly enfeebled and somewhat quickened." "The respiration is shorter, shallower, feebler, and, perhaps, quicker." The vital capacity of the lungs is diminished, even when there are no evidences whatever of the presence of tubercular deposits." "Innervation is commonly lessened." "The menstrual function is frequently disturbed, but probably not in a greater degree than occurs in health. There is much liability to leucorrhœa." These and a variety of other statements regarding the mode of performance of the functions of the body, are deduced from numerous and careful observations, but for the facts on which they are based, or by which they are illustrated, we must refer to Dr Smith's work. In regard to the appetite, however, it may be stated that Dr Smith has discovered that, in the early stage of consumption, patients show a decided dislike to fat, that this dislike is more common in phthisis than it is in other diseased conditions, and that it is more marked in the early than in the subsequent stages of the disease.

Dr Smith, in the next place, directs attention to certain abnormal manifestations which not unfrequently precede any evidence of the existence of tubercle in the lungs. Thus, muscular pains are common both in the front and back of the chest. There is generally some degree of tickling or irritation of the throat due to hyperæsthesia of the pharynx, which, on examination, is sometimes found pallid and contracted, at others a little congested, with the superficial glands enlarged and vascular. Cough is generally inconsiderable, short and repeated a few times at each attack, and appears to be due rather to the state of the pharynx than to the condition of the lungs. At this time, too, expectoration is scanty, and is chiefly derived from the fauces and pharynx.

Dr Smith's views regarding hæmoptysis are somewhat peculiar. Whilst admitting that, at some period of the disease, two-thirds of all phthisical patients have spat blood, our author maintains that the quantity is generally very small, amounting to a streak or speck in one, or only a few masses of the sputa, that it is quite exceptional for any large amount to be expectorated, and that the quantity lost is too small to have the least significance. With regard to the seat of the hæmorrhage, Dr Smith considers it in the highest degree improbable that it proceeds from the lungs; he believes that it comes from the pharynx, which, as already stated, is often in an irritable condition, and that even large emissions of blood may occur without any inflammation or marked local congestion of any part of the lungs, and be due to the rupture of a small vessel upon the mucous lining of the pharynx or large air-tubes. Dr Smith consequently regards hæmoptysis, not as showing that mischief has already commenced in the lungs, but as indicating a condition of system in which phthisis will most probably occur. The frequent coexistence of hæmoptysis and phthisis is explained in the following manner,—hæmoptysis, to a trifling extent, occurs in almost all persons, however healthy, at some period of life, generally owing to some slight affection of the pharynx, with congestion of the mucous membrane; phthisis is the most prevalent of all mortal diseases, therefore it would be remarkable if the two conditions were not present together, "although it is quite within belief that the local causes of hæmoptysis may be more potential in certain persons than in others, precisely as we find variation in the influence of all agencies whatever." With these views we cannot, for various reasons, concur. We would not designate as hæmoptysis the presence in the sputa, on a solitary occasion, of a mere streak of blood; and if we did, we should cease to attach any importance to its occurrence, for, even on Dr Smith's own admission, this frequently happens to the healthy, as often, for anything we know to the contrary, as to the phthisical. In most cases of phthisis, according to our experience, a sufficient amount of blood has been expectorated to attract the distinct attention of the patient, either by the emission of a certain quantity of pure blood, or at least by decided staining of the sputa amounting to more than a mere streak or speck. We have often found distinct evidence of the existence of tubercle to coincide with the presence of hæmoptysis; and all observers must have noticed the marked relief to dyspnœa and sense of constriction of the chest which the expectoration of a moderate quantity of blood often affords.

We much more nearly concur in the following statement by Louis regarding the diagnostic value of this symptom:—"For all these reasons, I am of opinion that hæmoptysis, if it be somewhat severe, and have not occurred under the exceptional circumstances alluded to (namely, in the case of persons who have received a severe contusion of the chest, or of women whose catamenia have been suppressed), denotes with infinite probability, no matter what have been the period of its occurrence, the actual presence of some tubercles in the lungs. I do not say that it does so with certainty, for several cases, of the correct observation of which no doubt can be entertained, appear to constitute fortunate exceptions to the general rule."

The next source of evidence of the existence of the early stage of phthisis is the examination of the lungs by inspection, auscultation, and percussion. Without entering minutely into a consideration of the physical signs, Dr Smith makes some interesting observations regarding them, and we shall state his conclusions in his own words.

> "1. When there is less breath-motion, less length of inspiration, and feeble yet tolerably even vesicular sounds, both with ordinary and forced respiration over the whole chest, or particularly at one or both apices, with or without slight dulness on gentle percussion of the clavicles, and without rales or any sign of bronchitis, we believe that there is the early or the pretubercular stage of phthisis.
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> "2. When there is dulness at least moderately pronounced and localized, and prolonged expiration, with, but sometimes without, flattening of the chest at the part affected, and with or without wavy or jerking respiration, and with unevenness of the respiratory sounds at the part affected, in addition to the signs of the first stage, and still without rales or other evidences of bronchitis, we consider that tubercle is deposited, and the disease in that part has passed into the second stage.
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> "3. When general bronchitis is also present, the diagnosis from the examination of the chest is almost impossible, and whilst the progress of the general symptoms may aid us, a correct opinion can only be formed after the signs of old bronchitis have disappeared, or the general evidences of phthisis both in the lungs and system have increased.
>
> "4. The state of the general system is substantially the same, whether before or soon after the first deposition of tubercle, but the degree of variation from health will have increased in the latter with lapse of time. Hence, whilst this state must be considered in forming our diagnosis of phthisis, it is equally indicative in the two stages, except perhaps in degree."—P. 183.

The next division of Dr Smith's work is devoted to the treatment of the early stage of consumption; it contains a great deal of valuable matter, but our notice of it must be necessarily brief. The great object in the treatment is to restore the bulk of the body by diminishing excessive elimination on the one hand, and increasing the supply on the other. The first indication is chiefly fulfilled by moderating the action of the skin, kidneys, and bowels; the second by a due regulation of the diet. Where excessive elimination of fluid takes place by the skin, Dr Smith recommends the inunction of some fatty matter, giving the preference to lard as less offensive than the liquid oils. Dr Smith does not believe that the fat is absorbed, but thinks that it acts mechanically by covering the pores of the skin, thereby restraining its action, and offering a certain protection against external agents acting upon a highly sensitive structure. For our own part, we cannot help believing that a portion of this oily matter is absorbed, as we see, under its use, persons increase in bulk to a degree which cannot be explained by a mere diminution of elimination. The application of cold to the skin is strongly to be recommended, as tending powerfully to remove the atonic and relaxed condition which favours undue perspiration. It is best practised by dipping a towel in water at about the temperature of the room, in which common salt has been dissolved, and quickly applying and reapplying it to all parts of the body; the surface is then to be thoroughly and rapidly dried with a coarse towel.

A proper regulation of the diet is perhaps the most important point in the treatment of consumption. Dr Smith believes that in the early stage of this disease not only is the appetite diminished, but that there is an absolute defect in the quantity of food taken, and that in particular the nitrogenous element is commonly deficient. There is a special danger of a deficiency of nitrogen in the case of adolescents when a change of dietary takes place, the diminished quantity of milk not being compensated for by a proportionally increased quantity of other nitrogenous food. Of course this danger is greatest among the poor, but it exists even among the rich, in whom tea or coffee, containing scarcely any nitrogen, constitutes with a little milk a great part of the breakfast, which formerly consisted of milk containing thirty or forty grains of nitrogen. The dietary recommended by Dr Smith is calculated to supply between three and four hundred grains of nitrogen, and is thus stated:—

"Three pints of milk; 1¼ lb. of bread; 6 oz. of uncooked meat (equal to 4 oz. of cooked meat); ¾ lb. of potatoes; 1½ oz. of butter and 3 oz. of uncooked bacon, or 4 oz. of eggs. This would give the following quantity of nitrogen:—

| | |
|---|---|
| 3 pints of milk, new and good, grains | 132 |
| 6 oz. of meat (fat and lean) | 48 |
| 20 oz. of bread | 155 |
| 12 oz. potatoes | 9 |
| | 344" |

In many cases the patient cannot take so much food, and under these circumstances Dr Smith recommends the use of glutenized food, especially of gluten bread, one ounce of which contains as much nitrogen as seven ounces of ordinary bread.

To promote the assimilation of food, exercise is of the greatest importance; under its use there is an increase in the metamorphosis of the tissues and in the final transformation of food; and if the quantity of food-transformation be in excess of the tissues wasted, a further amount of tissue will be deposited. Walking is, on the whole, the most suitable mode of taking exercise; but in reference to women, Dr Smith strongly recommends skipping, as allowing considerable exertion to be made in an easy manner. The respiration, and consequently the free expansion of the lungs, may also be powerfully promoted by causing the patient to practise daily, for ten minutes at a time, the taking of deep voluntary inspirations. This is attended with a rapid increase in the expansibility of the chest, and with an invigoration of the whole vital actions of the body.

In reference to the use of medicines in this disease, we may quote the following passage :—

> "In perhaps every case it is necessary to administer the mineral tonics: 15 minims of the Tinct. ferri sesquichloridi, with or without an equal quantity of chloric ether, should be taken thrice a-day in a wineglass of water, wine, beer, or quassia infusion; or 8 grains of the citrate of iron and quinine thrice a-day. The iodide of iron, when given, should be frequently intermitted. Cinchona and the mineral acids may be exchanged at intervals for the preparations of iron. Two or three drachms of cod-liver oil should be given twice or thrice a-day, when it is tolerated. The condition of the menstrual function, the digestion, and the alvine evacuation, will all need frequent attention. The best internal remedy for the cough is morphia in doses of 1-16 or 1-12 of a grain, taken every four or six hours, with mucilage or syrup. Frequent examinations of the state of the pharynx should be made; and this, with the state of the stomach and skin, will determine whether the acid or the alkaline plan of treatment must be temporarily pursued."—P. 336.

The remainder of the section on the treatment of phthisis is devoted to a consideration of the management of certain special symptoms, such as muscular pains and hæmoptysis, and concludes with some interesting observations on the therapeutical effects of climate.

We must now take leave of Dr Smith, expressing, in conclusion, our high sense of the able manner in which he has investigated the symptoms of the disease, and of the talent with which he has directed his physiological and pathological knowledge to its treatment.

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*Traité des Maladies à Urines Albumineuses et Sucreés, ou de l'Albuminurie et du Diabète Sucré dans leurs rapports avec les Maladies.* Par le Docteur J. ABEILLE, Ancien Médecin de l'Hopital du Roule, Chevalier de la Légion d'Honneur, etc., etc.

*Treatise on Diseases with Albuminous and Saccharine Urine, or on Albuminuria and Diabetes in their relation to Diseases.* By Doctor J. ABEILLE. Paris: J. B. Baillière and Son. Pp. 773: 1863.

THE author of this work is already known to have devoted considerable attention to the important subjects of which it treats. Several brief memoirs by him on Albuminuria and on Saccharine Urine have during former years appeared, and lately his investigations concerning the urine in Diphtheria have gained pretty general diffusion through the value assigned to them by so distinguished a writer as M. Trousseau.

The present extended work of M. Abeille is divided into two books: the first being occupied with albuminuria, or rather, to use his own words, with those diseases in which the urine becomes temporarily or persistently albuminous; while the second, comprising sixty-five only of the total seven hundred and seventy-three pages of which the entire volume consists, treats of those diseases which are associated with a saccharine condition of the urine.

The first book commences with a sufficiently interesting historical chapter. The discovery of albumen in the urine is assigned to Fordyce, who, in his Elements of Pathology, published in London in 1768, remarked that the chyle, serum, and coagulable lymph, may be removed from the system by the urine. A greater merit, however, belongs to Cotugno, who was the earliest to associate the presence of albumen in the urine with certain cases of dropsy. Several authors after these and before Dr Bright, paid attention to the coagulable condition of the urine, specially Blackall, and thus prepared the way for the really brilliant discovery by which the name of the former physician has been rendered illustrious. We have no great fault to find with M. Abeille's historical introduction; nevertheless, some of his statements in it are very far from being accurate. He says of one author's paper, on Temporary Albuminuria, that it was published in the Monthly Journal for 1858, the fact being that the memoir in question appeared in 1852. This circumstance, indeed, would scarcely require notice, were it not that M. Abeille charges the writer with claiming a priority of observation respecting the albuminous condition of the urine in certain cases of erysipelas. We have consulted the paper referred to, and have now to state that no such claim is advanced—the whole is a creation of M. Abeille's own too fertile brain,—and it is scarcely creditable when we read a couple of lines further down, "La vérité est, qu'en 1849, nous l'avions devancé et que nous avions insisté sur la présence de l'albuminurie dans les érysipèles vastes, fait non signalé avant nous."

The historical sketch is succeeded by four chapters, which are respectively occupied with the consideration of *the normal structure of the kidneys*, the *physiological functions* of these organs, the *condition of the urine* in diseases attended by albuminuria, and the *pathological anatomy* of Bright's disease. There is nothing novel in any of these chapters; indeed, the information contained in them falls far short in interest and exactness of what is to be found in the pages of several older but still highly esteemed works. M. Abeille is not always circumspect in the choice of the language he employs. We object to his speaking of so distinguished an authority on renal diseases as Dr George Johnson in the following terms:—"Pour arriver à cette dénégation, il a recours à un subterfuge." Having carefully perused the passage in Dr Johnson's work to which M. Abeille refers, relating to the amount of importance to be assigned to the presence of renal epithelium in the urine, we have no hesitation in saying that there exists no evidence whatever of the author having had recourse to an *evasion*, his meaning and the explanation afforded are alike clear and comprehensive. If obscurity exist, it is to be found in the statements of those writers whom M. Abeille approvingly quotes.

Arrived at page 147, M. Abeille addresses himself to the real subject of his volume, Albuminuria in its relation to different diseases,—considering, first, those affections in which the presence of albumen in the urine is temporary or transient, lasting usually for only a few days. *Albuminurie passagère.*—Under this head, reference to albuminuria in the following diseases occurs: 1. *Neuralgia*, illustrated by one, assuredly by no means remarkable instance. 2. *Diseases of the respiratory organs*, among these *Phthisis*, of which an illustration is afforded of albuminuria alternating with hæmoptysis; also *Pneumonia* and *Bronchitis*. In the case of the former, M. Abeille styles the albuminuria which occurs as critical, "Albuminurie critique," and justifies the employment of this expression by stating that the phenomenon in question invariably manifests itself during the period of declension of the pneumonia—the stage of resolution. We are rather surprised to find no reference made under this head to the observations of Finger and Becquerel. Nor can we find any proof of our author being acquainted with the really important results obtained by Heller and by Dr Parkes. The occurrence of albuminuria in acute pneumonia is a clinical fact of very considerable significance; for in ordinary acute bronchitis, and other inflammatory affections within the chest, we do not meet with it. Statements differ as to the period of the disease when the phenomenon is noticeable. Some affirm that it occurs during the advance of condensation, when, as Heller remarks, the chlorides are most deficient; others have found the experience of M. Abeille to be theirs, and have conjectured that some of the absorbed exudation is thus removed from the system. It is only by renewed careful examination that this important point can be determined.

In *Erysipelas* and *Hospital Gangrene*, M. Abeille has found a temporary albuminuria, not ordinarily in slight cases of the former, but in such as are distinguished by the extent of surface affected and by their severity.

*Diphtheria* is not unfrequently accompanied by an albuminous condition of the urine,—the precise frequency has not as yet been determined; indeed, it seems to vary considerably. M. Abeille has found it in two out of three cases in one epidemic prevalence, while at another season he has only seen it in one-third of those affected. Curiously enough there is no reference to the observations of Mangin on this subject. M. Abeille takes the same view of the albuminuria in diphtheria and analagous diseases as that entertained by M. Trousseau, regarding it as the direct consequence of the specific morbid condition of the blood, which is believed to exist in such affections.

Under the same head of *Temporary Albuminuria*, we find observations on the urine in *Typhoid* fever, and *Typhus*, and in *Paludal* Fevers, finally, in *Cholera*, and a few other diseases.

*Persistent or Lasting Albuminuria.*—" Albuminurie persistante ou durable," as opposed to temporary albuminuria, is next considered. This condition is discussed at length in relation to the occurrence of a coagulable state of the urine in *Scarlatina* specially, and in *Bright's* disease. The author assails the opinions of previous observers, but does not contribute much, if anything, to our knowledge of this important subject.

In the *second* book, occupied with *Diabetes*, our readers, familiar with the most recent researches, particularly those of Dr Pavy,[1] will not find much to engage their attention.

## *Systematic Handbook of Volumetric Analysis.* By FRANCIS SUTTON, F.C.S. London: Churchill and Sons: Pp. 282.

As volumetric analysis means a mode of estimating the quantities of substances, the subject of Mr Sutton's handbook will have more interest to the pure chemist, than to the members of the medical profession generally. As distinguished from the older methods of analysis, the volumetric plan proceeds by saturating, or oxidizing, or precipitating, the constituents of the substance to be analyzed, by means of a solution whose strength or chemical power has been previously ascertained, so that the same volume or measure of it will always effect the same amount of saturation or precipitation. A solution so prepared was formerly called a *standard* or *normal* solution, but to convey the notion of its use more accurately, it is now called a *titrated* solution, from the French word *titre*, strength or power. Such solutions have been in use in British laboratories for many years, but only to a limited extent. The later editions of such works as that of Fresenius's Quantitative Analysis have, however, been making us more familiar with volumetric methods of analysis; and medical readers have been well introduced to these by Thudicum's excellent work on the Pathology of the Urine. But up to the present time we have had only two works in English specially devoted to volumetric analysis, and that of our author is the fullest and best of these. He is, therefore, worthy of all praise in supplying this deficiency in our chemical literature. Mr Sutton does not profess to give original methods of analysis, preferring, as he says in his preface, " to adhere mainly to those processes which have received the approval of general experience." But his book is none the worse on that account, as it is best that our analytical manuals should chiefly contain those processes which are known to be accurate and trustworthy. We approve also of our author's practice of candidly pointing out when a process is not thoroughly to be relied upon.

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[1] See review in our January number.

The great recommendation which volumetric methods of analysis have over the older ones, is the speed and accuracy with which they can be performed, an advantage which must be as acceptable to our readers who are occasional analysts, as to those to whom analysis is a constant profession. Our author has made volumetric analysis all the more acceptable to the English chemist, by placing before him a plan by which the grain weight, and measures which are related to it, may still be used. In other works where volumetric processes are given, these are all stated in the French metrical system of weights and measures, which, being too slowly adopted in Great Britain, needs the constant use of calculations to reduce its values to the standards we have in use in this country; so that a plan which dispenses with such calculations is generally to be preferred.

Mr Sutton divides his work into five parts, the first of which he devotes to the description of the few pieces of apparatus required; and here he has introduced some neat and accurate woodcuts. The other subjects treated of in Part I. are chiefly preliminary,—as the preparation of titrated solutions, etc. Parts II., III., and IV. are respectively devoted to the description of processes for analysis by saturation, by oxidation and reduction, and by precipitation. Part V. is taken up with the details of methods showing various applications of volumetric analysis. Amongst these occurs a valuable section on the analysis of urine,—the processes contained in which we can from practical experience recommend as worthy of all confidence. Under the head of the determination of urea, Liebig's process only is given. We think an omission has been made in not including Davy's method also, as it is clearly a volumetric process, and has the advantage, moreover, of being easy in execution, and accurate in its results.

In treating of the various subjects contained in the five parts of his work, Mr Sutton has put them before his readers in a clear and concise way. We have, therefore, great pleasure in recommending this Handbook of Volumetric Analysis to the English student of analytical chemistry.

## *Sixteenth Annual Report of the Chinese Hospital at Shanghae, for the Year* 1862.

We have been favoured with a copy of the Sixteenth Annual Report of the Chinese Hospital at Shanghae, at present under the care of Dr James Henderson. He received his education under the auspices of the Edinburgh Medical Missionary Society, went out to China a few years ago, and conducts the institution avowedly on medical missionary principles. The hospital has recently undergone considerable improvements, and now affords accommodation, in six wards, for thirty in-door patients, and the requisite number of attendants. Dr Henderson's house is close by, so that his services are available at any hour, day and night. His principal assistant and the hospital native preacher have rooms for themselves and their families above the wards in one wing of the building. There is a hall capacious enough for the reception of 300 out-door patients. The surgery is at the western extremity of this apartment, and in it all the medicines are prepared, and the minor surgical operations performed, while operations of a graver character are performed in the middle of the hall.

At *eight* A.M., there is a short religious service, intended for the attendants and all the in-door patients who are able to be present. At *ten*, the out-door patients begin to assemble, and at *twelve*, there is reading of the Scriptures and sermon by a native preacher and a member of the London Mission. At *one*, the surgeon begins his work. We may form some idea of its onerous nature, when informed that the number of patients, in 1862, was 33,253, or, in other words, not counting Sundays, more than 100 *new* patients on an average every day. The work is overtaken thus. Ten patients are admitted into the surgery at a time—men and women alternately—and each of these is taken aside, examined, and prescribed for, while the others wait. Cases requiring operations of any moment are reserved until the others have been seen, and afterwards remain for a time as in-door patients. The in-door patients are visited and attended to immediately after the out-door ones have been examined. As a matter of course, cases of accident or acute disease are admitted and cared for at all hours.

The following details show still more clearly, than the mere number of patients, the amount of benefit which the institution is capable of conferring. During the last four months, there were 52 operations for artificial pupil; 25 for entropion; 5 for cataract; 17 for pterygium, and 4 for glaucoma; 29 bullets were extracted; 2 tumours excised; there was 1 amputation; 18 cases of tapping; 28 fractures of the upper extremity, and 18 of the lower; there were 6 cases of stricture of the urethra, and 9 operations for hæmorrhoids; besides a large number of the ordinary minor surgical operations.

It is obvious that much substantial benefit must, in this way, be conferred upon the native community both in town and country, as regards their bodily health; and we cannot doubt, reasoning from similar experience in other parts of the world, that the great truths of the Christian religion, which, as we have seen, are systematically proclaimed, will be daily listened to by many grateful and attentive hearers; for it must be allowed, that no one possesses such favourable opportunities for impressing these truths upon the heathen as the medical missionary.

# Part Third.

# PERISCOPE.

## MIDWIFERY.

### ON THE INFLUENCE OF VICARIOUS MENSTRUATION ON OVULATION. BY DR PUECH.

Dr Puech has communicated to the Academy of Sciences, Paris, a paper of which the conclusions are the following:—

1*st*, Vicarious menstruation, supplemental hæmorrhage, is said to exist, when at the regular periods a flow of blood takes place elsewhere than from the genital organs.

2*d*, All parts of the body may be the seat of these hæmorrhages; nevertheless, they occur more frequently in some localities than in others; thus, in Dr Puech's cases, they were met with, from the stomach 32 times, from the breasts 25 times, from the lungs 24 times, from the nasal mucous membrane 18 times.

3*d*, In all cases which have been carefully observed, antecedents either of hysteria, or of an exaggerated nervous sensibility, have been noticed.

4*th*, In general the menses have been absent (183 times), but sometimes (15 times), at the very time of occurrence of the vicarious menstruation, a slight flow of blood has been noticed.

5*th*, The genital organs are generally healthy, although they have sometimes been found abnormal. In 11 cases there was atresia, either congenital or accidental.

6*th*, With the exception of these last-mentioned cases, absence of the menses does not imply sterility; except in the case of grave disorders, ovulation continues to take place, and the rupture of the Graafian vesicle corresponds with the period of vicarious menstruation.

7*th*, Pregnancy, accordingly, is possible, and has been observed. It suspends the deviation for the time, although it may reappear after the confinement, or at the cessation of nursing.

8*th*, Although compatible with health, and occasionally existing for the whole period between puberty and the time of cessation of menstruation, the deviation is a pathological condition; nay, more, it is not free from danger, having occasionally been the cause of death.—*Gazette des Hopitaux*, April 21, 1863.

### CASE OF PRESENTATION OF THE RIGHT ARM AND SHOULDER. DELIVERY BY THE NATURAL POWERS, OR SPONTANEOUS EVOLUTION. BY DR RICHARD HODGES.

On Sunday, the 8th of January of the present year, I was summoned to visit a lady in labour with her fourth child. At eleven o'clock on that day, the os uteri was found fully dilated, and the membranes protruding through it, distending the vagina; but no presentation could be ascertained. During a pain the liquor amnii escaped, and no more uterine action occurred throughout that day, the following day, Monday, or the next, Tuesday, until towards the evening, when, on account of slight pains, I made an examination, and found a hand high up presenting. The patient was cheerful throughout this interval, sat up the greater part of each day, felt as if nothing was the matter with her, and considered it strange that the process of labour should have been so suspended. The pains, however, did not forget to return; they kept returning and increasing in intensity, and with their return the presenting part was more and more easily recognised. Ascertaining that the right hand, arm, and shoulder presented, and believing it unlikely that delivery would take place without assistance, I cautiously introduced my hand into the uterus, with the view of bringing down the feet; but I was unable to do so, from the uterus firmly embracing the fœtus, and preventing my hand passing on far enough to seize them. By the continuance of the pains, however, the presentation was forced lower and lower; the arm and shoulder now protruded at the pubis, and the side of the chest pressed out the perineum; the breech, by further efforts at this time, descended and turned forwards, and was expelled as in an ordinary breech presentation, the body of the child curving or bending to a sufficient degree to admit of delivery quickly taking place, the arm and shoulder all the while remaining under the arch of the pubis, and not receding. The head was now alone left, and soon extruded, and the whole process was over at two o'clock on the morning of Wednesday, and in less time than on previous occasions when the presentation was natural. The child was above the average size, and, there is reason to believe, had been dead several days.

The case just narrated is interesting, not only on account of showing the fact that nature is, for the most part, adequate to the end in view,—that she is the best and safest physician,—but also in exemplifying a mode in which expulsion under such circumstances takes place, and regarding which a diversity of opinion has prevailed; one supposing that the lower extremities descended during a pain, and made room for the upper, which ascended as the others came down, till, the body turning round on its axis, the breech was expelled, as in an original presentation of that part; while another, believing it impossible for the upper extremities to mount up into the contracting uterus, and that, consequently, no part which once protruded ever receded.—*Transactions of the Obstetrical Society of London for* 1862.

### CASE OF UTERO-PLACENTAL ADHESIONS WHICH GAVE RISE TO SEVERE SYMPTOMS.

BY DR DELORD.

THE questions of inflammation, adhesion, retention, and absorption of the placenta, are still enveloped in sufficient obscurity to authorize every practitioner to endeavour to throw light upon them by his personal experience. I may, therefore, be permitted to publish an observation which appears to me interesting, in reference to the above points.

In the month of May last, I was sent for by the wife of a notary, who was entering upon the ninth month of her first pregnancy. Although I could recognise no sign indicative of the commencement of labour, Mrs A. experienced dull constant pains in the loins and abdomen. Movement somewhat increased these pains, but not to such a degree as to render them insupportable. I prolonged my examination; and pressure, carefully and methodically exerted over the whole surface of the uterus, having failed to discover any sensitive point, I concluded that the seat of the pain was situated more deeply. Pressed by my questions the patient stated, that independent of the lumbar and abdominal pains, she experienced a peculiar dull pain, which was constant and was located in the right portion of the fundus of the uterus. This avowal removed all the doubts I might have entertained, and I considered myself justified in diagnosing an inflammation of the placenta, to combat which I prescribed repose, a light diet, and injections. Under the influence of this treatment Mrs A. felt better, but she still experienced an unusual sensation, a sort of weight and dragging in the region of the uterus. The result proved that, in referring this sensation to the existence of utero-placental adhesions, I was not deceived. On the 18th of the following June, Mrs A. was delivered at the full time of a healthy infant. Under any other circumstances I should have waited, according to the obstetrical rule I have laid down for myself, the natural expulsion of the placenta; but as in this case I had serious grounds for apprehension, I applied slight frictions over the uterine region, hoping in this way to give rise to some effectual contractions, and so to hasten the delivery. At the end of half an hour of vain expectation, I administered a full dose of the decoction of ergot. The effect of this soon seemed to manifest itself; but at the same time the patient turned pale, and I discovered that internal hæmorrhage had taken place. The cord, rendered friable by inflammation, gave way under the traction which I considered it right to employ without delay. I introduced my hand into the uterus from which a gush of blood took place; fainting fits succeeded one another, and the poor mother seemed on the point of death. In this critical situation I compressed with my left hand the abdominal aorta; then, by means of my right hand introduced deeply into the uterus, I separated all that part of the placenta which could be detached, pinching at the same time the inner surface of the uterus with a view to provoking salutary contractions. This manœuvre was crowned with success, and the patient was thus saved from the immediate danger of hæmorrhage. But if the condition of matters was now less alarming, the fact of the retention of a considerable portion of the placenta could not but inspire me with inquietude. A consultation was held, and the unanimous opinion was, that the sole indication for the present consisted in sustaining the strength of the patient, and in extracting the remains of the placenta by every possible means. The next day the pulse became rapid, and the abdomen became somewhat painful over the uterine region. During the following days a sanious and fetid discharge took place from the genital organs. There were occasional rigors, extreme prostration, and other signs of purulent absorption. The treatment consisted in the administration of beef-tea, sulphate of quina, and cinchona wine, and in the employment of detersive and disinfectant injections. As to extracting the remains of the placenta, the idea required to be abandoned, as the patient peremptorily refused to allow any such attempt to be made. Nevertheless, after a few days, a portion of the placenta was expelled naturally, when, for a second time, alarming symptoms threatened to compromise the life of Mrs A., which, however, yielded to the treatment previously employed. On the 14th of June a second fragment of the placenta was expelled. From that time the amelioration was progressive and uninterrupted. Convalescence was established in the course of the fourth week, and on the 30th of August I could convince myself that the health of Mrs A. was perfect.

In this case, as in another which occurred to me in 1840, absorption of the placental substance retained in the uterus by adhesions consecutive to plastic inflammation of the placenta, took place without accident. The symptoms of putrid absorption observed in the present case depended exclusively on the presence of fragments detached from the mass of the placenta, as was proved by the circumstance that, after the expulsion of the fragments, all the dangerous symptoms disappeared. From these facts we may conclude that resorption of the placenta is only possible when it has been united to the surface of the uterus by means of an organized plastic growth, and when a common life has been established between the two organs by means of vessels of new formation. Under these circumstances, resorption may take place without any bad symptoms. If, on the contrary, the placenta has separated in a mass, it is only susceptible of diminution by means of putrid decomposition, and then it acts as a foreign body and as a source of infection, giving rise to accidents which are almost always mortal.—*Journal de Médecine et de Chirurgie pratiques*, March 1863.

## BRONCHO-PNEUMONIA IN LYING-IN WOMEN. BY DR ROBERT BARNES.

I HAVE long been familiar with the fact that lying-in women are liable to a peculiar form of broncho-pneumonia. It is generally considered that the pulmonary symptoms which arise during childbed are the consequence of the violent straining attending the expulsive stage of labour, and of "taking cold" during the exposure sometimes incident to that event. To me this explanation is far from sufficient. As in typhoid fever, so in puerperal fever, the lungs are apt to be involved. In either case the cause is similar. I may here observe that a marked characteristic of typhoid fever is extreme alkalinity of the blood. The urine I have frequently found highly ammoniacal on voiding. A similar condition commonly marks the blood in puerperal fever. On one occasion I observed that the bladder, being partially paralyzed, and the urine consequently retained in the intervals of being drawn by the catheter three times a-day, the urine decomposed so rapidly in the bladder as to evolve large quantities of ammoniacal gas, which escaped in a stream with a gurgling sound when the catheter was introduced. These symptoms, with others which need not be enumerated, indicate a dyscrasia of the blood, which must produce certain irritating effects throughout the body. The diarrhœa of puerperal fever, and the diarrhœa which frequently appears in childbed, apart from overt fever, is the simple effect of the irritation of the intestinal mucous membrane by the septic or other offending matter circulating with the blood. Peritonitis, cellulitis of the limbs, synovitis,—all arise in the same way.

Accompanying this diarrhœa, or apart from it, we may have broncho-pneumonia. This, in like manner, is simply the effect of the irritation of the bronchial mucous membrane or parenchyma of the lungs by the same offending matter. In some of these cases the breath of the patient has possessed an odour distinctly resembling that of the lochial discharges. Broncho-pneumonia may, in short, be regarded as a symptom, or a part, of puerperal fever. But in many cases the fever is masked, or so slight that it escapes observation, and the attention is fixed upon the pulmonary symptoms alone.

In these cases there is generally a considerable degree of prostration. The whole or the greater part of the mucous tract of the lungs is involved. The expectoration amounts to bronchorrhœa. Large and fine crepitation are heard in almost every part of the chest. The sputa are sometimes tinged with blood. Depletion is not borne. The most successful treatment consists in the administration of cinchona, senega, or serpentaria, in the form of decoction or infusion, with ammonia, and at a later stage with nitrohydrochloric acid, blisters to the chest, good nourishment, in the form of strong beef-tea, eggs and milk, and a moderate allowance of wine or brandy.

The form of broncho-pneumonia I have thus sketched is not to be confounded with that which is the result of capillary embolia of the pulmonary arteries, although there is this affinity between them, that both are set up by offending matter brought to the lung-tissues by the circulating blood.

The importance of recognising this form of broncho-pneumonia will be admitted when its bearings upon childbed mortality are considered. In some statistical statements I have noticed that certain kinds of death are excluded, on the presumption that labour had nothing to do with the fatal result. Bronchitis and pneumonia are thus treated, although to the informed critic the deaths from these causes may be as plainly traceable to the puerperal process as are the deaths from peritonitis.

It is by the complication of this form of pneumonic irritation that I account for the fatal acceleration of phthisis after labour.—*Transactions of the Obstetrical Society of London for* 1862.

### ON THE TREATMENT OF PUERPERAL CONVULSIONS. BY PROFESSOR DEPAUL.

Chloroform having been for some time back enthusiastically recommended as a means of combating puerperal convulsions, M. Depaul has thought it right to enter into some details regarding the treatment of this affection, and to examine into the comparative value of the means employed, either with a view of preventing the occurrence of the disease, or of removing the symptoms which characterize it, and which threaten the life of the mother and the child. It may at once be stated that the successes attributed to chloroform have not converted to its employment the successes of Paul Dubois, and that in this respect the former and the present professor are at one regarding the superiority of bloodletting in the condition under consideration.

To prevent eclampsia during pregnancy, it has been recommended to prescribe bleeding and purgatives for robust, sanguine women, subject to cerebral congestion; baths and anti-spasmodics for those who are delicate and nervous; and diuretics for those whose urine is albuminous, and in whom there is serous infiltration of the cellular tissue. It is very difficult to determine the degree of value to be ascribed to these means, as in several cases where the prodromata of eclampsia have been well characterized, and where the treatment has been purely expectant, the attack has not taken place. Nevertheless, under these circumstances, Professor Depaul expresses himself in favour of bleeding, which, according to him, diminishes the proportion of albumen, while it does not cause it altogether to disappear, which is equally efficacious in diminishing the œdema and headache, and which in his hands has often prevented the manifestation of attacks which appeared imminent. The curative treatment is partly medical, partly operative. Some years ago the principle was established, that, in the case of puerperal convulsions, it was above all necessary to empty the uterus. In consequence of this, a series of manual and instrumental interventions were recommended, which M. Depaul characterizes as bad. At present, the Professor considers himself authorized by experience in laying down another principle, and in saying that puerperal convulsions must be treated like an acute disease, occurring suddenly under certain conditions, having its peculiar features and course, and which, after ten, twelve, twenty, thirty attacks, terminates in cure or by death, no matter what is done to hasten or provoke the action of the uterus. It is evident that we cannot here think of operations, the object of which is to induce premature labour, since these operations demand an amount of time which generally exceeds the duration of the disease.

Whatever be the cause and the nature of puerperal convulsions, it is well to study what appears, at least, to be the mode of action of the means employed against the disease. These means, without including bloodletting and anæsthetics, are numerous. But sinapisms give no good result, blisters are too slow in their action in cases of such urgency, ice, compression of the carotids, diuretics, are not, in M. Depaul's opinion, to be depended upon. Professor Depaul places bloodletting in the first rank among the means of treating eclampsia. His only complaint is, that those who employ this method, do so too sparingly. The advice generally given is to draw ten or twelve ounces of blood, and then to apply leeches behind the ears. But this is not sufficient. Bloodletting, unless copious, does little good. M. Depaul begins with a bleeding of sixteen ounces. This first bleeding generally diminishes the frequency of the attacks. If at the end of half an hour the attacks become more frequent, a second bleeding of sixteen ounces is practised; leeches are then applied behind the ears. This treatment was lately employed in the case of a patient at the Clinique. The two bleedings and the leeches did not weaken her, but they caused the complete disappearance of the convulsions; accordingly no farther bleeding was practised. If, on the contrary, the attacks had recurred, M. Depaul would not have hesitated to prescribe a third and a fourth bleeding, so as to have withdrawn two or three additional pounds of blood in the course of a few hours. During a period of twelve years this physician contented himself with practising a simple bleeding of ten or twelve ounces. He then ordered an application of leeches behind the ears, and prescribed cutaneous and intestinal derivatives; in spite of this treatment he lost more than half his patients. During the twelve years following this period, M. Depaul has acted differently, and the mortality has fallen much below fifty per cent. If, says he, I am summoned at the commencement of the attack, I affirm that, by means of bleedings practised in this way one after another (*coup sur coup*), I shall save the patient; and such has been my experience in a large number of cases.

In speaking of the therapeutical effects of anæsthesia artificially induced, M. Depaul declares that he finds a certain difficulty in sharing the favourable opinion entertained by his colleagues regarding the good effects of chloroform in puerperal convulsions. Without discussing the mode of action of chloroform, and without attempting to explain the facts observed, M. Depaul believes that in many of the cases where this agent has been employed, there had been a previous treatment by bloodletting or other means. In other circumstances chloroform has not acted favourably, and of this there was an example in the Clinique. A woman, said M. Depaul, was attacked by puerperal convulsions; a house physician who had come from the Maternité, where chloroform is in great repute, kept the patient for four hours under the influence of this anæsthetic. Upwards of three ounces of the liquid were employed, and, far from having a good effect, the result was eminently unfavourable. In place of becoming fewer and less severe, the attacks became more violent and more numerous, although the patient had been delivered for some hours, a circumstance which it is well to note with regard to the pretended influence of delivery in bringing about the cessation of the convulsions. Matters were in this state when M. Depaul arrived; he caused the patient to be bled twice within half an hour; leeches were then applied, and she was cured of the convulsions. She died at a later period, of peritonitis, a termination easily explained by the predisposition of those affected with convulsions to puerperal affections. This fact, then, is not in favour of chloroform; but as a single fact is not sufficient to justify us in accepting or rejecting a therapeutic agent, M. Depaul will repeat experiments of this kind, since, said he, chloroform had succeeded in another lying-in hospital.

In regard, now, to operative interference in cases of puerperal convulsions. During twenty years, as already stated, this treatment held the first place. It was considered necessary, at all hazards, to disembarass the gravid uterus, and the only dispute was regarding the choice of the means to be employed, according to the stage of the pregnancy. If the dilatation was complete, and the head well down, the forceps were applied. If the presentation was unnatural, turning was practised. But even in the cases where the interference of the accoucheur is possible and easy, M. Depaul maintains that the first indication is the treatment of the convulsions. This physician begins with a large bleeding, then, in order to save the infant from the death which threatens it, he terminates the delivery. But suppose another case; the os presents a dilatation equal to the diameter of a shilling. Are we, then, by means of rapid and violent means, to bring about forced delivery? This has often been done; but M. Depaul cannot too strongly recommend young accoucheurs not to imitate this mode of practice. Formerly, Professor Dubois recommended it; he incised the os at the sides and behind, and as soon as the dilatation was sufficient, which it sometimes was within five minutes, he delivered either with the forceps or with the hand. But in spite of the skill of the illustrious accoucheur, the results were deplorable, and, accordingly, M. Dubois abandoned this mode of treatment. The only circumstance which could justify this practice would be a regard for the safety of the infant, if, for example, after each attack the circulation of the fœtus seemed, on auscultation, to become feebler. The question is a delicate one; but, for his own part, M. Depaul declares unhesitatingly that in such circumstances he would prefer having recourse to bleeding. Finally, there is a third case where there is less room for hesitation,—it is that of a primipara attacked with convulsions, and in whom the cervix is still long and closed. The absurdity of having recourse to means with a view to inducing premature labour in the presence of a disease which may prove fatal in a few hours has already been alluded to. Whatever, in fact, be the means employed with this object, ten, twelve, twenty hours, sometimes more, will elapse before the result is attained. But by the end of twenty hours puerperal convulsions will have terminated either in recovery or in death.—*Journal de Médecine et de Chirurgie pratiques*, May 1863.

### ON THE USE OF MEDICATED PESSARIES IN THE TREATMENT OF UTERINE DISEASE.

### BY THOMAS HAWKES TANNER, M.D.

THE great value of a variety of local applications in the treatment of uterine disease seems to be insufficiently appreciated by the profession at large. This is somewhat strange, considering the time which has elapsed since Dr Simpson specially directed attention to their exceeding utility. Yet it is certain that a large number of practitioners know of no agents which may be directly applied to the uterus and vagina save the various kinds of caustics and injections. The latter are certainly of great benefit in many cases; but it must be allowed that, even when properly used, they are often of only temporary service, since they cannot be kept in contact with the diseased part for more than a few minutes.

The chief reason, I believe, for the non-employment of medicated pessaries has been the difficulty of so making them that they can be efficiently applied by the patient herself. When formed of certain drugs mixed into a mass with lard and wax, they are either so soft that the sufferer cannot introduce them into the vagina; or, on the contrary, they are so hard that they fail to dissolve, and are expelled in just the same condition as that in which they were introduced. Although I have now employed these agents very freely for some years, I have found very few druggists who would take the trouble so to make them that they were of any service. The difficulties just alluded to have, however, been overcome since Mr White Cooper directed attention (*Lancet*, 28th June 1862) to the utility of the butter obtained from the *Theobroma cacao nut*, from which chocolate is made, as the basis for ophthalmic ointments. This material possesses many valuable qualities, the chief being these:—It has an agreeable smell, and does not soil the fingers when handled; it does not become rancid; while, more particularly, though very firm, it has the property of becoming fluid at a low temperature. It is sometimes a little too stiff, but this fault is readily obviated by combining with it a little olive-oil or glycerine. Pessaries made with cacao butter, though they have the consistence of wax while cold, are dissolved in the course of a few minutes when introduced into the vagina.

If ophthalmic surgeons are much indebted to Mr White Cooper for directing attention to the uses of this butter, obstetric physicians are under no less an obligation. For, although it had been used in America for making ointments for some time before this gentleman wrote of its merits, yet no physicians in this country were acquainted with its value, as far as I can learn from many inquiries.

There are few uterine diseases in which the use of medicated pessaries may not advantageously form a part of the treatment. But they are more especially valuable in acute and chronic inflammation of the cervix uteri; in internal metritis, with exfoliation of the lining membrane of the uterus; in slight prolapsus or procidentia; in cancer; in all varieties of ovaritis; as well as in many affections of the female bladder. By means of them the diseased parts may be kept constantly bathed in such drugs as mercury, iodine, lead, zinc, belladonna, opium, conium, etc. They are not only most efficacious in relieving pain, but they also shield the diseased and irritable surface from contact with the vaginal walls. Owing to this latter property, they are of great utility in healing excoriations about the labia uteri; though, of course, part of the benefit derived from their use in these cases must be attributed to the absorption of the materials composing them. By their employment, moreover, the necessity for frequent examination of the morbid structures is greatly diminished.

The following formulæ are given as examples of the way in which I generally prescribe these remedies. It is only necessary to premise that rather large doses of the drugs are necessary, inasmuch as absorption through the walls of the vagina is slow and uncertain.

1. *Iodide of Lead and Belladonna Pessaries.*—℞. Plumbi iodidi, ℈ij; Extracti belladonnæ, ℈j; Butyri cacao, ʒiv; Olei olivæ, ʒj. Misce. Divide into four pessaries, and order one to be introduced into the vagina every night or every other night.

2. *Mercurial Pessaries.*—℞. Unguenti hydrargyri, ℈iv—ʒij; Butyri cacao, ʒiv; Olei olivæ, ʒj. Misce. Where there is tenderness of the cervix uteri, one scruple of extract of belladonna or two scruples of extract of conium should be added to the mass. Divide into four pessaries.

3. *Lead and Opium Pessaries.*—℞. Plumbi acetatis, ℈j; Extracti opii, gr. xij; Butyri cacao, ʒiv; Olei olivæ, ʒj. Misce. Divide into four pessaries. Order one to be used every other night.

4. *Zinc and Belladonna Pessaries.*—℞. Zinci oxydi, ʒj; Extracti belladonnæ, gr. xij—℈j; Butyri cacao, ʒiv; Olei olivæ, ʒj. Misce. Divide into four pessaries. One to be used every night.

5. *Iodide of Potassium and Conium Pessaries.*—℞. Potassii iodidi, ʒj; Extracti conii, ℈iv; Butyri cacao, ʒiv; Glycerinii puri, ʒj. Misce. Divide into four pessaries, and direct one to be used every night.

6. *Tannin and Catechu Pessaries.*—℞. Tanninæ, ℈ij; Pulveris catechu, ʒj; Butyri cacao, ʒiv; Olei olivæ, ʒj. Misce. Divide into four pessaries, and order one to be used every other night.

In some diseases of the uterine cavity, attended with copious muco-purulent discharges or with hæmorrhage, the greatest benefit may be obtained from the local use of astringents. Once or twice a-week a pessary made of tannin and the cacao butter, about two and a half inches long, and of the size of an ordinary stick of nitrate of silver, may be introduced up the canal of the uterus, and left there. It soon dissolves, and thus coats the whole seat of the discharge with the medicament, the coating remaining attached for many hours. Such a pessary, weighing about thirty-six grains, will usually contain from twelve to twenty grains of tannin. Of course, other substances—as alum, sulphate of zinc, dried sulphate of iron, etc.—may be employed in the same manner, if their use be indicated. This plan of treatment has none of the objections which apply to throwing fluids into the cavity of the uterus, a proceeding that is certainly not unattended with danger. There is no necessity, moreover, for dilating the cervix with sponge-tents prior to the introduction of the pessary; inasmuch as, when sanguineous or purulent discharges have long been present, the os and cervical canal will always be found sufficiently patulous to permit of the introduction of these astringent rods.—*Transactions of the Obstetrical Society of London for* 1862.

# Part Fourth.

## MEDICAL NEWS.

### MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLII.—MEETING VIII.

6*th May* 1863.—JAMES SPENCE, Esq., *President*, in the Chair.

#### I. ENLARGEMENT OF THE SPLEEN.

*Dr Warburton Begbie* exhibited a greatly enlarged spleen from a patient who had suffered from leukæmia. Two years ago he had brought before the Society the patient who was the subject of it, and had shown microscopical demonstrations of his blood in which there was a very great excess of white corpuscles. The patient died a few weeks ago, the immediate cause of death having been a large carbuncle. Up to that time he had remained in much the same state as he had been in when brought before the Society, except that he had become gradually weaker. On post-mortem examination the spleen weighed close upon eleven pounds, the greatest weight which, so far as Dr Begbie knew, the organ had been known to attain in such a case. No doubt the spleen was sometimes found heavier after intermittent fever, but Dr Begbie knew of no case of leukæmia in which the weight had been so great as in this instance. An interesting point was that, during life, hypexanthine had been found in the urine; it had not yet been determined how much of it there was in the spleen, but Dr Begbie trusted to be able to do so.

#### II. URINARY CALCULI REMOVED BY LITHOTOMY.

*Dr P. H. Watson* showed two urinary calculi which he had recently removed by lithotomy, and gave the particulars of the cases. (An account of the cases will be found at page 1105 of the present number of this Journal.)

### III. ON THE RELATIVE WEIGHT OF THE VISCERA ON THE TWO SIDES OF THE BODY.

*Dr John Struthers* read a paper on this subject, which will be found at page 1086 of this number of the Journal.

### IV. ARABIAN LITHOTOMY.

*Dr Handyside* read the following extract of a letter from Mr P. K. Vartan, L.R.C.S. Ed., settled in practice at Nazareth:—"Some time since I heard of an Arab who was said to be a clever lithotomist: none like him, the only man who knew the art, or who could detect the presence of a calculus! Though anxious to see one who doubtless had himself propagated such an absurd notion among this people, it was only lately that I had that opportunity. I heard some weeks ago that this Arab had under his charge a man suffering from *stone*, who had endured this agony for upwards of six years, and that, after many native doctors had failed, this Arab at last had succeeded in discovering the existence of the disease. The patient had come to him from a distance, and had been cut by him four days previously; but, after manipulating about an hour, and of course tormenting the poor creature to the highest degree, he could not succeed in removing the stone. On the third day after, he had repeated the process during the same length of time, but without success. The day following that, the wife of the poor sufferer came to my house, weeping, and prayed me to go and anæsthetize her husband on the next day, when she expected the Arab would return and resume his endeavours to extract the stone. Had I been possessed of a pair of lithotomy forceps I would have gone and extracted the stone, rather than be the mere agent to administer chloroform whilst a rude Arab was ploughing with his fingers in this poor man's body. As it was, however, I promised, for the sake of humanity, to do as the poor woman wished.

"Next day, accordingly, I went to the house to meet the Arab; and, finding the patient a fit subject for the administration of chloroform, I awaited the arrival of the operator, examining meanwhile, the case of instruments that he had left behind him at his previous visit. These were:—a pair of curved scissors; a pair of small, straight bone-forceps; a pair of small, straight bone-forceps, one blade broken; a wretched pair of tooth-forceps; a pair of a smith's or tinker's forceps; an instrument something like a compass; two old lancets, fixed at the joints by thread; a piece of wire; an old pair of dissecting forceps; a small broken file; a broad lancet; a straight grooved director; a useless piece of iron. The lithotomist came at last; and to my interrogation he answered that he would finish his work at this time in a quarter of an hour. He commenced, but could not finish in one hour. I began then to withdraw the chloroform; but the wife of the poor patient entreated me to continue it by little and little till nearly two hours had elapsed, when I could no longer, with propriety, listen to her entreaties to continue it. The operator was also obliged to discontinue his efforts. No wonder that he could not extract the stone, for it was very large, and the man had nothing but his fingers to work with, and none of the instruments that you use in the operation of lithotomy.

"In examining a case of stone his sound is his right forefinger, which he introduces into the rectum to feel if there is any hard movable body in the bladder. To facilitate this procedure he presses the hypogastric region of the patient with his left fist, as hard as he can. After he is sure there *is* a stone he gets four or five men to his assistance. He cuts into the "raphé perinei" with one of the old lancets above mentioned, bit by bit, till he opens the bladder; then he introduces the forefinger of one hand into the rectum (whilst a robust man is pressing the hypogastric region with his two fists very hard) and brings the stone to the margin of the wound, and rolls it out, if it is small enough; but in the above case it was too large. In spite of three unsuccessful trials, the stupid fellow had told the patient and his friends that after seven days (after the third trial) it would fall out by itself. I thought that the poor man would surely die before the seven days, if the stone remained in him; but if it were removed, perhaps he might live. I therefore, the same night, *made* a pair of forceps to extract the stone on the following day, and resolved to attend him, and so to give him a chance. But the wife had a strong faith that the stone would after seven days fall out by itself, and she declined to permit me to interfere. In vain I endeavoured to persuade her. The next day she came to ask me to do whatever I thought best; but my services were useless then, for the man was sinking, and the same evening he died. As soon as I heard that he was dead I went to the house, and with some difficulty got permission to extract the stone, which in two minutes' time I did, though the body was rigid. The people were surprised that I succeeded in removing, in such a short time, what the Arab could not remove after three long trials. They were now convinced that the doctors in England are not ignorant of this art, but are rather more clever than this famous Arab. The measurements of the stone are 3 inches in length, 2 inches in breadth, 1¾ inch thick. The weight of it is about 4 oz. Not having reagents, I do not know of what it consists. I should like to send it to you, if you think it may be of use to you, or to the museum of the College of Surgeons.

"As regards the after-treatment, the Arab proceeds in the following empirical manner:—As soon as he removes the stone (if he succeeds) he applies to the wound a young pigeon, cutting its head and legs off, opening its body, and laying its warm viscera next the wound. After about two hours he applies an electuary of salt, honey, and two astringent substances, twice daily, for about three days. After that he makes a second application of the electuary, which he leaves till the wound heals. He forsakes his patient, indeed, after the second day; and whether he lives or dies does not concern him at all, —this operator thinking that whenever the stone is removed his responsibility is at an end."

### V. PROBE-POINTED STEEL CATHETER.

*Dr P. H. Watson* showed a probe catheter, and stated the advantages which it appeared to possess over the ordinary instrument. (A description of the instrument will be given in the next number of this Journal.)

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## PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

### SESSION XXII.—MEETING III.

*January* 14, 1863.—Dr PATTISON, *President*, in the Chair.

### I. ON TWO PECULIARITIES OCCURRING FREQUENTLY IN THE OBSTETRICAL HISTORY OF WOMEN WHO HAVE BORN IDIOTS.

*Dr Arthur Mitchell* read a paper on the above subject, and began by stating that, while investigating the obstetrical history of women who had given birth to idiots, certain peculiarities were found to occur with such frequency as to attract attention, and give rise to the feeling that there might *possibly* be some connexion between them and the idiocy of the offspring, *either directly or through a common cause.* To several of these he had already directed attention; and it was his present object to point out two, which he thought deserving of consideration.

*First*, He had found that many idiots were born within twelve months after a miscarriage or still-birth.

He stated that his experience led him to think that, in so far as the offspring is concerned, it is a misfortune when conception takes place two, three, or four months after miscarriage, and the risk of misfortune is increased when the miscarriage has been complicated with hæmorrhage, caused by accident or disease, or followed by fever. Impregnation soon after a still-birth appears to be equally injurious. It would seem, indeed, that a considerable interval of repose is necessary to allow the uterus to regain that condition which fits it again to accomplish its part in reproduction healthfully and vigorously.

Dr Mitchell then gave the particulars of several cases illustrating this point; adding that he did not mean it to be supposed for a moment that, in all or in the majority of the instances in which the patient had been born soon after a miscarriage or still-birth, he attributed the idiocy to that as the cause. All he thought was that conception soon after a miscarriage or still-birth had a *tendency* to produce defect of some sort (and occasionally idiocy) in the offspring. Even this opinion he stated cautiously. That in the mother which leads to the miscarriage may lead also to the idiocy, and the only *connexion* may be *one through a common cause.* He thought, however, that it would be found that among children who were the fruit of such conceptions a larger proportion of *defects* would occur than among children generally. He regretted that the subject was one which he could not investigate statistically, having no data of any kind for comparison; but, notwithstanding this, he had been induced to communicate to the Society his experience and views on the subject. This remark applied equally to the second peculiarity, to which he then directed attention.

*Second*, It frequently happens that, between the birth of the idiot and that of the child which precedes or follows, an interval occurs which is much longer than usual, or that after the birth of the idiot permanent sterility appears.

This may be regarded as in direct opposition to the former peculiarity; but if strictly examined it is not so in any sense that might not be true of the statement that over-feeding and under-feeding produce gout, or produce disease.

When this abnormal interval occurs before the birth of the idiot, he thought it might point to some want of health in the reproductive organs, of which the defective child might be regarded as another expression. Again, when the idiot is born eighteen or twenty-four months after the preceding child, but when for six or seven years thereafter no impregnation occurs, he thought there was reason to suspect that the imperfection in reproductive power which showed itself in the idiot had merely another and fuller expression in the subsequent barrenness. And so also when permanent sterility follows. In many cases indications of barrenness preceded the birth of the idiot and become permanent thereafter.

Dr Mitchell then detailed a number of cases illustrating this point, and concluded by stating that this peculiarity in the obstetrical history of the mothers of idiots, or in the natural history of idiocy, had come before him so frequently as to arrest his attention. He had found it without seeking it, and he had been forced to the conclusion that the frequency was exceptionally great; but this it was not in his power to prove. He had to content himself, therefore, with drawing attention to the fact of its frequent occurrence, and stating the conclusion to which he had himself insensibly come.

*Dr Simpson* said that, while he admired the former papers with which Dr Mitchell had favoured the Society, he was obliged to confess that he entertained considerable doubts as to some of the conclusions to which he (Dr Mitchell) had arrived in his present communication. For instance, Dr Mitchell had stated that children born soon after a miscarriage, and those born a considerable time afterwards, were very frequently idiotic,—statements which appeared to him (Dr Simpson) to be contradictory, and to require more extensive data than yet existed before they could be accepted. There were few mothers who went through life without having one or more miscarriages, so that if Dr Mitchell were correct, we should expect to find one or more idiots in almost every family. Out of 454 idiots, Dr Mitchell mentioned 8 who were born soon after a miscarriage, and from which circumstance he referred the idiocy in these cases. Now he (Dr Simpson) had no doubt that out of 454 women living in this street (Thistle Street) few or none would be found to have given birth to idiots. He (Dr Simpson) had no doubt that anything which weakens the vital powers of the uterus would tend to affect the vital acts of development within it; yet we require to proceed with caution in our endeavour to lay down the causes of so important a disease as idiocy or insanity.

*Dr Mitchell* stated that he did not regard the opinions he had formed as *proved*, but as *probably correct.* He had, on looking over his cases, observed that these peculiarities occurred frequently, and he had endeavoured to determine if any relation existed between the phenomena,—that is, between the defect in the offspring and the peculiarities in question. That such a relation does sometimes exist, his investigations led him to believe. With reference to the first, it might be the relation of cause and effect; but as regards the second, it could scarcely be considered as holding that position. It would be difficult to subject such a belief to rigid statistical proof, and he had not the material for doing so. Many opinions in medical science rest on the same kind of evidence. The cases he had given were merely illustrative cases, and were not intended to represent the whole number, or to be used in any way as a proportion. In his paper he had not even stated the number of cases from the examination of which these illustrations had been drawn. He thought that further investigation would show that impregnation soon after a miscarriage or still-birth was a disadvantage to the offspring, and he thought it would also be found that temporary sterility often arose from causes which might not be altogether absent when conception occurred, and which might give defect in the product. He could not see that the two statements are in any real opposition.

In answer to a question from the President, Dr Mitchell said that deafness, squinting, etc., were very frequent among the relatives of idiots; and that idiocy was more frequent in first children, and in boys than in girls.

### II. ON DIFFICULT PARTURITION AS A CAUSE OF IDIOCY.

*Dr Arthur Mitchell* read a paper supplementary to one read by him last year on this subject. He began by pointing out that in his first paper on this subject he had given his reasons for concluding that the act of birth must be regarded as one of the causes of idiocy,—in other words, that a certain proportion of the idiocy of this country is due to injuries sustained by the child in the process of parturition. He showed that he had drawn attention to the strength of the *à priori* argument for such a conclusion, and had then given a summary of all the facts obtained by him which went to prove or disprove the conclusion. He pointed out that there were no corresponding facts regarding the general community with which minute comparisons could be instituted, and that it would be difficult to obtain these; but that they were not so necessary as they would have been if the facts he adduced had been less strong and decided.

In his first paper he had stated that 22 out of 494 idiots had been delivered by forceps. He drew attention to this statement, which he thought had been misunderstood. It appeared to be thought that he had regarded the idiocy in all these 22 cases as referable to injuries inflicted directly by the forceps, and inferentially that instrumental assistance had been unwisely resorted to in these cases, and that an argument had been supplied against the use of the forceps in midwifery. He was of opinion that this feeling had been strengthened by his having stated that in 9 of the 22 cases the use of the forceps was attested by cicatrices. He stated that he neither expressed nor held such opinions. He was collecting all such facts in the history of idiots as would help us to determine whether the act of parturition might not occasionally injure the child so as to cause idiocy, and whether the risk of this injury was not increased in proportion as the parturition was anomalous or difficult; and it would have been a grave omission had he not ascertained and stated the number of times in which the labour had been so difficult or abnormal as to require the forceps—*of which difficulty*, taking the practice of the whole country into account, *this is one evidence*, *the cicatrices being another*. He thought all would admit that mechanical injury, serious enough to lead to idiocy, was sometimes inflicted by the blades of the forceps; but in these 22 cases there was nothing to show that the pressure of the blades of the forceps, and *not the prolonged pressure in the passages*, had produced the injury. He held it as possible, indeed, that the opportune use of the forceps might save the child from such injury. But, further, neither the forceps nor pressure in the passages produced idiocy in all the 22 cases. This might have been assumed, and the histories of several of the cases showed it to be so. But they could not be excluded on this account. The statement exhibited the frequency of forceps deliveries in a given number of idiots, and *every case* ha to be included, so that we might judge whether the frequency of such deliveries was greater in the class of idiots than in the general community.

Dr Mitchell enlarged on this point, and made some important remarks as to the mode of conducting such an investigation as that into the causes of idiocy, and he concluded by giving the details of several cases in which idiocy appeared to be very closely linked to difficult parturition as its cause.

*Dr Keiller* said that, when Dr Mitchell read his paper last year, he mentioned the case of a child which he delivered by the forceps, and which, he was afraid, would become an idiot. The child, six or seven months after birth, was attacked with convulsions, and died soon after. He was at that time inclined to attribute the convulsions and threatened idiocy to the application of the forceps; but, on further consideration, he thought that the prolonged labour was to blame, and that if the forceps had been applied earlier a more favourable issue might have been attained.

*Dr Simpson* observed that the forceps do not always produce cerebral injuries, and that prolonged labour was perhaps more injurious both to child and mother. Some years ago a woman was shown to the Society, on whom the usually fatal operation of embryotomy was performed by Mungo Park the celebrated traveller. Fortunately before he (Mungo Park) had had time to proceed far with the operation, labour pains came on and expelled a well-formed, healthy female child.

The *Secretary* read the following letter from Dr Hamilton, dated Falkirk, 13th January 1863; but remarked that had Dr Hamilton been able to favour the Society with his presence, he would have found his observations to have been anticipated by Dr Mitchell:—

"My dear Sir,—I notice from the circular of the Obstetric Society, sent me to-day, that Dr Mitchell is to bring the subject of idiocy and its connexion with parturition again before the Society, and I regret very much that I shall not be able to be present. I should have liked much to mention an interesting case bearing upon this subject, in which the father has two brothers and a sister idiots, and the mother has come through eight of the most difficult labours in which I have known the children to survive, and yet none of the progeny has been idiotic, or in the least wanting in intelligence. They were all delivered with the forceps, and one I recollect had the largest indentation produced by pressure on the sacrum I recollect having met with. This child died about four years of age, of bowel complaint, and had for a year or two, at least, slight weakness on one side of the body. I should like also to have mentioned the fact that, since I read the discussion on Dr Mitchell's paper in the Edinburgh Medical Journal for October 1862, I have been trying to recollect any idiotic child born under my care, and have been unable to do so. Of course this is speaking very roughly, but still their numbers, if there have been any, must have been very small, else I should certainly have recollected some of them. This remark is perhaps of some importance, when it is recollected that I have used the forceps, as I stated last year to the Society, upwards of 200 times, especially as some of the gentlemen, in the discussion alluded to, seemed to consider the application of the forceps, and not the difficult parturition, as the cause of idiocy. Such an opinion seems to me very like the exploded one, that because statistics showed a mortality to the child of 1 in 4 or 5 where the forceps had been used, the forceps were the cause of that mortality,—the real fact being, that, in a vast majority of such cases, the children were dead before the forceps were used. If it can be shown, as I think there is no difficulty in doing, that the judicious application of the forceps enormously decreases the *infantile mortality*, then I see nothing which does not equally prove that their use ought proportionately to diminish the number of idiotic children, assuming that difficult labour is a cause of idiocy. In all the cases I have delivered with the forceps, I never saw any one in which they, *per se*, had inflicted any but slight scalp injuries on the child; and where any symptoms indicating compression of the brain have occurred, my opinion has almost invariably been, that these were due to the difficulty of the labour rather than to the effects produced by the application of the forceps. I am, my dear Sir, sincerely yours, G. HAMILTON."

### III. ERUPTIVE FEVERS IN PUERPERAL PATIENTS.

*Professor Simpson* said he had been led to bring this subject under the notice of the Society at present, in consequence of having been lately called to see, in consultation, two cases of puerperal scarlatina, which had occurred in the practice of one of the most able and accomplished of our country practitioners.

CASE 1.—The first patient was a young primipara, who, at a distance from Edinburgh, was confined after a labour of no unusual difficulty, and remained well till the second day after her confinement, when the pulse rose, and she became so delirious that it was thought she was labouring under an attack of puerperal mania. When he (Prof. S.) saw her first on the third day, the delirium had subsided; but a scarlatinal eruption had broken out over the body. The eruption was of a character so indefinite, as to leave some doubt as to whether it might not be due to the rubefacient action of the turpentine stupes that had been freely employed. Next day, on returning to see the patient, he was told that the eruption had disappeared. It reappeared after a time and again disappeared; and came and went in this way for a couple of days till the patient died. She had had scarlatina some years before. This time there was slight sore-throat, but not much; and the diagnosis had been very difficult at first, and the case peculiar, in that the premonitory symptoms seemed rather to point to mania than to any kind of eruptive disease.

CASE 2.—In the second case, on the second or third day after the birth of a fourth child, the patient fevered and showed a scarlet eruption all over the face, trunk, and limbs. The tongue and throat were clean; but the pulse was so weak and quick, that he (Prof. S.) had been led to form but a gloomy prognosis. She had, however, ultimately recovered. Probably all the Fellows had seen such cases; and he would like just to make this remark, which was applicable alike to small-pox, measles, and scarlatina, that when they occurred in a pregnant female they very rarely proved fatal to her, whatever the date of pregnancy might be; or if they did not attack her till ten days or a fortnight had elapsed after her confinement, they were not specially dangerous; but whenever they appear in a woman within six or eight days after her delivery, they are always alarming and very often fatal.

CASE 3.—Rather more than a year ago, a lady who had been placed under his care, died very suddenly of measles coming on shortly after delivery. The disease had appeared in one of the children more than a fortnight before the period of her confinement was due, and he had her immediately separated from the family, who were staying about two miles out in the country, and brought into town, and no kind of direct communication was allowed to take place between the two houses. Her labour, her thirteenth, was effected on a Wednesday, without any notable peculiarity, and she continued well for some days thereafter. But on the Sunday afternoon she had a tremendous rigor, which was succeeded in the evening by a high degree of fever, and during the night by great restlessness. Copious perspiration and a free evacuation next forenoon greatly relieved her; but in the afternoon the rigor returned, succeeded by a high fever, with burning hot skin and rapid pulse. Towards morning the brow became rough and red, and it seemed for a time as if an eruption were about to break out on the face. The pulse came down a little during the forenoon; but the rigor having recurred in the third afternoon, the patient sank and died, fifty-two or fifty-three hours after the occurrence of the first shivering-fit. The post-mortem examination which was made by Dr Turner, revealed no special local lesion other than those changes which are found in the liver and other glandular organs in patients dying of rapidly destructive fevers. It would, perhaps, have been impossible to say with absolute certainty that this puerpera died of an attack of measles; but at the time of her death, he (Prof. S.) had no doubt as to the nature of her attack, and his opinion was confirmed when, some days afterwards, an eruption of measles appeared on the baby, although it had been kept away from the infected household.

The remarks he had made with regard to scarlatina and measles held good with respect to small-pox also. There were many cases on record where small-pox, having attacked a pregnant woman, had run its course without producing any very alarming symptoms in herself, though it caused the death of her intra-uterine offspring. But when it came on within a short period after delivery had been effected, it was extremely dangerous.

CASE 4.—A patient who had been delivered in the Lying-in Hospital last month, had been seized with small-pox seven days after her confinement.

There was a distinct vaccination-scar on her arm. There was not a very profuse crop of vesicles; but on the tenth or eleventh day, some petechiæ spots appeared on the chest, which multiplied in number and increased in size till some of them looked like ecchymoses; and on the sixteenth day she died.

With regard to diagnosis, he might remark that it was not always very easy at first sight to determine the nature of the case, when a puerperal woman became the subject of an eruptive fever. He once saw a case along with Dr Newbigging, where some of the symptoms looked like those of scarlatina, but no eruption whatever could be detected. Some of the children, however, afterwards became ill, and in them the scarlatina eruption was quite distinct. The case was the more remarkable that a sister of this lady, who had also been recently confined, but who was living in the country, and had no communication with the affected household except through the doctor, was attacked with scarlatina and died also. He (Prof. S.) did not agree with Dr Ramsbotham in supposing that the poison of the special epidemic fever was retained in the system of a puerperal woman some time before it broke out; he rather believed that it ran in them its usual course. It was difficult to determine the cause of the great fatality of these fevers in such patients; but it was probably due partly to the weakened condition of the system, but still more to the presence at that time in the blood of a quantity of effete material, which was ready to be thrown into a kind of septic destruction whenever the materies morbi was introduced.

Case 5.—When speaking of puerperal disease, he (Prof. S.) might perhaps be allowed to read the notes of a case of purpura, which had occurred in one of the patients recently delivered in the Lying-in Hospital. It was the first case of the kind he remembered ever to have seen; but Dr Lyell of Newburgh, who had been in town, and to whom he had mentioned this case, told him that he had seen two instances of the disease, one of them marked by this peculiarity that delivery was attended with severe post-partum hæmorrhage. The notes which he (Prof. S.) now begged to read, had been drawn out by Dr Murray, House-surgeon to the hospital:—

"M. F., æt. 24, cook, entered the Maternity Hospital on 12th December at 12 noon, in labour with her third child. She had been in labour about twelve hours previous, and on examination the os uteri was about the size of a shilling, and her pains very severe and frequent, only a minute or two being between each pain. On inquiry, it was ascertained that she was only in her seventh month of pregnancy, and that she had always been in the enjoyment of good health, having been engaged as cook in a gentleman's family, up to the time of entering the hospital. She has always had the best and most nourishing food, and has not been addicted to intemperate habits 'in drinking' at least. From extensive inquiries made at her friends, her statement has been confirmed on these points, and further, they state that, though possessing ample opportunities for observation, they never saw any spots or eruption on the skin. The patient, however, states that for the last few months her urine had been scanty and high coloured. On entering the hospital, the only thing noticeable was that her eyes were somewhat congested, and that she had a dull and stupid appearance. Nothing, however, was to be seen on her skin. Half an hour after entering the delivery-room, she complained of a sensation of intense itchiness over her whole body, which was immediately followed by the sudden appearance of an eruption resembling nettle-rash. She had also intense thirst; frequent desire to pass her urine, which was high coloured. The eyes became more congested; the pains, however, diminished in severity. A marked feature in the case was severe vomiting, which reduced her strength very much, and for which she was ordered a tablespoonful of brandy in water every hour.

"13*th December*, 4 P.M.—The membranes ruptured, when a still-born child was delivered immediately; the placenta came away in a few minutes, and the discharge was as usual. The child, from its appearance, must have been dead some time.

"*Evening*, 11 P.M.—Sent for the nurse, complaining of severe vomiting, which had continued at intervals since she left the delivery-room, about 5 P.M.; though very thirsty, could not retain any fluid on the stomach.—℞ Acidi hydrocyanici med., ♏ij.; aquæ, ℥i. *Sig.* Fiat haustus, to be taken immediately. Vomiting greatly relieved. Patient passed a very restless night, but did not complain of any pain or any other thing. Had a peculiar action of her hands, as if she felt some difficulty of breathing. Nothing, however, was noticeable in the chest. Pulse 108, quick and feeble. Nothing to be seen on the skin.

"14*th*, 9 A.M.—Patient's face has an œdematous appearance; eyelids, especially the lower, very œdematous. Complains of severe pain in the back and loins, which are tender on pressure. Conjunctiva very much injected. A dark red appearance over the whole body. Intense heat over the whole surface. Pulse 110. Respiration sighing and long drawn. Pain in the back still continues. Answers questions intelligently. Lochial discharge slightly diminished in quantity, having rather an offensive smell.—℞ Tr. aconiti, ♏xij.; aquæ, @ ℥iij. A teaspoonful every half-hour. Wet cloths (cold) to be applied to the head. Makes water frequently and in small quantity. *Vespere.*—Pulse 94. Pain in the back *excessive* (with diminished amount of urine), causing her to start every moment.—℞ Pul. Doveri, gr. vi.; Tales duo. Whole body was swollen, and foot pits slightly on pressure.

"15*th.*—Patient has been relieved of pain, but has not slept. The face, arms, legs, and indeed the whole body much swollen, pitting on pressure, and of a dark crimson appearance. A feeling of stiffness over the whole body. Conjunctiva greatly injected, almost distended with effused blood. Had a severe attack of bleeding from the nose, which came on at short intervals till her death. Tongue densely coated; pulse 120, quick and feeble. Skin intensely hot, communicates a burning sensation to the touch; has a universally dark-red appearance. Vomiting almost gone. Stools are of a dark appearance.—℞ Tr. ferri muriatis, ♏x.; Sp. ether. nit., ʒss. M. fiat haustus.

"*Afternoon*, 3 P.M.—Urine passed in greater abundance. Skin still very hot and pungent; pulse 130. Some urine was now drawn off by the catheter, when it was found to be very dark and thick. Tested by heat and nitric acid, it became denser, but did not coagulate, and on cooling, deposited a thick dark-brown sediment. Under the microscope it showed numerous blood-corpuscles and some epithelial cells. *Evening.*—Lochia entirely suppressed. No tenderness on pressure over the region of the uterus. Injected a weak solution of Condy's fluid into the uterus, and gave a hot-air bath for one and three-quarter hour; the latter produced a copious perspiration, relieving the swelling of the body and causing a disposition to sleep.

"16*th.*—Patient passed a quiet night; swelling of the arms gone down; body feels easier; pulse 120. *Small petechiæ* observable on the eyelids and cheek-bones. Ordered hot cloths to the vulva, to restore the lochial secretion if possible. Sudden supervention of pain over the uterus. Cold cloths to be applied to head, and hot cloths to vulva, uninterruptedly. While this was being done, patient's pulse suddenly fell to 90, when she felt easier and looked quieter; but if these means were intermitted, pulse rose to 120, and she again complained of uneasiness in the abdomen.

"11 P.M.—An abundant crop of petechiæ was observed on the legs. A few being intermixed with bluish-black spots on the abdomen. The patient, however, complained most of her throat, and said that she could hardly swallow. She spat a considerable quantity of blood. On examination, not much was to be seen, possibly from the difficulty of obtaining a right view of it. The upper part of the chest on both sides was resonant on percussion, but the breathing on the posterior part of the right side was somewhat bronchial.—℞ Potassæ chloratis, ℈i.; Acidi hydrochlorici, ♏xx. Fiat gargarisma. Ordered wine.

"17*th.*—Patient has passed a very restless night; lips and teeth covered with *sordes;* throat much worse. Patient speaks indistinctly; pulse 120, very feeble. Complains of a sore feeling all over her body. Skin was rubbed off the left elbow; has bled freely at the nose. Petechiæ on the face almost grown to ecchymosis.—℞ Quinæ disulphatis, gr. v.; Acidi sulphurici dil. ♏iv.; Vini Xerici, ℥ij. To be administered frequently.

"1·15 P.M.—Patient breathing short and difficult, accompanied by a crepitating sound, and occasionally by a puffing out of the cheeks, as in apoplexy. Voice almost gone; but consciousness quite retained.—Died at 3 P.M.

"The autopsy was made by Dr T. Grainger Stewart.

"*Integumentary System.*—Body well nourished. Over the abdomen we have abundant striæ gravidarum. Face generally of a sallow colour; here and there are ecchymosed spots elevated above the surface, varying in size from a pinhead to a florin. Conjunctiva intensely reddened. On the back of the thorax the cuticle is abraded and the epidermis extremely congested, there being a bag of blood below.

"*Epidermis.*—The whole surface of the body and particularly the legs, are covered with purpuric spots. In the substance of the muscles, bloody spots are also seen.

"*Viscera.*—Sanguineous effusion into pleura. Hæmorrhage into lungs. Old pleurisy of right side, with adhesions. Heart flabby, with hæmorrhagic spots on its surface. Bloody fluid in the peritoneum. In the upper half of small, and whole of large intestines, abundant bloody patches. Liver soft and brown-coloured. Some hæmorrhage had taken place into the capsule of Glisson. Spleen somewhat enlarged, soft, and full of pulp. Slight effusion on the surface of brain. Hæmorrhage into the substance and pelvis of the kidneys.

"*Uterus.*—Walls 1¼ inch thick, 9 inches long. When opened up, measured about 1 foot across, and contained a bloody decidua."

*Dr Keiller* said that he delivered, some years ago, a lady, who immediately afterwards was attacked with serious scarlatinal symptoms. She had an easy delivery. Next day she fevered, and at first there was some difficulty in making out the nature of the case; but in a few days the sister was seized with scarlet fever, thus leaving no doubt as to its nature. Contrary to what usually happens the lady recovered. He (Dr K.) thought at first that he might have been the means of communicating the disease, inasmuch as he had just seen a case of scarlet fever before delivering her; but on reflection he came to the conclusion he had not been, because the fever came much sooner than it was possible to have done if he had carried the infection.

*Dr Cochrane* alluded to the case of a lady who resided in the country, in a district free from scarlet fever, and who, on receiving a letter from a sister affected with scarlet fever, was seized with it soon afterwards.

### IV. FATAL OBSTRUCTION OF THE BOWELS FROM SCIRRHUS OF THE UTERUS AND OVARIES.

*Dr Keiller* related a case of fatal obstruction of the bowels from scirrhus of the uterus and ovaries. The case occurred under Dr Keiller's care in the Royal Infirmary, and the following are the notes of it as reported in the Hospital journal:—

"C. G., æt. 38, admitted into Ward XIV. on 9th September 1857.

"*History.*—Since the birth of her last child, five years ago, she has had a tendency to prolapsus uteri, with severe pain in the loins and over the pubes. Since then these pains have never been entirely absent; but she enjoyed comparatively good health till within the last five months, during which time her appetite has been capricious. She has lost flesh, and has been subject to frequent constipation and vomiting. For some years she has had troublesome vaginal discharge.

"When examined on 10th September, the thoracic organs presented no sign of disease, but the abdomen was tympanitic and painful on pressure. On examining per vaginam, the true nature of the case was ascertained. From within an inch of the external orifice of the vagina the genital organs seemed to be one compacted mass of semi-cartilaginous scirrhous disease. Examined per rectum, this mass was equally distinct, and evidently constricted the calibre of the bowel. There is at present little discharge. Urine, acid; sp. gr. 1023; amber-coloured; no albumen; mucous deposit. The urine was subsequently examined at frequent intervals, and the above characters continued almost unvaried, with the single exception of the sp. gr., which was once so low as 1012.

"As she has had no stool for four days,—℞ Pulv. jalapæ, ℈j.; calomel, gr.v. Statim sumend.

"11*th September.*—The purge administered last night has had no effect. Nurse is therefore instructed to administer an injection olei olivæ, ℥vj., and to repeat it in the course of six hours. *Evening.*—The injection brought away a very small amount of feculent matter, but the patient expresses herself much relieved, and the tympanitis abdominis is diminished. Same treatment to be pursued.

"13*th.*—She is better to-day, but complains of gnawing pain in the loins, uterus, and vagina. She has a daily allowance of port-wine ℥vj., and lives chiefly on beef-tea and arrow-root. She is afraid to swallow solids, as she fears 'there is little chance of their escape from her body.' ℞ Quinæ disulph. gr.j.; ext. rhei, gr.ij.; ext. hyoscyami, Q. S. ut ft. pil.; omni nocte sumenda, horâ somni. The enemata are still administered every six hours with relief to the patient. Each enema is administered by carefullly coaxing the long gum-elastic tube of the enema apparatus past the obstructing mass. The pipe was generally passed about eight inches into the bowel.

"16*th.*—The olive-oil injections being very expensive, are replaced by gruel enemata. This forenoon there was a slight passage of feculent matter, with great relief. Takes food better, and has less pain. Urine, sp. gr. 1025; loaded. Passes from 68 to 80 ounces in twenty-four hours; so that, considering its loaded condition, the renal is probably vicarious to the alvine secretion. The uniform swelling of the abdomen has disappeared, and there is now only a large oval tumour extending from the left hypochondrium downwards to the left iliac region.

"24*th.*—For three days she has had no stool, although the gruel injections have been uniformly administered twice a-day, and at each administration the patient has felt relief. The last gruel enema had added to it, at Professor MacDonald's desire, olei crotonis tig., ♏iij., but without result. The abdomen is very tympanitic, tense, and painful. There seems also to be some effusion into the cavity of the peritoneum. The tongue is deeply furred. Takes only port-wine ℥vj. daily, a little tea, and draughts of milk and lime-water. The walls of the abdomen to be gently rubbed with—℞ Tr. aconiti (Flemingi) ʒss.; ext. belladonnæ, ʒij.; linim. camph. co., ℥ij. Ft. linimentum, more dieto utensum.

"3*d October.*—The gruel injections are still administered twice a-day, but without having, save in one instance, produced any feculent stool. The patient is distressed with constant retching, and occasional vomiting of food. Once the vomiting seemed feculent, but more accurate examination proved it to be a mixture of port-wine, curdled milk, and other ingesta. Copious acrid uterine discharge continually wells out from between the vulva. This discharge has the offensive odour so peculiar to these cases. To have castor-oil added to the gruel at each injection, and, in addition,—℞ Olei ricini, ℥ss.; olei terebinth, ℥ss. Statim sumend.

"7*th.*—All endeavours to give the poor patient relief by stool have been fruitless. There is considerable effusion into the abdomen. The skin over the abdominal parietes is very tense, and pressure causes much pain. The tongue is of a dark logwood hue, with deep fissures in the centre, and heavy white fur on either side. The teeth are covered with sordes, and she is evidently near her end. ℞ Spir. amm. aromat. ʒj., pro re natâ. The exceedingly tense condition of the walls of the abdomen, caused, doubtless, by the supperaddition of a very large quantity of gas to the long-retained feculent contents of the intestines, gave rise to the question of puncturing the intestines,—a proceeding said to be not uncommon in some of the French hospitals. This was, however, thought inexpedient; and, instead, it was attempted to introduce a long flexible tube far up the intestine, so as, if possible, to permit the escape of the flatus through this channel. The attempt was made this morning, but unsuccessfully. It was found impossible, by any means tried, to coax the flexible tube up the intestine to a greater distance than twelve inches.

"8*th.*—Died by syncope at nine o'clock this morning. During the last four days of her life, pain was alleviated by ♏xxx. sol. mur. morph. administered twice a-day. For this medicine the patient expressed extreme thankfulness.

"*Sectio Cadaveris.—9th October*, 2 P.M. (from Dr Haldane's report.)—*External Appearances.*—Great emaciation; abdomen much distended, tympanitic. As soon as the abdominal wall was perforated, there was a great escape of fetid gas from the cavity of the peritoneum.

"The *thoracic* viscera were pushed upwards and compressed by the distended condition of the abdomen, but were otherwise quite natural.

"The heart weighed seven ounces.

"*Abdomen.*—The large intestine was so enormously distended as to resemble that of a horse. The small intestine was also distended, but to a lesser degree. The whole surface of the peritoneum was coated with soft recent lymph, and the coils of small intestine were glued together by it. About half a pint of a purulent-looking fluid had accumulated in the dependent part of the abdomen. The pelvis was filled by a hard firm mass, and the rectum was firmly compressed by this mass against the brim of the pelvis.

"On careful examination, a minute perforation was found close to the angle of union of the transverse and descending colon.

"The pelvic viscera were removed in mass, not, however, without difficulty, owing to the generally indurated condition of the cellular tissue. The uterus was smooth externally, and somewhat enlarged. The os uteri and cervix were almost entirely destroyed by cancerous ulceration; when the body of the organ was cut into, the length was found to be 3½ inches, the thickness fully 2 inches. The whole of the body and fundus of the uterus were converted into a mass of cancer, having a greyish-white, somewhat opaque appearance on section, and exuding on pressure a pretty abundant creamy juice. Examined microscopically, this juice was found to contain numerous characteristic cancer-cells, compound granular corpuscles, and granular matter. There was no softening or ulceration save at the cervix. The upper part of the vagina was cancerous and ulcerated. The ovaries were closely adherent to both sides of the uterus: their normal structure was quite gone; each was nearly as large as an apple (but more flattened), and consisted of cancerous matter and cysts.

"The peritoneum reflected from the bladder to the uterus was thickened, and studded with cancerous masses.

"The cellular tissue between the bladder and uterus was dense, greyish-white, and cancerous.

"On opening the urinary bladder, its posterior wall had an irregular lobulated appearance, from the presence of rounded cancerous masses, which projected forwards. The mucous membrane over them was entire.

"The cellular tissue all around the uterus, as well as the pelvic lymphatic glands and the lower lumbar glands, were in a cancerous condition. The rectum was closely applied to the uterus, and immediately connected to it by cancerous cellular tissue. It was found impossible to pass the finger along the rectum either from above or from the anus, owing to the existence of tight strictures. When the rectum was laid open, its walls were found at various places in a cancerous state. Two inches and a half above the anus the walls of the gut were much thickened, cancerous, ulcerated, and there was a tight stricture. The gut extended upwards, and then was twisted twice upon itself; at the upper twist a sort of pouch was left, which contained a hard mass of feculent matter. There was another stricture of the gut just where the rectum was adherent to the upper surface of the uterus and the left ovary, and immediately above this the gut expanded in a funnel-shaped manner. Above this stricture the whole of the large intestine was immensely dilated, and when cut into, was found to contain air and feculent matter. The latter was semi-fluid, with numerous small hardened masses. On examining the mucous membrane, it was found to be extensively ulcerated. The portion affected was about the half of the transverse and the commencement of the descending colon. The ulçerations were of a perfectly simple character, there being no deposit of any kind in the mucous or other coats of the gut. The ulcerations varied in size from a pin's head to a shilling, were generally of a rounded form, had clean edges, and their bases were red and vascular. In general the muscular coat was clearly exposed. In addition to these rounded ulcers were others of an irregular shape, formed apparently by the coalescence of several. The bases of these had generally a sloughy appearance, and it was one of these which had caused the perforation already described. These ulcers were mostly situated at the part of the gut corresponding to the insertion of the transverse mesocolon. In addition to these there were several longitudinal fissures which had evidently been caused by rupture of the mucous membrane, due to the unusual distention of the bowel. The mucous membrane above this, as well as of the cœcum, was a little congested, but natural. The small intestines and mesenteric glands were healthy.

"The stomach was rather contracted. It, as well as the œsophagus, contained a deep yellow-coloured pultaceous matter, but without feculent odour.

"The liver was natural. The gall-bladder contained a single gall-stone, consisting of bile-pigment.

"The kidneys, pancreas, and spleen were natural.

"No other lesions found."

Dr Keiller showed a drawing, exhibiting the peculiar appearance presented by the inner surface of the enormously distended bowel.

## ROYAL INFIRMARY OF EDINBURGH.

Dr John Struthers has resigned the office of Acting Surgeon to the Royal Infirmary, and has been succeeded by Dr P. H. Watson. Mr A. M. Edwards has been appointed Assistant-Surgeon in room of Dr Watson.

## PUBLICATIONS RECEIVED.

Barker,—Malaria and Miasmata. By T. H. Barker, M.D., etc. London, 1863.

Barwell,—Cure of Clubfoot without Cutting Tendons, etc. By Richard Barwell, F.R.C.S. London, 1863.

Folsom,—Senses of Smell and Taste. Boylston Prize. By Norton Folsom. Boston, U.S., 1863.

Graves,—Studies in Physiology and Medicine, by the late Robert James Graves, F.R.S. Edited by Professor William Stokes. London, 1863.

Haughton,—Outlines of a New Theory of Muscular Action. By the Rev. Sam. Haughton, M.D., etc. London, 1863.

Laurence,—Progress of Ophthalmic Surgery. By J. Z. Laurence, M.B. London, 1863.

Leared,—Causes and Treatment of Imperfect Digestion. By A. Leared, M.D. Third Edition. London, 1863.

Lunacy,—Fifth Annual Report of Commissioners in. Edinburgh, 1863.

Nottingham,—Seventh Ann. Report of Un. Lun. Asyl. for the Co. and Burgh of. 1862.

Obstetrical Society of London, Transactions of the, for 1862. London, 1863.

Squire,—Pharmacopœias of the London Hospitals. By Peter Squire, F.L.S., etc. London, 1863.

Thompson,—Practical Lithotomy and Lithotrity. By Henry Thompson, F.R.C.S. London, 1863.

Vogler,—Diagnostik und Behandlung der chronischen Laryngitis. Von Dr H. Vogler. Berlin, 1863.

Waters,—Emphysema of the Lungs. By A. T. H. Waters, M.D., etc. London, 1862.

Wood,—Rupture, Inguinal, Crural, and Umbilical. By John Wood, F.R.C.S. Eng. London, 1863.

## PERIODICALS RECEIVED.

Births, Deaths, and Marriages, Monthly and Quarterly Returns of,—April 1863.

Boston Medical and Surgical Journal,—April 2, 9, 15, 23, 1863.

British Medical Journal,—May 2, 9, 16, 23. London, 1863.

Bulletin Générale de Thérapeutique,—Liv. 3-8. Paris, 1863.

Dublin Medical Press,—April 29, May 6, 13, 20, 1863.

Dublin Quarterly Journal of Medical Science. May 1863.

Gazette des Hôpitaux,—Apr. 21, 23, 25, 28, 30, May 2, 5, 7, 9, 12, 14 et 16, 19. Paris, 1863.

Gazette Hebdomadaire de Médecine, etc.—May 1, 8, 15, 22. Paris, 1863.

Gazette Médicale d'Orient,—April 1863.

Gazette Médicale de Paris,—May 2, 9, 16, 23, 1863.

Health and Meteorology of Manchester for April 1863, and Committee's Report for 1862.

Journal de Médecine et de Chirurgie,—May. Paris, 1863.

Medical Times and Gazette,—May 2, 9, 16, 23. London, 1863.

Medizinische Jahrbücher,—Part I. Vienna, 1863.

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